

THE PALGRAVE COMPANION TO CAMBRIDGE ECONOMICS

Edited by Robert A. Cord



The Palgrave Companion to Cambridge Economics

Robert A. Cord

Editor

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For my wife, Doris

Introduction

These two volumes are about the economics and economists associated with Cambridge University. They are the first in a series to be published by Palgrave, the main goal of which is to examine the many and varied contributions made by important *centres* of economics. With only a very few exceptions, the focus of most history of economic thought studies, at least in terms of books,¹ has been on schools of thought. Such an approach provides valuable insights into how competing schools interact and how some come to predominate, for whatever reason and length of time, while others fall out of fashion or indeed never attain any particular notoriety. However, a key deficiency of such a modus operandi is that it often fails to address the many processes and tensions that can and do occur at the level of the individual university, the personnel of which may be fighting internal battles for supremacy at the same time as trying to establish external hegemony.

A number of challenges have been thrown up in choosing which centre should be examined first on top of the issues of deciding a common structure and content. The fact that Cambridge can lay claim to having amongst the longest national and international reputation in terms of teaching and research in economics coupled with the editor's personal connection with the University can perhaps explain why it has been chosen first. The next volume in the series will examine the London School of Economics.

This brings us to the question of structure and content. Each volume in the series will consist of two parts. The first part will contain a set of chapters which will consider the contributions made by a centre where these contributions are considered to be important, albeit subject to a mixture of personal

¹ Articles are, of course, another matter.

preference and soundings from those who know better. The second, longer part will be made up of chapters discussing the contributions of individual economists attached to a particular centre. 'Attached' is the crucial word. Some economists are easy to identify with a single institution as they may, for example, have spent their whole academic careers at it. Those who have moved from institution to institution are the more difficult cases. One way forward in these instances is to place an economist in the institution where they have carried out their most important work, although this, in its turn, carries with it the danger of disagreement over what 'their most important work' was or is perceived to be and how this perception may have changed over time. Another factor perhaps worthy of consideration is an economist's education: where such an education has been received at the knee of a master, to what extent has this influenced the subsequent work of the noted pupil, and how should this be considered when that pupil has flown the nest and settled at another institution? Questions around leadership style, discipleship, loyalty, access to publication outlets and financing also enter the frame. Given this matrix of possibilities, disagreement about who should be in which volume is inevitable. However, I hope that the outrage will not be too great given the overarching goal of the series.

Robert A. Cord

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Part I

Themes in Cambridge Economics

1

Cambridge's Contribution to the Revival of Classical Political Economy

Nuno Ornelas Martins

1 Introduction

The idea of a revival of political economy has been pervasive throughout the development of Cambridge economics. Thomas Robert Malthus, who was regarded, at least at one point, by John Maynard Keynes (1933 [1972]: 71), as 'the first of the Cambridge economists', claimed he was recovering Adam Smith's approach, against David Ricardo's development of it which had, according to Malthus, led to a distortion of Smith's perspective. However, Malthus did not establish a school of thought at Cambridge. This was done by Alfred Marshall, who established the Cambridge Faculty of Economics and Politics in 1903. Like Malthus, Marshall also thought he was recovering the essence of Smith's thought, and also of classical political economy, so much so that Thorstein Veblen called Marshall's approach 'neo-classical' economics, in order to distinguish it from the 'Austrian' approach of other marginalists like Carl Menger.

Marshall believed he was continuing classical political economy not only as developed by Smith but also as continued by Ricardo and John Stuart Mill. It may seem that Marshall was already in contradiction with Malthus, who

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wanted to recover Smith's analysis against Ricardo's alleged distortion. But in truth, what happened is that in Marshall's day, 'Ricardian' economics meant something very different from what Ricardo himself thought, so much so that we may say that during Marshall's life Malthus would have been more of a 'Ricardian' than Ricardo himself. The supply and demand analysis that Marshall developed is fully in line with Malthus, and with Malthus's early criticism of Ricardo's emphasis on cost of production as the ultimate determinant of value. Malthus, like Marshall later, focused on supply and demand forces as the ultimate determinants of value, unlike Ricardo who focused on cost of production.

However, Marshall's interpretation of Ricardo was just the standard interpretation that had been developed throughout the nineteenth century in Britain. The difference between Ricardo and the nineteenth-century 'Ricardians' was elaborated by Karl Marx, and became again clearer after Piero Sraffa provided his own interpretation of Ricardo, which showed the difference between Ricardo's study of production and the 'neo-classical' supply and demand analysis. In so doing, Sraffa interprets Ricardo in a very different way from Marshall, who in turn had interpreted Ricardo in a very different way from Malthus. Moreover, just as Marshall's approach generated an important stream of thought in Cambridge, so did Sraffa's approach, which combined with Keynes's and Michał Kalecki's work, led to the emergence of another central stream of economic thought within the Cambridge tradition, namely Post-Keynesianism. This approach gained prominence during the Cambridge controversies in the theory of capital, in which authors like Joan Robinson and Sraffa criticised the marginalist framework, developing the classical theory of value according to their own interpretation of it—see Harcourt (1972, 1976) and Cohen and Harcourt (2003, 2005).

The Marshallian neoclassical approach and the Post-Keynesian approach thus emerge as attempts at a revival of classical political economy. But these two traditions, which are the two key branches of the wider Cambridge economic tradition, are very different. In fact, both advance incompatible and opposing interpretations of classical political economy. But both aim at recovering classical political economy, each in its own way. The revival of classical political economy is thus a recurring theme in the Cambridge tradition, which influenced its two key streams of thought. In this chapter, I intend to address the Cambridge contribution to the revival of classical political economy, a contribution which was most significant not just for the revival of classical political economy but also for the development of the Cambridge economic tradition itself.

2 Classical Political Economy

Before addressing the various attempts to revive classical political economy at Cambridge, we must first understand what classical political economy is. The term 'classical' political economy was first used by Marx in order to distinguish the contributions of the classical economists from the contributions of the 'vulgar' economists which, according to Marx, were transforming the scientific analysis of classical political economy into a superficial analysis of commonplaces, and, worse, a mere apologetic for the existing bourgeois economy and society.

For Marx, a scientific study of the underlying causes of value must look beyond supply and demand forces, but should instead examine the underlying conditions of production (Ricardo made the same point to Malthus). Thus, for Marx the classical economists were those that looked at the process of (re)production in order to find the causes of value, determined in terms of cost of production. Vulgar economy, in turn, does not go beyond supply and demand analysis (which Marx saw as mere superficial phenomena), and takes it to be the ultimate determinant of value.

We can identify William Petty, Richard Cantillon, François Quesnay, Smith, and Ricardo as the more influential authors who fit into Marx's definition of classical political economy. Petty looked at labour and land as the ultimate causes of wealth. For him, the cost of a commodity could be defined in terms of the land and labour that is necessary to produce it. However, Petty also noted that we can find a relationship between labour and land by finding the quantity of land which is necessary to provide sustenance to a labourer for a given quantity of time. This means we can measure cost ultimately in terms of the quantity of land necessary to produce a commodity (where this quantity of land includes a part which is necessary to provide food for the labourer). Cantillon agreed with this procedure of converting labour into land, which provided an objective measure of cost in terms of the quantity of land.

Quesnay and the Physiocrats also adopt an objective conception of value, focusing on land. In the *Tableau Economique*, Quesnay provides the first systematic account of the circular process of economic reproduction. For the Physiocrats, agriculture is the only activity which generates a surplus, while other activities merely reproduce the capital they use. In the *Tableau*, the surplus is consumed by the landlords, who do not contribute to the production process. The surplus is then the part of production which is not necessary for the reproduction of the existing economic system. According to Quesnay, prosperity occurs when the surplus is used mainly in productive activities, and decadence comes when the surplus is used mainly in luxurious consumption.

Smith signals an important change in the theory of value, switching the emphasis from land to labour. For Smith, the value of a commodity is measured ultimately in terms of human labour. As Smith focuses on labour, rather than land, he sees all sectors where productive labour is performed as sectors that generate a surplus, rather than seeing agriculture as the only sector where a surplus is produced, as Quesnay did. Smith concedes that agriculture is the most productive activity, and he notes that this fact led Quesnay to the exaggerated claim that agriculture is the only productive activity, that is, the only activity that produces a surplus above what is necessary for the reproduction of the existing economic system. But Smith notes that other sectors also produce a surplus. Like Quesnay, Smith believed that the economy and society will flourish when the surplus is used in productive activities, and decadence arises when the surplus is used in luxurious (non-productive) consumption.

However, Smith's analysis of cost in terms of human labour was somewhat ambiguous, leading to opposing interpretations. The difference at stake here is between commanded labour, which is how much labour a given commodity can purchase, and embodied labour, which is how much labour was spent in the production of the commodity. Ricardo argued that value consisted of embodied labour. Malthus, in contrast, argued that Smith's measure of cost was commanded value. In so doing, Malthus claimed to be recovering Smith's approach against Ricardo's alleged distortion of it.

The fact that Smith argued both for commanded labour and embodied labour is connected to the difference between the *measurement* of value, where Smith used commanded labour, and the *cause* of value, which can be taken to be embodied labour. To understand the debate between Ricardo and Malthus, we must understand their different approaches to the theory of value, which are connected to the fact that Malthus focused on the measurement of value, while Ricardo wanted to go beyond the measurement of value, to its underlying causes, as Marx did more explicitly.

According to Ricardo, the value of the commodity tends to the cost of production, which depends upon embodied labour. Ricardo (1817) [1821] presupposes an economy where there is no full employment of labour, that is, where labour is available for further production. Ricardo argues that if demand drives the price above the cost of production, the quantity brought to the market can be increased using the labour which is available for further production. If demand drives the price below the cost of production, the quantity brought to the market can be reduced until the price reaches the cost of production.

For Ricardo, as for Smith, the cost of production includes the surplus, since it is constituted by wages, profits, and rent. Thus, the cost of production is the

natural price. The market price, which is the price that is actually observed, is permanently gravitating towards the natural price, that is, towards the cost of production, due to the activity of supply and demand. But for Smith and Ricardo, supply and demand forces merely explain the gravitation of the market price which tends towards or fluctuates around the natural price. Smith defined effectual demand as the demand of those who can pay the natural price, and are willing to do so. This means that the natural price is not determined by supply and demand. Rather, the natural price is a determinant of effective demand, since effective demand is defined in terms of the natural price itself. The natural price, in turn, is explained in terms of the cost of production.

Malthus (1820), however, argued that the cost of production of a commodity should be measured in terms of commanded labour which is, remember, how much labour can be purchased using a given commodity. Malthus notes that Smith himself had made reference to commanded labour as a measure of value. But the value that a commodity can command in a market is determined precisely by supply and demand forces. In fact, Malthus noted that the cost of production is, for Smith, constituted in terms of the sum of wages, profits, and rent required to produce one unit. Moreover, Malthus argued that each of these components of the cost of production is, in turn, explained by supply and demand forces. For Malthus, supply and demand are the ultimate determinants of value and wealth, and he argues that if we follow this line of reasoning, this must also be the case in Smith's analysis.

Ricardo, in turn, was critical of Smith's ambiguity concerning commanded labour and embodied labour, and argued that the cost of production must always be interpreted in terms of embodied labour, that is, how much labour time was actually consumed in the production of a commodity. This means that instead of looking to market exchange in order to find commanded labour, as Malthus did, Ricardo looked at the production process in order to find embodied labour.

Moreover, Ricardo did not explain wages, profits, and rent in terms of supply and demand forces. For Ricardo, wages were set in terms of the subsistence level. Here, Ricardo accepts Malthus's principle of population (which was, in fact, also present in many other previous authors, including Smith, Steuart, and Cantillon) according to which the increase in population and the ensuing competition between workers leads wages towards the subsistence level. However, for the various classical authors, the subsistence level did not denote merely the minimum threshold for bare physical survival. Rather, the subsistence level constitutes the standard of living which is customary and acceptable according to existing habits and conventions. The level of wages is thus set by institutional factors.

The surplus is defined as the difference between total production and wages, each measured in terms of embodied labour. For Smith, the surplus is the excess of what masters receive at the end of each production period over their advances (wages, tools, raw materials, and the like) at the beginning of the period of production. Ricardo develops this scheme, defining profit as the surplus value obtained on the worst land. Rent is the difference between value obtained on a given land and the value obtained on the worst land.

Ricardo did not take wages, profits, and rent to be independent factors, which can be merely added up in order to obtain the natural price. Since profits are the difference between the value of production and the value of wages, the latter set by institutional factors, then an increase in wages will decrease profits. Wages and profits are thus interdependent. Moreover, if the worst land increases its productivity, while other lands maintain their productivity, profits increase and rent decreases, since rent is the difference between the productivity of these lands (this simple example presupposes that the worst land does not cease to be the worst land in virtue of this improvement, but, if it does, the interdependence between rent and profits just becomes even more complex).

We can then see that while Malthus looks at market exchange, and the interaction between supply and demand, as the ultimate determinants of value (of commanded labour), Ricardo looks at the production process as the ultimate determinant of value (measured in terms of labour embodied in the production process). Ricardo criticised Smith's lack of clarity on these matters and developed Smith's approach in an attempt to make it a coherent one. Malthus felt that in so doing, Ricardo was moving away from Smith's thought. Thus, Malthus presents his contribution as a return to Smith, away from Ricardo's development of Smith's thought. Rightly or wrongly, Malthus believed himself to be Smith's most faithful disciple.

Malthus's contribution is the first attempt at a revival of Smith's thought (or what Malthus believed to be Smith's thought) by an influential author associated with Cambridge. If we follow Keynes's acknowledgement of Malthus as 'the first of the Cambridge economists', we can then see that the Cambridge economic tradition seen in such broader terms starts as a revival of Smith's classical analysis.

3 Vulgar Economy and the Marginal Revolution

As Anthony Waterman (1996) notes, one could go further back than Malthus as, Waterman argues, Keynes eventually did, taking into account Chapter XI of William Paley's 1785 book *The Principles of Moral and Political Philosophy*.

In this chapter, Paley addresses the connections between agriculture and trade, noting how a surplus is generated in agriculture, which sustains those who engage in trade and produce articles to be sold to those engaged in agriculture. In this sense, it is the agricultural surplus which leads to a given level of population being sustained at a given standard of living, as it was the case for the classical authors from Petty to Ricardo, and for Malthus too, despite the differences between Malthus and Ricardo on the theory of value.

The seeming differences between Malthus and Ricardo can be traced back to Smith's ambiguities, which Ricardo sought to clarify into a coherent theory where value is explained in terms of the process of production. Smith, by asserting that value comes from human labour without clarifying whether it was commanded labour or embodied labour, moved away from a strict and objective analysis of the production process which had been developed by Petty, Cantillon, and Quesnay. Ricardo focused again on the production process and on agriculture as the key to understanding the surplus in objective terms, as Petty, Cantillon, and Quesnay had done.

However, the economists who claimed to be the successors of Ricardo, often termed 'Ricardian' economists, abandoned the attempt to measure value in objective terms. Even before that, Jean-Baptiste Say had already interpreted Smith differently than Ricardo, resorting to supply and demand analysis to determine value, as Malthus did. Mill provided an important step in the consolidation of the idea that supply and demand are the ultimate determinants of value, as Malthus had argued against Ricardo. Nevertheless, Mill seemed to believe that he was following Ricardo, like many other 'Ricardian' economists throughout the nineteenth century who explained value in terms of supply and demand.

Moreover, the middle of the nineteenth century also witnessed the introduction of subjective elements into the measurement of the cost of production. Nassau William Senior argued that interest on capital is the remuneration for the abstinence of someone who decides to save and invest in order to obtain a future income, abstaining from present consumption. John Elliott Cairnes stresses that this abstinence is a sacrifice, on equal terms with the labour of the worker. For Senior and Cairnes, the cost of production is measured not only in terms of human labour but also in terms of abstinence (the 'sacrifice' of waiting).

Senior outlines four postulates from which economic laws can be deduced. The first of these postulates is that human beings attempt to obtain additional wealth with as little sacrifice as possible. The psychological notion of sacrifice thus takes centre stage in economic analysis in the middle of the nineteenth century. With the introduction of subjective elements by Senior and Cairnes

into the analysis of the cost of production, we are thus far removed from the analysis of economists from Petty to Ricardo, where costs were measured in terms of objective factors that can be observed by looking at the production process, such as the quantity of land necessary for production (stressed by Petty, Cantillon, and Quesnay) or the quantity of labour embodied in the production process (stressed by Ricardo).

When the cost of production starts to include subjective elements, it becomes impossible to measure it by looking only at the production process. Rather, we must look to market exchange to find how subjective aspects are being valued in market exchange. In this context, supply and demand start to be seen as the ultimate determinants of value and wealth. It is simply not possible to measure value in objective terms (in terms of objective quantities of land and labour) once the cost of production includes subjective elements. The analysis developed by Mill, Senior, and Cairnes is thus much more in line with Malthus's supply and demand approach, rather than with Ricardo's objective analysis of the process of production (by Mill I mean John Stuart, since his father James seems to have adopted a more objective analysis, more in line with the classical authors from Petty to Ricardo).

The analysis of Malthus, Say, Mill, Senior, and Cairnes constitutes what Marx called 'vulgar economy', that is, a superficial study of supply and demand which, unlike classical political economy, does not consider the underlying causes that are to be found in the process of production. According to Marx's distinction, 'classical political economy' is the period from Petty to Ricardo, as noted above, and 'vulgar economy' is the period from Malthus to Cairnes. In both periods, we find ambiguous figures, like Smith, who was, however, more in line with the classical analysis, and Mill, who was more in line with the vulgar analysis, despite his hesitations.

It is not only in the study of supply but also in the study of demand that subjective elements are brought into economic analysis throughout the nineteenth century. During this period, Augustin Cournot formalised supply and demand analysis through supply and demand curves. Also, authors like Jules Dupuit and Hermann Heinrich Gossen studied demand in terms of subjective preferences, introducing essential aspects of the notion of marginal utility.

However, it is only in the 1870s that value starts to be measured in terms of marginal utility, within the so-called marginal revolution undertaken by Carl Menger, Stanley Jevons, Léon Walras, and Marshall. The ideas advanced by these authors were not very different from those advocated by Dupuit and Gossen. Moreover, the differential method recommended by them—with the exception of Menger, who did not advocate the use of mathematical methods—had already been used by Cournot in order to address the theory

of value, which had a great influence on Walras and Marshall, and by Johann Heinrich von Thünen, whom Marshall acknowledged as a central influence in his own work. But it is only in the 1870s that we find a widespread acceptance of the marginalist ideas.

4 The Development of Marshall's Method and Theory

The marginal revolution was perceived as a break with classical political economy. Following Marx's distinction, however, we may question whether the break with classical political economy (as defined by Marx) in fact occurred earlier, when the 'classical' analysis of value in terms of the production process is substituted by the 'vulgar' study of supply and demand, which emerged within 'vulgar economy', and was later developed by the marginalists. This would then explain why Marshall saw no great difference between his analysis, and the analysis of the authors who came before him, like Mill. In Marshall's day, the term 'classical' was employed in order to denote the whole period from Smith to Cairnes, rather than to designate the period from Petty to Ricardo. That is, classical political economy, as defined by Marx, was not distinguished from vulgar economy. Rather, both the classical and the vulgar approach were subsumed under the category 'classical', which corresponded to what Marx called vulgar economy, since Smith and Ricardo were interpreted in a way that made them conform to 'vulgar' theory. In addition, the neglect of the contributions of Petty, Cantillon, and Quesnay—who clearly had an objective conception of value, while focusing on Smith, who had a more ambiguous conception—only contributed more to the failure to distinguish classical theory from vulgar theory.

Thus, when the term 'neo-classical' economics emerges, first employed by Veblen (1900), it is used to designate Marshall's approach, which Veblen saw as an example of the best work undertaken in continuity with the 'classical' tradition from Smith to Cairnes. Marshall (1890) himself presented his contribution as being in continuity with the tradition of Smith, Ricardo, and Mill, but one may wonder to what extent he really believed in this—Harcourt (1981: 40) notes that Joan Robinson thought that 'Marshall was foxing on this'. Furthermore, if we distinguish between 'classical' and 'vulgar' economy using Marx's criterion, we are then led to the conclusion that Marshall continues Malthus's supply and demand analysis rather than Ricardo's analysis. Just as Malthus saw himself as a restorer of what he saw as Smith's supply and demand analysis, so Marshall saw himself as continuing what he saw as the 'classical' supply and demand analysis.

Marshall was not merely an economist who happened to be at Cambridge at some point in his life. Rather, Marshall played a decisive role in the foundation of Cambridge economics as a consistent and continuing tradition which rejected the widespread use of mathematics in economics, and provided an alternative to Jevons's marginalism. But whoever we take to be the first influential economist of the Cambridge tradition, Malthus or Marshall, we can agree that both stressed continuity with what they believed to be classical political economy stemming from Smith. Malthus and Marshall believed their approach was some sort of a recovery of Smith's classical approach, against the distortions it had suffered in other contributions. Malthus's chief target here was Ricardo, as noted above, whilst Marshall's chief target was the marginalists, who stressed discontinuity with the classicals.

Where Ricardo's (1817) [1821] text seemed to contradict the supply and demand approach that Marshall developed, Marshall (1890) [1920] simply noted that this is the consequence of Ricardo's careless writing. So Marshall does not find great differences between Ricardo and Malthus, taking both to be within the supply and demand analysis that he, Marshall, developed. But Marshall's development of supply and demand analysis went much further beyond the state in which Malthus, Say, Mill, Senior, and Cairnes had left it. Marshall was an extraordinarily competent mathematician, trained in the Cambridge Mathematical Tripos, which maintained a geometrical approach to mathematics much different from the symbolic approach that was dominant on the Continent (I discuss this issue in more detail in Martins 2013: Chapter 5). Marshall's use of mathematics enabled him to address significant philosophical problems he perceived early on, with important implications for supply and demand analysis.

One philosophical problem was that of internal relations. Internal relations can be defined as relations which are constitutive of the related entities (see Tony Lawson 2003). Internal relations are a central notion within the philosophy of Georg Wilhelm Friedrich Hegel, which greatly influenced Marshall and the Cambridge atmosphere of Marshall's time. The emergence of the twentieth-century school of analytical philosophy within Cambridge, led by Bertrand Russell and G.E. Moore, was to a large extent a reply to Hegelian philosophy, against the idea of internal relations, or at least against the idea that all relations are internal. Russell thought that if everything is connected to everything else, we can never gain exact knowledge of a given component without knowing also the other components of reality which are internally related to it, and are thus constitutive of it (see Martins *ibid.*: Chapter 9). But this means that exact knowledge is impossible, since we can never grasp the whole as a whole (whole qua whole), since that would mean having full knowledge of everything. Thus, Russell advocated that relations

must be external, that is, not constitutive of the related components, which are then independent atoms. Russell's logical atomism, which also influenced the young Ludwig Wittgenstein, was thus the rejection of Hegelianism.

Marshall, however, did not reject Hegelianism. He tried instead to solve the problem of how to grasp knowledge of a given component of reality, while accepting that everything is internally related. Walras's general equilibrium analysis focused on the economic system as a whole. But Marshall wanted a method that not only took into account internal relations but would also be suitable and adequate for a practical analysis of concrete problems. Marshall's solution was the method of 'particular equilibrium analysis', or partial equilibrium analysis, as it is now called. This approach allowed for the analysis of a specific aspect of reality, rather than focusing on an abstract general equilibrium system as a whole. However, it raised the following question: How can knowledge of a part of reality be achieved if it is connected to everything else?

Marshall's solution was to assume that everything else remains constant, for a time, that is, *ceteris paribus*. Marshall found the methodological justification for this assumption in the differential calculus of Newton and Leibniz, as he writes in *Industry and Trade* (Marshall 1919: 667–668). Marshall distinguishes the direct effects of a change from the indirect effects of a change. While in a case of direct effects, A influences B directly, in the case of an indirect effect, we have A influencing C, and it is C in turn which influences B. Marshall notes that as Newton and Leibniz had shown, in the case of small changes, a very small thing of a very small thing is actually a negligible thing. If A has a small effect on C, which in turn has a small effect on B, the overall effect of A on B will be negligible. Just as the product of two infinitesimal changes can be neglected, so can the indirect effects be neglected, by virtue of *ceteris paribus*, while focusing on the direct effects only. Using this method, Marshall could then obtain supply and demand curves for a specific market, while assuming everything else remains constant. Supply and demand curves are the exogenous data used for the mutual determination of prices and quantities under partial equilibrium.

Infinitesimal calculus, which Marshall grasped early on in his mathematical studies, provided him with a solution to the philosophical problem of internal relations. Cournot's principle of continuity and von Thünen's principle of substitution, which played a major role in Marshall's supply and demand analysis, were also developed using infinitesimal calculus. By using the calculus to solve the problem of internal relations, Marshall was following two of the economists he most admired, Cournot and von Thünen. Henry Sidgwick, who Marshall called his intellectual father and mother, and who played a key role in the early development of the Cambridge economic

tradition (see Schultz's chapter on Sidgwick in this volume), also referred to the use of the calculus in his unpublished writings.¹

Of course, Marshall notes that as time goes by, the indirect effects may become very significant. Marshall's method (or actually, Newton's method and Leibniz's calculus) is the more appropriate the smaller is the time interval, which will make changes small, and indirect effects negligible. Marshall argues that supply and demand are the ultimate determinants of value in all periods, analogously to Malthus, who argued that supply and demand are the ultimate determinants of value not only for market prices but also for natural prices. But Marshall did not put as much emphasis on partial equilibrium analysis of the long period through supply and demand as, for example, his successor, Arthur Pigou, did.

5 Sraffa, Keynes, and the Post-Keynesian Revival of Classical Political Economy

The Marshallian approach soon received criticisms from within the Cambridge tradition. The most famous critiques of the Marshallian framework were made by Sraffa, who came to Cambridge after publishing his critique, and Keynes. Sraffa's criticism was much more devastating than Keynes's because it undermined not only Marshall's theory, as Keynes did, but also Marshall's method, something Keynes did not do, at least not as consistently as Sraffa did. It was Sraffa who noted that Marshall's method was developed with the problem of internal relations in mind, as we can see from Sraffa's (D3/12/11/11) unpublished manuscripts at the Wren Library, Trinity College, Cambridge University, where Sraffa discusses the passages from *Industry and Trade* mentioned above, in which Marshall explains his method. Sraffa finds Marshall's use of the method of Newton and Leibniz an intelligent, but fallacious effort.

Sraffa (D3/12/42/10, sixth proposition) argues that Newton's method is not applicable to economics, and that Marshall must have been short of arguments to use such an unfair trick. The method Marshall adopted was used by Newton and Leibniz for the study of infinitesimal changes. But Sraffa notes that in economics we do not deal with infinitesimal changes. Thus, we cannot rely upon the method of Newton and Leibniz, which presupposes them. In economics, we must consider not only direct effects but also indirect effects, which are not negligible if changes are not infinitesimal.

¹ See Add Msc 96/2, Henry Sidgwick Papers, Wren Library, Trinity College, Cambridge University.

In two famous articles, Sraffa (1925, 1926) shows that we cannot use supply and demand curves while assuming that everything else remains constant. Movements in the demand curve will lead to changes in the quantities of factors of production used. These are used also in other markets, leading to changes in the prices of the factors of production across several markets, and to changes in the supply curve of the market under analysis. We cannot assume that everything else remains constant, or that these effects are negligible, since we are not dealing with infinitesimal changes. Thus, supply and demand curves cannot be taken to be independent determinants of prices and quantities. Also, we cannot assume that one curve moves while the other curve remains constant. Lastly, changes in prices lead also to changes in the number of firms operating in the market, contributing to further changes in the supply curve, which cannot be known using only the information provided by the supply and demand curves.

What Sraffa's critique implies is that Marshall's method does not really solve the philosophical problem of internal relations, which had been perceived early on by Marshall, and by Cambridge philosophers like Russell, Moore, and the young Wittgenstein. The problem of internal relations had been addressed in Cambridge in the early twentieth century by assuming that we could look at components of reality while assuming those components to be isolated from everything else. We can see this both in Marshall's economics (and in the Cambridge economic tradition he founded) and in Russell's and Moore's philosophy (and in the Cambridge school of analytical philosophy that they founded).

Sraffa rejected Marshall's solution to this problem, and also influenced Wittgenstein's later works, where Wittgenstein abandoned his early approach to the problem. Sraffa's rejection of Marshall's partial equilibrium analysis together with Wittgenstein's abandonment of his earlier logical atomism (which was, in turn, influenced by Friedrich Ludwig Gottlob Frege and Russell) are connected to the problem of internal relations.

The solution Sraffa found for the problem of internal relations was to look at the conditions for the reproduction of the economy and society as a whole, at a given moment in time. By focusing on the whole economy, we can take into account that reality is deeply interconnected. Moreover, by focusing on a given moment in time, we can abstract from changes that lead to complex processes through time which we cannot fully grasp analytically.

However, the question arises as to which moment of time we should look at in order to understand the economy. Reality is permanently changing, and different configurations appear at different moments. Pierangelo Garegnani (1984)

suggests that we look at the normal position of the economy, which is simply an average of various positions that take place over time. Over a sufficiently long period of time, we can then have a firmer grasp of the more persistent elements that constitute the underlying conditions of reproduction of the economy and society (see, in addition, Harcourt (1981) on how this idea also appears in Marshall, Keynes, and Sraffa).

Sraffa (1960) stressed that the method he adopted was simply the old standpoint of the classical political economists like Smith and Ricardo, which had been developed by Marx. Ronald Meek (1961) quickly interpreted Sraffa's 1960 book as a revival of classical political economy. Sraffa provided a system where prices are determined by the cost of production, for a given distribution of income. Marshall's (1890) [1920] theory of value was a development of the supply and demand framework that had been advanced by Malthus in his critique of Ricardo. Sraffa's theory of value, in contrast, was a revival of Ricardo's theory of value, where prices depend upon the cost of production, and supply and demand merely explain deviations from this cost, and cannot provide a systematic account of the underlying causes of value, as Sraffa (1925, 1926) demonstrated in his critique of Marshallian supply and demand curves.

In the first footnote of *The General Theory*, Keynes (1936 [1973]: 3, fn. 1) writes that although Marx invented the term 'classical economists' to denote Ricardo and his predecessors, Keynes himself had become accustomed to using the term 'classical' to designate those he believed were developing Ricardian economics, namely Mill, Francis Ysidro Edgeworth, Marshall, and Pigou. For this reason, Keynes (*ibid.*) presents *The General Theory* as a critique of classical theory, where classical theory is understood as a project that was to be found in its most advanced form in the writings of Marshall and Pigou. We can then see that while Sraffa presented his approach as a revival of classical political economy, Keynes presents his theory as a critique of classical political economy.

This happens because Sraffa and Keynes had very different, or indeed opposing, conceptions of classical political economy in mind. Keynes, like Marshall, interprets classical political economy as being no different from what Marx called vulgar political economy. Like Marshall, Keynes also sees Ricardo as part of this project. Sraffa (D3/12/4/10), by contrast, follows Marx's distinction between classical political economy and vulgar political economy, and sees Marshall's approach as a further development of vulgar political economy, with a more sophisticated technique. Thus, Sraffa saw Malthus, rather than Ricardo, as the true precursor of Marshall's supply and demand analysis. The confusion is even greater because Keynes also believed that Malthus was the most important precursor of his own [Keynes's] theory.

For Keynes, the defining feature of classical political economy was the adoption of Say's Law, that is, the idea that supply creates its own demand. Keynes (*ibid.*: 32) notes that authors like Malthus and Marx had rejected this idea. According to Keynesian theory, we cannot presuppose that the economy will tend automatically to full employment of labour. Rather, the economy may fail to achieve full employment for an indefinite period of time.

Keynes's own contribution was much influenced by the economists with whom he met regularly to discuss his ideas, such as Richard Kahn, James Meade, Joan Robinson, Austin Robinson, and Piero Sraffa, the so-called Cambridge Circus (before the 1930s, Keynes worked mainly with Dennis Robertson). Ralph Hawtrey and Roy Harrod are also important in this connection. Michał Kalecki had developed the same ideas as Keynes, which appeared in articles written before *The General Theory* was published. Coming from a Marxist background, Kalecki developed these ideas in a way which shows more evidently how they are clearly compatible with the classical surplus theory as developed by Marx.

However, the fact that Keynes presented his theory as a critique of what he saw as the classical theory, and the fact that he followed the vulgar interpretation of classical theory, obscured important connections between his theory of employment and the classical theory of value. In fact, if we interpret the classical theory in line with Sraffa's perspective (which is in line with Marx's original distinction between classical and vulgar theory), we find that Keynes's theory is much more compatible with Ricardo's perspective than with Malthus's, contrary to what Keynes himself thought. Malthus's approach is a forerunner of the theories that Keynes is really criticising, developed by Marshall and Pigou.

This issue can be seen more clearly if we note that Ricardo's theory actually presupposes that there is no inherent tendency towards full employment, as is the case with Keynesian theory, while the marginalist supply and demand analysis that follows from the vulgar approach that stems from Malthus presupposes a case of full employment. Ricardo assumes Say's identity between supply and demand, but there is nothing in Ricardo's theory to sustain such an assumption, and the theory can do equally well without it, or in fact it becomes much more consistent without it, since Ricardo's theory of value presupposes that there is no full employment of labour.

Walras saw this point, but used it to criticise Ricardo's theory. Walras notes that, for Ricardo, the price tends to the cost of production because if demand drives the price above the cost of production, more commodities can be produced so that the price is brought back to the cost of production. The reason why more commodities can be produced, Ricardo argues, is because he

presupposes that labour is available to do so. Walras criticised this Ricardian assumption, that more commodities can be produced to respond to demand and bring the price back to the cost of production. While Ricardo presupposed an economy where typically there is not full employment of labour, that is, labour is available for further production, Walras presupposed an economy where typically there is full employment of all factors of production, including labour, and so labour is not available for further production. There will be only a redistribution of fully employed supplies of factor services between the production of different commodities, according to supply and demand. Hence, prices will be determined by supply and demand, since variations in demand cannot be offset by variations in quantity produced. Demand is thus an independent determinant of prices, explained in turn in terms of marginal utility.

Marshall and Pigou further developed this supply and demand analysis, where prices and quantities are determined mutually. Keynes's critique is targeted at the belief that supply and demand brings automatically full employment (while integrating theories of money and finance and prices with the determination of income and employment), not at what Ricardo actually wrote. Ricardo, like classical analysis in general, did not even address the determination of quantities that Keynes is focusing on. Rather, the classical authors focused on the determination of value while taking quantities as given (and, in the case of Ricardo, clearly presupposing that full employment cannot be taken for granted, as Keynes also did). Thus, the classical theory can be more readily combined with a theory that explains quantities, such as the Keynesian theory.

The contributions centred around the work of Keynes, Kalecki, and Sraffa led to the emergence of a new branch within the Cambridge economic tradition, the Cambridge Keynesian tradition (see Harcourt 2006 and Pasinetti 2007). Of course, depending on whether the emphasis is on Keynes, Kalecki, or Sraffa, we find differences within various sub-branches of the Cambridge Keynesian tradition. But the writings of the various authors of the Cambridge Keynesian tradition consistently reject the neoclassical approach of Marshall and Pigou. The Cambridge controversies in the theory of capital show us a moment where Keynesians, Kaleckians, and Sraffians stand united in the rejection of neoclassical economics, while advancing a perspective in line with Marx's interpretation of classical political economy (see Harcourt (1972, 1976) and Cohen and Harcourt (2003) on the Cambridge controversies in the theory of capital).

A central theme within the Cambridge Keynesian tradition was a study of cumulative causation processes, undertaken by Kalecki, Joan Robinson, and

Nicholas Kaldor who, like the classical economists, saw how the distribution of the surplus is an essential aspect of the circular process of reproduction and accumulation of capital.

6 The Cambridge Welfare Tradition Versus the Cambridge Keynesian Tradition

While Keynes, Kalecki, and Sraffa, together with many others, were providing the key contributions for the development of the Cambridge Keynesian tradition, Pigou continued the tradition of Sidgwick and Marshall, focusing on human well-being. Marshall defined economics as the study of mankind in the ordinary business of life while focusing on the material requisites of well-being. Like Sidgwick, Marshall acknowledged a distinction between wealth and well-being, which was developed more systematically by Pigou (1912, 1920).

Pigou's analysis of human well-being led to the development of what we may term, following Harcourt (2003), the Cambridge 'welfare' tradition (see also Harcourt's 'Foreword' in Martins (2013) and the chapter on welfare by McLure and Arthmar in this volume). Needless to say, the authors that developed each tradition, the Cambridge welfare tradition and the Cambridge Keynesian tradition, present themselves as a continuation of classical political economy, but with each possessing opposing views on what classical political economy is, as noted above.

The Cambridge welfare tradition adopted a marginalist framework in the analysis of human well-being. Thus, Sidgwick, Marshall, and Pigou noted that wealth and welfare need not coincide because different human beings possess different marginal utilities, and thus achieve different levels of well-being even if they all possess the same wealth. Pigou drew upon the utilitarian framework to argue that a more equal distribution of income and wealth leads to a higher level of social utility. The individuals who have lower income and wealth have a higher marginal utility for an additional unit of income or wealth. As such, the redistribution of income and wealth towards those who possess a lower level of income and wealth (and thus a higher marginal utility) leads to an increase in total utility within society.

One cannot help noting the striking similarities between Pigou's reasoning on marginal utility and Keynes's reasoning when studying the marginal propensity to consume (MPC). In the final chapter of *The General Theory*—which is on the social philosophy towards which *The General Theory* could lead—Keynes (1936 [1973]: 372–384) argues that the redistribution of income

towards those with lower incomes leads to a greater level of total consumption, since those with lower incomes have higher MPCs. Thus, Keynes saw a more equal distribution of income as a means to increase effective demand, as did Kalecki. Keynes's argument is formally analogous to Pigou's, but Pigou focuses on utility, and Keynes on consumption.

Pigou's argument for redistribution was soon challenged by Lionel Robbins's (1932) [1935] questioning of the possibility of interpersonal comparisons of utility. Much work on social choice theory was undertaken in order to challenge Robbins's doubts, most notably by Amartya Sen (1982), who became the key contemporary exponent of the Cambridge welfare tradition, developed also by authors such as Meade and Tony Atkinson. Sen (*ibid.*) argued that partial comparisons of well-being are possible in many cases. Sen's contribution was much stimulated by the central theme of the Cambridge welfare tradition, namely the distinction between wealth and welfare. But Sen's analysis of this distinction leads him to reject the use of utility as a measure of human well-being. Influenced by Maurice Dobb and his notion of 'rich description', Sen (1985) came to study human well-being in broader terms, focusing on human capabilities, which are what a person can do or be.

Hilary Putnam and Vivian Walsh (2011) argue that in so doing, Sen (1987) is simply bringing back the rich philosophical anthropology of the classical political economists. They argue that while Sraffa's contribution can be best interpreted as the first stage in the revival of classical political economy, centred on the analytical economic framework of the classical theorists, Sen's contribution can be best interpreted as the second stage, centred on the philosophical anthropology of the classical authors.

The role that Sen's (*ibid.*) capability approach can play within the Cambridge Keynesian tradition is seen more clearly if we look at the theory of value and distribution. A central feature that emerges in the Cambridge controversies in the theory of capital, and is taken into account in the subsequent developments undertaken within the Cambridge Keynesian tradition, is that distribution is not determined by marginal productivities of factors of production, as in neoclassical economics. For Sraffa, distribution of income is an exogenous determinant of prices, and of the choice of the methods of production. For Keynes and Kalecki, distribution is a determinant of consumption, and of effective demand, as noted above. Sraffa's revival of classical political economy left quantities as an exogenous variable, and can be combined with the Keynesian–Kaleckian principle of effective demand, as argued by Pasinetti and Garegnani, amongst others, who provide a Keynesian–Sraffian synthesis (see Harcourt 2006). But the Keynesian–Sraffian framework leaves

distribution as an exogenous variable from the point of view of economic theory, and can be fruitfully complemented with Sen's capability approach, as argued in Putnam and Walsh and Martins (2013).

The fact that the contributions of Sraffa, Keynes, Kalecki, and Sen can be seen as part of a coherent project signals also an approximation between the Cambridge Keynesian tradition and the Cambridge welfare tradition, which has Sen as its greatest contemporary exponent. However, this approximation is possible only because Sen rejected the elements of the Cambridge welfare tradition that were not accepted by the Cambridge Keynesian tradition, in particular the use of marginal utility to study human well-being, and the theory of value (see Martins (2013: Chapter 6) for an elaboration). That is, Sen developed the key theme of the Cambridge welfare tradition, the distinction between wealth and welfare, in a direction which is actually compatible with, or indeed complementary to, the revival of classical political economy undertaken within the Cambridge Keynesian tradition. But even given the argument that he is the greatest contemporary exponent of the Cambridge welfare tradition, Sen is the only influential author of that tradition who abandoned marginal theory and utilitarianism. He is an exception since there remains a significant theoretical gap between the Cambridge Keynesian tradition and the Cambridge welfare tradition.

With respect to the methodological issues he explores, Sen is also closer to the modern Cambridge Keynesian tradition than to those authors who developed the Cambridge welfare tradition after Pigou. Sen (1989) noted, quite famously, that in economics it is better to be vaguely right than precisely wrong. In so doing, he was paraphrasing Keynes, who in turn was paraphrasing the British philosopher, H. Wildon Carr, who translated the work of Henri Bergson to English, and was much influenced by his philosophy.

The role of language is essential here. Keynes criticised the differential method used by Marshall and Pigou, which Sraffa also criticised, and the use of symbolic mathematics. Keynes (1936 [1973] 297–298) noted that symbolic mathematics presupposes the independence of the various factors involved, while ordinary discourse does not, since when using everyday language we can keep at the back of our heads the complexities and interdependencies of the real world. That is, in a world which is internally related, ordinary discourse is a more adequate language than symbolic mathematics, which presupposes an atomistic world (see also Harcourt (1987, 1995) and Lawson (1997, 2003) on the use of mathematics in economics).

The problem at stake here is the problem of internal relations, which was faced by Marshall early on, and led to Sraffa's and Keynes's critique of Marshall. Keynes believed the world was internally related, and stresses the

need to address the economy as a whole. As Harcourt (1987, 2003) explains, there are three aspects in Keynes thinking which are closely connected to the Cambridge tradition as a whole: the whole is more than the sum of the components; agents act in a context of inescapable uncertainty; and the need for a plurality of languages for expressing reality.

These elements of Keynes's thinking go back to the foundation of the Cambridge economic tradition with Marshall. The Cambridge Keynesian tradition provided a very different interpretation of classical political economy than the Cambridge welfare tradition of Marshall and Pigou. The economic theories developed by the Cambridge Keynesian tradition and the Cambridge welfare tradition are radically different. However, despite the very different theories developed within the two traditions—which spring from different interpretations of classical political economy—the underlying philosophy of both is the same. It is a philosophy aimed at developing a realistic theory of an internally related world, with the aim of improving human well-being.

7 Concluding Remarks

Neoclassical economics did not maintain Marshall's realist approach throughout the twentieth century, not even within the Economics Faculty that Marshall founded at Cambridge, as Harcourt (2003) noted 100 years after its foundation. The emphasis on the use of mathematico-deductivist methods, which had been criticised by Keynes early on, is now the central characteristic of mainstream economics, as Lawson (2003) notes.

Economics was always shaped by the mathematical methods used. The classical political economists, from Petty to Ricardo, used only arithmetic (as Marx and Sraffa also did later). The marginalists relied on differential calculus, and Marshall even found in it a justification for his partial equilibrium method, a justification which Sraffa and Keynes criticised. The twentieth century witnessed the further development of calculus, and the appearance of econometrics (criticised by Keynes), and of fixed-point theorems which shaped two central mathematical developments within mainstream economics, namely general equilibrium theory and game theory. The use of the mathematical methods developed during the twentieth century presupposes, of course, independence of the factors involved, that is, the absence of internal relations, which is the same thing as presupposing atomism, as Keynes realised.

Keynes's critique of the use of mathematical methods that presuppose atomism has been developed at Cambridge, especially by Tony Lawson (1997, 2003).

In so doing, Lawson recovers the fundamental notion of the philosophy of the Cambridge tradition, namely the notion of internal relations. Effectively, it is the recognition that economic reality is internally related that leads Lawson to the development of a realist perspective, and of a critique of mainstream economics. Although Lawson started his analysis of these themes through a study of Keynes's *Treatise on Probability*, the method of study developed by Lawson is quite in line with Sraffa's own approach. Just as Sraffa focused on the conditions of possibility for the reproduction of economic structures as a whole, so Lawson focuses on the conditions of possibility for the reproduction of social structures as a whole. The need of focusing on conditions of possibility for the reproduction of a whole stems from the recognition that reality is internally related, so much so that we cannot focus on the analysis of a given component while assuming that everything else remains constant, as Marshall did when addressing the problem of internal relations.

Lawson (*ibid.*) notes that in the natural sciences, we often can insulate and thereby identify a given mechanism through laboratory experimentation, in turn allowing for the application of mathematical methods. But in the social realm, we are in an open system, where mathematical methods that presuppose closed systems are not effective. In the social realm, the best methodological procedure available consists in finding partial regularities that persist through time. Kaldor misleadingly labelled these as 'stylized facts', and they were subsequently more appropriately reformulated as demi-regularities (or partial regularities) by Lawson. The closest analogue to a mathematical constant which can be found in economics are those persistent demi-regularities. However, most often the only mathematics that we need to possess in order to study them is arithmetic, used by classical economists since Petty.

The method of focusing on enduring regularities, that persist through time as a centre of gravitation, and can be observed as such in the real world, was the classical method, as Garegnani (1984) notes. Harcourt (1981) explains that it was also the method used by different Cambridge economists who shaped the Cambridge economic tradition, like Marshall, Keynes, and Sraffa. But at the level of economic theory, those economists had conflicting interpretations of classical theory: the Marshallian one, which is the dominant interpretation (which Keynes also followed, leading him to criticise classical theory), and the Sraffian one, which is simply Marx's original interpretation. The Cambridge contribution to the revival of classical political economy followed those two divergent interpretations, which remain today the most influential.

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2

Cambridge's Contribution to Methodology in Economics

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1 Introduction

Over its history, Cambridge has had a notable impact on the methodology of economics, and it is the purpose of this chapter to chart and discuss this contribution. We focus on some central methodological themes: the aims and scope of economics, how to deal with the limitations of both pure induction and pure deduction as a basis for an economic methodology; the appropriate nature and role of assumptions, of mathematical deduction and of real experience; and the place of moral considerations in economic theory. Methodology is understood as arising from epistemology and ontology, so we will include philosophy in our coverage. Indeed, it is in providing philosophical foundations that a significant part of the Cambridge contribution lies. Since ontology entails an understanding of the subject matter of economics, methodology is seen as encompassing also a view on the scope of economics.

By contribution we will mean explicit methodological (or philosophical) analysis by Cambridge economists and its effect on the methodological analysis of others, whether or not the effect involved a misinterpretation of the original analysis. While Cambridge has seen a wide range of methodological

This chapter has benefited from comments from Geoff Harcourt.

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practice and of theoretical contributions which embodied distinctive *implicit* methodological positions, we only consider explicit references to methodology. Even then the coverage is inevitably limited, not least because of the number of major figures who have been based at Cambridge and the wide reach of Cambridge's influence through the international spread of its former students.

Further, the development of philosophical and methodological thought alongside theoretical developments has been a complex process; the interpretations offered here inevitably gloss over much of that complexity. Particular emphasis is placed on interpretations emanating from Cambridge. The focus will be on some common threads which could be said to constitute a Cambridge tradition in economic methodology.¹ Martins's (2013) remarkable study and his chapter in the current volume makes the case (with reference to philosophical foundations) that there has been a revival in Cambridge of the political economy tradition of Smith and Ricardo. Here rather we start with the older political economy tradition in Cambridge, starting with Newton's experimental method, and consider contributions to methodology which could be seen as deviations from this Cambridge tradition, as well as the more recent revival of the older methodological tradition. In the process we trace the evolution of Cambridge's contribution of a 'third way' approach to methodology.

We consider first the Cambridge philosophical tradition as it existed at the emergence of classical political economy at the end of the eighteenth century, focusing, in particular, on mathematics, since it provides the background to Malthus's methodological contribution in his debates with Ricardo. The *Methodenstreit* then provides the context for the publication of John Neville Keynes's *The Scope and Method of Political Economy* at the time of Marshall's launching of political economy at Cambridge as a discipline separated from economic history. The older Newtonian Cambridge tradition resumed its influence with John Maynard Keynes's *A Treatise on Probability*, which is discussed in relation to contemporary Cambridge philosophy and in terms of the subsequent methodology of Keynes's economics. Keynes's followers in the Circus were more explicit on the subject of methodology, in a tradition continued and developed by Cambridge scholars up to the present day working outside the mainstream methodology tradition which now dominates the Faculty of Economics and Politics. It will be argued that keeping alive discussion of the philosophy of economics has been as important a contribution as particular philosophies of economics.

¹ See Dow et al. (1998) for a discussion of what constitutes a 'tradition' in economic thought.

2 The Cambridge Philosophical Tradition and Classical Political Economy

The Cambridge figure who arguably had the most profound influence on the methodology of political economy as it emerged in the eighteenth century was Newton, whom Keynes (1946 [1972]: 363) called 'Cambridge's greatest son'.² The scientific methodology he developed, as understood in Cambridge, had a distinctive character which carried over to subsequent developments in economic methodology in Cambridge. His experimental method for natural science involved *analysis* of experimental results leading to *synthesis* in the form of general principles in a process of abduction. These principles were provisional, allowing for the possibility that subsequent experiments might confound them, requiring reformulation. This methodology had several key features: it was realist in requiring a grounding in real experience with a purpose of uncovering real causal mechanisms, it was cautious as to the truth value of general principles, and these principles were open to revision in the light of new experience and took the form of tendencies rather than laws (Montes 2006).

For Descartes, in contrast, reason was primary, generating propositions by means of deductive logic with certainty (assuming the premises to be self-evidently true). These propositions could then take the form of natural laws, with empirical evidence only being sought for purposes of confirmation. The other counterpoint to Newton's influence was the empiricist position that the only source of knowledge was induction from enumeration of repeated instances. Newton's abductive methodology thus followed a path between the two extremes of pure deductivism and pure inductivism, involving a logic of induction through the mental process of identifying patterns.³ Keynes's (1946) [1972] essay on Newton was explicit that he was neither a rationalist nor an empiricist.⁴ Keynes (*ibid.*: 365) drew attention to the role of Newton's mind in forming hypotheses, maintaining that he conducted experiments to provide evidence for what he already knew intuitively.⁵ As Comim (2006: 129) puts it, 'the Newtonian system is

² Newton's thought of course had its own antecedents, but we begin the account with him.

³ See Loasby (2003) for a discussion of theorising along these lines, with particular reference to Smith's theory of mind.

⁴ There may be an autobiographical overlay to Keynes's interpretation, but if so this would reinforce the view that Keynes saw himself reinvigorating a Cambridge methodological tradition.

⁵ The British physical chemist Harold Hartley wrote to Keynes in response to his paper on Newton, noting the similarities with Faraday's methodology which he [Hartley] had earlier described as follows: 'In

neither an imagined construct nor a collection of haphazard empirical laws but something that transcends both'. This Newtonian middle way methodology, or *via media*, was to prove characteristic of a distinctive strand of methodological thinking in Cambridge which continues to evolve to the present day.

Martins (2013: Chapter 5) discusses the difference between Newton's and Descartes's methodologies as lying in their approaches to mathematics. While Newton relied on classical geometry as separate from arithmetic, Descartes combined the two in analytical geometry. While for Newton, geometrical argument related directly to real experience, arithmetic (algebraic) operations did not necessarily do so, as in the irrational numbers. Similarly, the calculus generated infinitesimally small numbers as well as infinity and points, which again have no real counterpart, yet they could be identified with respect to Cartesian coordinates. For Newton, it was important that mathematical argument correspond to real experience, building on common-sense understandings to the extent that this mathematical tradition continued in Cambridge in the education of economists, it influencing the methodology of the emerging classical tradition there.

For the development of political economy at Cambridge we need first to look at the methodological approach underpinning the emergence of political economy in Scotland, where Newton's influence was profound. As Montes (2006) and Comim (2006) show, Scottish Enlightenment thinkers shared the Cambridge understanding of Newton, rather than the rationalist Continental understanding of him. Hume and Smith in particular were great admirers of Newton and sought to apply his methodology to the emerging field of political economy, while being explicitly critical of Descartes's methodology. Further, Hume's resolution to the problems with both deductivism and inductivism provided a philosophical justification for Newton's methodology. Newton's approach to mathematics fed through to Scottish political economy mediated through the Scottish philosophy of common sense—a *via media* which required mathematical concepts to have their origin in sense data, while allowing abstraction in the form of simplification (Olson 1971), as well as paying due regard to ordinary experience (Comim 2002). This common-sense philosophy was to re-emerge in Cambridge with G.E. Moore (1925) and his influence on Keynes (Coates 1996).

the period of his great achievements, his experiments were rarely continuous, the intervals between them suggesting the subconscious working of his mind. He waited until the impulse came and his "prescient wisdom" had planned the experiment and foreseen the result' (Hartley 1931 quoted in Kuehn 2013: 27).

For political economy, experiments consisted of extensive historical study of different real contexts, from which patterns emerged with the assistance of the imagination and use of metaphor and analogy, from which provisional generalisations might be formed. The aim was to employ the imagination in order to formulate provisional generalisations about underlying causal tendencies, knowing that the scope for knowledge was limited by the complexity of social systems (Hume's problem of induction). The form of history used for providing experimental evidence was thus analytical, or conjectural, history (supporting, for example, a stages approach to socio-economic history) rather than the recounting of instances. Finally, like Newton, the aim for Hume was to develop theoretical reasoning which could be reconciled with 'vulgar' (non-specialist)⁶ understanding (Comim 2006: 126). For Smith (1762–1763) [1983] this reconciliation was a necessary element of the scientist's rhetoric of persuasion (in the absence of demonstrative proof) and reflected a concern for science to be problem-oriented and to have practical application. It also reflected the common-sense philosophy which supports the continued reliance on common-sense beliefs unless there is good reason to abandon them.

Scottish political economy in turn influenced the development of political economy at Cambridge. If we take Keynes's (1933 [1972]: 79) suggestion that Paley might be considered the first of the Cambridge economists, Waterman (1996) makes the case that, as Paley's thinking progressed, he was increasingly influenced by Scottish political economy. Keynes (*ibid.*: 71) otherwise gave priority to Malthus as 'the first Cambridge economist'. Malthus is of particular interest, given his explicit contributions to methodology. It was under the influence of Scottish political economy as well as what was still a Newtonian mathematics education at Cambridge that Malthus developed his theory of population.⁷ His methodological position was most clear in the context of his debates with Ricardo (Cremaschi and Dascal 1996). The focal point of these disagreements lay in Malthus's belief 'in the impossibility of reducing human needs and tastes to mathematical figures', leading Ricardo to criticise Malthus as being 'unscientific' (*ibid.*: 479). Like Smith, Malthus was doubtful of the accuracy of measures of value, for example, as well as the dangers of premature generalisation.

Malthus did allow for some laws of human nature (e.g. the need for food and shelter and attraction between the sexes) and of the natural sciences, but otherwise the laws of political economy are only probable and open to exceptions,

⁶This meaning differs from the Marxist use of the term to refer to non-Marxist economics.

⁷However, Olson (1971) argues that analytical mathematics was increasingly influencing thinking at Cambridge by the second half of the eighteenth century.

given the complexity of social systems. He pursued a Newtonian *via media* in his political economy, between the extremes of oversimplifying the subject matter by mathematising political economy with inattention to evidence, on the one hand, and assuming that surface appearances are causes, on the other. He also pursued a middle way with respect to language, criticising Ricardo for an undue separation between ordinary language and scientific language, while at the same time allowing for some specialist language to be developed with care in the interests of clarity. Here, Malthus is seen to pursue the realist Newtonian/Humean agenda of reconciling specialist with non-specialist understanding, while Ricardo pursued a rationalist interpretation of Newton (Cremaschi and Dascal 1996).

Meanwhile, John Stuart Mill was coming to dominate the agenda for classical economics, adopting a methodology which is characterised both as deductivist (for discovery) and inductivist (for justification); each operation required the ‘atomic hypothesis’, so that each was the logical counterpart of the other (Carabelli 1988: 77, 239). He was challenged in this by the Cambridge polymath, William Whewell, who developed a philosophy of science along the lines of a middle way between Continental deductivism and English empiricism. Whewell’s inductivism involved innate ideas, or ‘conceptions’, and thus the operation of the mind, being applied to observation. He was a realist in that observation was an essential element of discovery, and theory was provisional: the conceptions themselves required checking against further observation. Among the areas of application of his philosophy of science was political economy. Whewell is notable for reformulating Ricardian theory by developing the ‘earliest *systematic* application of mathematical symbols of political economy in England’ seeing algebra as a preferable ‘language’ to the arithmetic of classical economics (Campanelli 2008: 741; italics in original). Nevertheless, he criticised the departure of Ricardo’s deductivist theorising from real experience, which for Whewell was the proper starting point for ‘conceptions’ and reasoning; he also doubted the capacity of mathematics to deal with humanity in the round, including the moral dimension (see Snyder 2006: 279–285 and Harcourt and Kriesler 2016). Others at Cambridge who were concerned with political economy, such as Sidgwick, also engaged in methodological debate, seeking a balance between the deductive and inductive approaches (Schultz 2008), although Edgeworth promoted the deductivist approach as being more akin to the methodology of the physical sciences (Creedy 2008).

While there was this debate in Britain concerning the nature and role of deduction and induction, with Cambridge playing an important part in attempts to promote a Newtonian middle way, the methodological debate

on the Continent was more polarised in terms of a dualistic understanding of deduction and induction. The arguments between the Austrian pure theorists and the German Historical School became known as the *Methodenstreit*, or methodological struggle. Alfred Marshall sought to defuse the struggle in order to enhance the standing of economics as a discipline. For Harcourt (2003 [2012]: 201), Marshall is the initiator of the Cambridge political economy tradition.

3 The Cambridge Resolution of the *Methodenstreit*

Marshall was not inclined to engage in methodological debate, and yet his methodological approach arguably had a great impact. This impact is divided between those (non-mainstream) economists who focus on his professed methodology and his analysis of evolutionary contexts (e.g. as in industrial districts), on the one hand, and those (mainstream) economists who have pursued the deductivist methodology he used in his formal theoretical marginal analysis, on the other. Groenewegen (1995: 415; italics in original) quotes Marshall, in a letter to Neville Keynes, as follows: 'I take an extreme position as to the *method & scope* of economics. In my new book I say of *methods* simply that economics has to use every method known to science'. This pluralist methodology included a particular view of mathematics which followed in the Newtonian–Scottish–Malthusian methodological tradition. First, the emphasis was on geometry rather than algebra or calculus. Second, mathematics was not to be used as an engine of enquiry, but rather as a substitute for verbal argument to be set aside when communicating theories with the aid of real-life examples. This reflected both that, like Hume, Marshall sought to make connections across the learned–vulgar divide and his view that political economy had a human and concrete nature.

Marshall's evolutionary view of the subject matter of economics limited the scope for general laws arrived at either by pure deduction or pure induction. Marshall explicitly argued against 'economic man' as an appropriate basis on which to build theory, on the grounds of its contradiction by observation. Deductivists in contrast proceeded by accepting the concept of economic man as 'self-evident', and as fruitful given the scope for application of marginalist analysis on the basis of this conception. Marshall's evolutionary view required him to engage in detailed study of real social contexts in order to grasp the nature of particular evolutions. Yet, for all his use of historical evidence, Marshall was critical of the English Historical School's empiricism

(pure inductivism) on the grounds that the evolutionary nature of the subject matter precluded identification of permanent uniformities (Groenewegen 1995: 310). It was rather Hegelian analytical history⁸ which he saw as playing an important methodological role, whereby the operation of the mind was brought to the identification of conjectural patterns in history. From Hegel too Marshall absorbed the organicist idea of the whole being more than the sum of the parts.

The presentation of the *Principles* thus involved a methodology which differed in important respects from the other builders of the marginalist revolution. Yet it was those elements of the content of the *Principles* which were most consistent with the development of marginalism elsewhere, rather than Marshall's professed methodology, which had the greatest impact on economics as a whole. Indeed, Whitaker (2008: 364) argues that 'his [Marshall's] method was in the general deductive tradition of John Stuart Mill'. His key theoretical contribution as far as most economists are concerned was the conceptual framework he developed for marginalist comparative static analysis of exchange, which spawned theoretical developments, generating law-like conclusions about the consequences of atomistic individual behaviour. For Marshall, perfect competition and laissez-faire were intended simply as starting points for the analysis, but they became the benchmark for the bulk of mainstream analysis.

The fact that Marshall's methodology has been open to conflicting interpretations is due in large part to the conflict between the methodology implicit in much of his theorising and the methodology he professed (Pratten 1998). But, whatever his impact on methodology (intended or unintended), Marshall is also important for his *indirect* methodological contributions. First is the part he played in establishing the Economics and Politics Tripos at Cambridge and of moving economics from the Moral Sciences Board, with profound effects on the future development of economics at Cambridge. In promoting the idea of economics as a mature discipline, Marshall put great emphasis on continuity of ideas (as in the first Preface to the *Principles*), rather than discontinuities. As a result, he distracted attention from the ways in which his own methodology differed from the other approaches in Britain and further afield.

It was in this respect that Marshall is also important for promoting his protégé, John Neville Keynes, the father of John Maynard Keynes. Keynes's methodological treatise, *The Scope and Method of Political Economy*, served

⁸There is a parallel with the Scottish analytical history tradition which can arguably be traced through Hegel, but it was Hegel who was the direct influence on Marshall (Groenewegen 1995 and Martins 2013).

to support Marshall's efforts to establish economics as a discipline (Deane 1983)⁹ and has had an impact on the wider discipline which continues to this day. For Neville Keynes, the subject matter of economics was wealth: 'By economic activities are meant those human activities that direct themselves towards the creation, appropriation, and accumulation of wealth; and by economic customs and institutions, the customs and institutions of human society in regard to wealth' (Neville Keynes 1904: 2). This definition is not inconsistent with the classical view of economics as being chiefly concerned with production, distribution and exchange, or Marshall's (1890: 14) more vague 'ordinary business of life'. But Keynes's definition was to be superseded by the Robbins definition in terms of scarcity, which diverted attention from customs and institutions to rational choice and deductivist methodology.

However, a particular understanding of Neville Keynes's methodology itself has had a lasting impact. In fact, to the extent that mainstream economics textbooks start with a methodological statement, it includes a version of his position on method. Keynes set out a hypothetico-deductivist methodology, which seemed to reconcile the two opposing positions of deductivism and inductivism. This was achieved by treating each as the counterpart of the other, such that rational argument was used for discovery while empirical evidence was used for confirmation of conclusions. But for Keynes induction was not the logical process in the Newtonian tradition of applying the mind to experience, but rather the collection of evidence on repeated instances of constant conjunctions of events. Rather than the transcendent middle way of the Cambridge tradition, therefore, this was simply a synthesis of deductivism and inductivism within a positivist logical framework.

Keynes's methodological analysis was well suited to seeking a synthetic solution to the *Methodenstreit*, being carefully worded and heavily qualified and thus open to a range of interpretations. But Keynes's emphasis was much more on the side of deductivism (Deane 1983; Moore 2003), not least because of his stated aim to identify uniformities (albeit with the *ceteris paribus* qualifier). Keynes did see evidence as having importance for confirming not only theoretical propositions but also the assumptions on which they are based. Like Marshall, Keynes had doubts about the concept of economic man. But the tendency was for this (realist, Cambridge) aspect of his methodology to be ignored. Once the rational choice assumptions were widely taken as uniformly supported by evidence (or self-evidently true), they seemed no longer to require confirmation; they could therefore provide a universal basis for hypotheses. By trying to set out a synthesis of competing methodological positions, Keynes ended up

⁹ Keynes has been seen as setting out a rationalisation of Marshall's methodology (Whitaker 2008).

downplaying Marshall's emphasis on real evidence, encouraging the deductivist reading of Marshall and supporting the rising tide of deductivist marginal analysis. Inductive argument was effectively limited to testing theories. Although presented as a middle way, in fact Keynes's hypothetico-deductive methodology encouraged a deductivist approach to establishing hypotheses.

This influence was reinforced by Keynes's discussion of the distinction between positive, normative, and applied economics (which he had earlier identified as characteristic of the deductivist position). While Hume had already distinguished the first two concepts, Keynes went much further in positing that a value-free economics was possible, to which values could later be added.¹⁰ As Deane (2008: 725) points out, this was a device to insulate the 'hard scientific core of economic theory...from the charges of ideological bias, or immorality, or relativity, as well as from failures in practical economic policies'. But as a result Keynes provided a rationale for deductivist methodology which could be presented as if value-free.

By supporting Marshall's drive to establish economics as a mature discipline, with intellectual continuity and methodological consensus, Keynes had helped to suppress important methodological issues and provided a foundation for Mill's increasingly dominant approach. Indeed, many at Cambridge proceeded within that framework. But, while it would seem that an important Cambridge contribution to economic methodology was to develop and promote a methodology which had emerged elsewhere, the next generation of Cambridge economists departed from the apparent consensus and made a series of influential contributions to methodology which can be traced back to the older Cambridge tradition.¹¹

4 John Maynard Keynes and Methodology in Twentieth-Century Cambridge

The leading figure of this new generation was Neville Keynes's son, John Maynard Keynes, who came to economics from the philosophy of mathematics. His explicit contributions to economic methodology were, as for Marshall,

¹⁰ Colander (1992) has revived this framework in order to argue for a pluralist methodology for applied economics, but where the theory to be applied is positive, and derived by means of deductive logic: 'The art of economics is applied economics. It relates the lessons learned in positive economics to the normative goals determined in normative economics' (ibid.: 192).

¹¹ While Weintraub (2005) challenges the idea of a continuing Cambridge tradition stemming from Marshall, given the diversity of approaches within Cambridge, our focus here is on *distinctive* Cambridge contributions. What is emerging is that one such contribution is the continuing thread of a distinctive methodological approach stemming from Newton.

spasmodic rather than the kind of focused study his father undertook. Yet his distinctive methodology, as set out by Shackle (1974), Chick (1983: Chapter 2), and Harcourt (1987), has had a profound influence on Post-Keynesian economics. The philosophical foundations of this methodology became a significant field of study, beginning with the path-breaking work of Meeks (1976), Carabelli (1988) and O'Donnell (1989). As Runde (1997: 240) points out, Keynes's methodological position fell outside the positivist tradition which, as we have seen, was encouraged (however inadvertently) by his father. Nevertheless, many of Maynard Keynes's methodological statements were expressed in the process of criticising the positivist mainstream, a phenomenon which has carried forward to modern-day heterodox economics.¹²

Keynes's earlier work on probability allows us to infer his philosophical approach when he came to economics, where his stated aim was not just methodological but epistemological: to 'revolutionise...the way the world thinks about economic problems' (Keynes 1935 [1973]: 492–493). In *A Treatise on Probability*, Keynes explored the grounds for belief as the basis for action under the general conditions of uncertainty which followed from his organicism. Where structures evolve and the interactions between their components evolve (i.e. the atomic principle does not hold and the physical or social system is open), probabilities cannot be quantified. But rather than falling into the philosophical dual of certainty/ignorance, Keynes explored the realm of uncertainty, outlining the mechanisms by which more or less reliable knowledge may be established with more or less confidence. Keynes was thus updating Hume's epistemology as a resolution to scepticism with respect to reason on the one hand and the problem of induction with respect to an organic system on the other. This was Keynes's own version of the middle way, in the tradition of Newton (and Hume and Smith), whereby the mind identified patterns from experience; the resulting generalisations were provisional in the face of an organic system and, for application (the purpose of theory), required close reference to that evolving reality (Carabelli 1988: Chapters 4 and 5). These generalisations about tendencies were to be reasonable even if they could not be demonstrated to be true.

Keynes explicitly challenged the prevailing positivist view of induction which involved gathering data on repeated instances ('pure' induction) as requiring empirical uniformity of nature. Rather he understood induction as being a logical process of employing negative analogy in order to uncover persistence in spite of difference. But Keynes departed from classical logic (which required certainty as to the truth of premises) and empirical logic (which

¹² See Gerrard (1997) for a comprehensive review of Keynes's methodology.

required unambiguous facts). Keynes was thus diverging from the endeavours of Russell and (the early) Whitehead to build a complete mathematical system based on classical logic. His (human or ordinary) logic was more suited to conditions of uncertainty. As with Hume's epistemology, Keynes founded his logic on common sense and convention and he employed ordinary language.¹³

Just as the scientist organises observations according to prior conceptualisations and patterns, so the individual or group in society has to apply judgement to observation. Keynes was quite explicit about the ambiguity of evidence this entails. He referred to direct knowledge based on experience in the following terms: 'Sensations which we may be said to *experience*, the ideas of meanings, about which we have thoughts which we may be said to *understand*, and facts and characteristics or relations of sense-data, or meanings which we may be said to *perceive*' (Keynes 1921 [1973]: 12; italics in original). The theory of probability was thus subjective in the sense that the evidence brought to bear and its assessment in relation to other sources of knowledge involved judgement. But it was objective in the sense that anyone in the same circumstances and with the same understandings of the evidence would arrive at the same judgement.¹⁴

When Keynes turned to economics, this epistemology had strong methodological implications (Chick 2003). First, he regarded economics as an art. While the father had privileged deductive theory as the core of the discipline, with values imported later and 'unscientific' methods only introduced at the stage of policy application, the son privileged the art of application at the core. For him, the requirements of practical application determine the methodology of theory development and, given the open-system nature of the subject matter, that methodology was pluralist. Ordinary logic required multiple strands of reasoning and evidence which could lend weight to argument, for economists as well as economic agents. Keynes was therefore highly critical of the monist methodology of positivism (O'Donnell 1989: Chapter 9).

As with Marshall, Keynes's ideas were then reduced by mainstream interpreters to their mathematical versions within a static equilibrium framework. Also like Marshall, Keynes himself used mathematical formulations, but only as a contributor to the overall argument. Keynes applied the same logic to the testing of theory by means of econometrics, a logic which he made explicit in his critique of Tinbergen. His primary critique was of econometric analysis which requires an invariant structure; he argued that the *onus* should be on

¹³ This theme was developed by Shackle (1983: 116) for whom mathematics was insufficient, being too restrictive a language compared to verbal argument: '[L]anguage at its full compass, where words are fingers touching the keyboard of a hearer's mind'.

¹⁴ On Keynes's debate with Ramsey over subjectivity, see Carabelli (1988: 96–97).

the econometrician to demonstrate that a particular case reasonably approximated a fixed structure, so that regression analysis was warranted. O'Donnell (1989) shows that, as with his objections to mathematics, Keynes's objections were to Tinbergen's specific techniques in relation to the subject matter, not to econometrics per se.

While rational economic man had increasingly taken hold as the axiomatic foundation of deductivist theory, Keynes was particularly critical of the fictional nature of the concept. In line with his organicist view of society, Keynes rejected an atomistic representation of economic agents. Rather he emphasised man's social aspect, not least when it comes to epistemology and the reliance on social conventional knowledge. Here again Keynes pursues a middle way, made explicit in his movement away from the reason–emotion dual represented by Russell and D.H. Lawrence, respectively (Keynes 1938 [1972]). The methodological implications of this epistemology are most evident in Chapter 12 of *The General Theory* (Keynes 1936 [1973]), where he analyses the determinants of the macroeconomic variable which was central to his theory of effective demand: investment (Runde 1997). His objections to the rationality assumptions arose from philosophical argument rather than explicit evidence; consistent with his theory of probability, Keynes's methodology centred on logic. Nevertheless, his philosophy was founded in turn on an organicist ontology which arose in an inchoate way from 'deep background' experience (Searle 1995). While Keynes did not seek out evidence to the same systematic extent as Marshall, his human logic was informed by experience.

Chick (2003) spells out further the connections between Keynes's ontology and epistemology and his methodology.¹⁵ From the former he set out to develop an open theoretical system which was general in its openness; the certainty of the closed mainstream system was a special case of the more general open system, characterised by uncertainty. This system therefore had at its heart a 'method of expectations', formed under uncertainty, within different time frames. Time played a crucial role, understood in logical terms, referring to causal sequence, or in historical terms, both of which gave equilibrium meanings beyond the limited mainstream meaning of a solution to simultaneous equations. Further, because money had a special role in decision-making with respect to an uncertain future, monetary and real factors were to be treated in an integrated way. But, contrary to the reductionism of the mainstream approach, some of the analysis was to be conducted at aggregative levels without an explicit derivation from individual decision-making. In

¹⁵Consistent with our characterisation of a Cambridge methodological tradition, Chick (2003: 311) characterises Keynes's theory of rational belief under uncertainty in terms of a 'third way'.

any case, again like Marshall, Keynes was conscious of the important role of the evolution (in historical time) of the social conventions and institutions which provide the framework for decision-making. Like Marshall he too was motivated to improve society. While he apparently separated off his social philosophy (consistent with Neville Keynes's normative–positive distinction) in advocating that the 'economic problem' be addressed first in order then to promote the Good Life, solving the economic problem itself involved moral judgements, for example, about the need to prevent unemployment and the need for institutions to promote the public interest (Chick and Dow 2013). Economics was a moral science.

Even though Keynes's methodology was misunderstood when interpreted from the prevailing positivist standpoint of the mainstream, there was nevertheless a Keynesian methodological revolution from that perspective (i.e. within a narrow understanding of methodology) (Dow 2010). On the one hand, Keynes sparked off the development of macroeconomic theory as a field distinct from microeconomics. On the other hand, he provided the related impetus to the development of large databases and econometric techniques to analyse them, and indeed provided an agenda for the development of econometrics in attempting to deal with the problems raised in his debate with Tinbergen. But Keynes had a more enduring impact among those who took his alternative methodology seriously, forming under the banner of Post-Keynesianism with its distinctive methodology (Chick 1995). This approach includes Keynes's typically Cambridge requirement for abstractions to take the form of simplification rather than fiction, with a view to allowing application to the more complex reality; for a focus on the passage of historical time with its implications for the uncertain basis for decision-making; and for aggregation to allow macroeconomic analysis distinct from microeconomics, paving the way for analysis of growth and distribution.

The literature on the Cambridge figures who contributed to this development is vast. Here we pick out just some of the important contributions specifically on methodology rather than the theory of Keynes's followers at Cambridge (the Circus) who perpetuated and developed aspects of his methodological legacy. Joan Robinson (1962: Chapter 4) pinpointed Keynes's departure from the mainstream approach in terms of a shift in the subject matter to the capitalist system as a phase in history, bringing to the surface moral issues (with respect to the free operation of markets) and time. Keynes's followers added to this a shift away from concern with the short period, where income distribution and power relations could be taken as given. Many of his followers in Cambridge were influenced by Marx and his classical concern with distribution and economic processes over the long period analysed in class terms.

Among the Circus, Joan Robinson made the most explicit methodological contribution. Her early book, *Economic Philosophy*, had a particular impact in the 1960s when there was a receptive audience for a reflection on the foundations of economics. Her methodological stance shifted somewhat over her career (Harcourt 1996a; Salanti 1996). But Robinson, like Keynes, believed that economics should address real issues, and take account of institutions (the 'rules of the game'), and therefore that theoretical assumptions be simplifications rather than fictions. As with Keynes, too, she challenged apparently-false assumptions on the basis of her ontology rather than specific empirical study. She also shared Keynes's views about the uncertainty of knowledge and thus the impossibility of establishing economic laws. She made a particular contribution in her clear distinction between a logical-time framework, which aims to establish causal mechanisms, and an historical-time framework, which aims to establish irreversible processes such that equilibrium is a position to which a process tends in the long run.

While Kaldor (1972) too criticised the ahistorical mainstream version of equilibrium, he also contributed to the methodological approach to evidence, developing the notion of stylised facts (as clarified and extended by Lawson 1989). Given the open nature of the economic system, detailed facts reflect a variety of tendencies at work under particular circumstances. By abstracting from this detail, stylised facts can form the basis of hypotheses. Here we see a version of the interaction of the mind with observed experience as a way of addressing the problem of induction, in the Newtonian/Smithian tradition. The Department of Applied Economics was established at Cambridge in 1945 to provide the empirical material on which such abductive reasoning could build.

In some respects Robinson could be said to have adopted a position similar to that of Neville Keynes: what Salanti (1996: 292) refers to as 'empirical apriorism'.¹⁶ To the extent that she aimed her critique of the mainstream at the realism of assumptions and the internal consistency of its logic, she was implicitly accepting the underlying positivist methodology. This was perhaps most evident in the 'capital controversies', where Robinson challenged mainstream marginalist analysis of value and production, such that the controversy came to be epitomised for many by the 'reswitching' problem which demonstrated that capital and its return need not be inversely related (Harcourt 1972). The fact that the force of this critique did not seem to be felt by the mainstream illustrates well the insufficiency of empirical apriorism as a methodology. In fact, while the debate was widely interpreted in the terms

¹⁶The continuing influence of Neville Keynes may have been due in part to the fact the Maynard Keynes never produced a systematic account of his position on methodology.

of the mainstream approach, the differences over capital theory arose from different ontologies and epistemologies. Robinson's views on method were in fact changing as she moved away from Marshall's influence and pursued the methodological implications of analysing a dynamic process over time. She argued further that ideology was embedded in mainstream theory, contrary to professed mainstream methodology. While her efforts to strip it out have been seen by some to reflect a Keynesian view—of the John Neville type—that ideology-free theory was indeed possible, Pasinetti (2008: 218) rather emphasises Robinson's position simply that ideological views need to be made explicit since economics was ideologically non-neutral.

Other key contemporary figures in Cambridge were Michał Kalecki and Piero Sraffa, who both came to Cambridge from different methodological traditions. Both influenced others at Cambridge and spawned schools of thought developing their ideas within their distinctive methodological frameworks. Both focused on issues of production and distribution in historical time, influenced by Marx. Kalecki drew on empirical evidence, filtered through his careful conceptual classification of variables, in order to establish stylised facts with respect to the short period (such as all wages being used up in consumption). Sraffa differed from Keynes and Kalecki in focusing on the long period and in presenting a formal mathematical *system* (rather than partial mathematical arguments). But, since the content was not marginalist, Sraffa's mathematics was different from the mainstream calculus, employing the mathematics of the classical period (Velupillai 2008). While the Ricardian tradition might seem to depart from both Malthusian and Post-Keynesian methodology, Sraffa traced his concern with struggle over the surplus in historical time back to both Ricardo and Malthus (via Marx).¹⁷ This interpretation of Ricardo as not being so different from Malthus was distinguished by Dobb (1931, 1959) from Marshall's version, which was arrived at through Mill (see Harcourt and Kriesler 2016).

The resulting strands of Post-Keynesianism differ in part in terms of how much of the analysis was amenable to formal mathematical expression (echoing the debates between Malthus and Ricardo). But others have used formal mathematical models of the long period which are more in the Marshallian tradition of specifying equilibrium in gravitational terms.¹⁸ There are nevertheless common elements to the methodologies emanating from the key

¹⁷ This reading of Ricardo implies that the methodological differences between him and Malthus were less marked than is implied by their own debate, or that the differences were more of degree than of kind, where the latter might characterise the differences between marginalist economics and Cambridge political economy.

¹⁸ See, for example, Harcourt's (2006: Chapter 5) discussion of Marglin (1984).

Cambridge figures which are in accord with what we have identified as a Cambridge methodological tradition. Thus, Pasinetti (1974: 43–44) identified common features between the Ricardian approach and Maynard Keynes's methodology, while Dutt and Amadeo (1990) set out the common methodological ground between Post-Keynesians and the neo-Ricardians who followed the Sraffian approach.¹⁹ Arestis et al. (1999) likewise identified the coherence of Post-Keynesianism, in spite of its different strands, in terms of methodological approach. The rather vexed question of assessing differences relative to commonalities needs to be considered in terms of a pluralist, non-dualistic approach to epistemology. Phyllis Deane (1983) criticised Marshall's and Neville Keynes's search for synthesis, advocating instead methodological pluralism. Her history of economic thought followed the Cambridge tradition we have identified by focusing on methodology from the perspective of (analytical) economic history (Deane 1978). Like Robinson, she kept philosophical issues to the fore at a time when she could reach a receptive audience.

Geoff Harcourt's history of economic thought also took a methodological perspective, charting the contributions of the Circus (see, for example, Harcourt (1972, 2006) and Harcourt and Kerr (2009) on the capital controversy). His own work has employed a pluralist methodology to theory development, governed by a close understanding of real circumstances and by policy concerns. He has made a particular contribution to methodology by articulating and justifying his 'horses for courses' approach whereby selections are made from a plurality of methods according to the problem at hand, rather than according to some internal requirement (Harcourt 1996b). In particular, it involves a continuation of the Cambridge critique of the mainstream requirement to express all arguments in terms of formal deductive mathematics. The 'horses for courses' approach contributes to the increasing focus in heterodox economics on pluralist methodology, not as anything goes, but as an ontologically grounded selection of methods (Lawson 1997a; King 2002).

5 Cambridge's Contributions in the Twenty-First Century

Many of the current strands of heterodox thought can be traced back in one way or another to Cambridge. Now heterodox economics involves a large worldwide community bound by a critique of monist adherence to mainstream methodology and the promotion instead of methodological pluralism.

¹⁹Note that their title is *Keynes's Third Alternative?*

Although the Faculty of Economics and Politics at Cambridge is now almost exclusively mainstream, Cambridge has again been making a distinctive methodological contribution to heterodox economics in the form of critical realism as spearheaded by Tony Lawson (1997b, 2003) and developed by the Cambridge Social Ontology Group.²⁰ Critical realism encapsulates much of the Cambridge middle way tradition (while also, like earlier Cambridge contributions following Maynard Keynes, drawing on elements of the Marxian tradition).

Critical realism is primarily a philosophical position, introduced by Lawson (1997b) as drawing on the philosophy of Roy Bhaskar, countering Enlightenment (especially Humean) philosophy according to a positivist reading of it. But a reading of Scottish philosophy as providing a philosophical foundation for the Newtonian tradition, and developing it for the social sciences, in fact shows that it provides an alternative foundation for critical realism, and one which accords with the Cambridge tradition (Dow 2002). Critical realist philosophy puts the focus on the ontological level, on the grounds that any epistemic question requires reference to some position or other as to the nature of the subject matter; the focus is on devising theory by a methodology which best allows practical questions to be addressed, rather than by any internal criteria. One of the major achievements of critical realism is to have made this so explicit, such that it has become increasingly commonplace in heterodox economics to spell out the underlying ontological position.

The argument in favour of making ontology explicit is aimed at bringing to the surface the contradiction between the closed-system ontology implied by mainstream methodology and the common understanding of the economic system as being open—a modern version of the Cambridge position. For critical realists, a closed-system ontology is defined in terms of event regularities which allow for identification of economic laws, which in turn allow prediction. An open-system ontology refers to a subject matter which is evolving, including evolving internal relations, such that there are no uniformities to uncover and little scope for precise prediction. Real social systems are seen as being structured according to three levels: the actual, the empirical, and the real. The aim of economic enquiry is to build up understanding of causal mechanisms operating at the (unobserved) real level, but if the system is open,

²⁰ Amartya Sen can also be interpreted as contributing to methodology in line with the predominant Cambridge tradition, with significant influence on current developments in economics. While his methodology is too large a subject to include here, see Martins's (2013) detailed treatment.

this understanding takes the form of identifying tendencies which may or may not be operating, singly or together, in any one set of circumstances.

The methodology for identifying tendencies is a development of the Cambridge middle way, eschewing the deduction/induction duality. Rather than seeking evidence in the form of repeated instances, critical realists seek to identify patterns, in the form of stylised facts, or partial regularities, or 'demi-regularities'. This involves an application of the mind, employing, for example, metaphor, in order to 'see' patterns which might be indications of causal mechanisms for further investigation. Particularly revealing are contrastive demi-regularities, which suggest the presence of a causal mechanism in some circumstances but not in others. These mechanisms are to be investigated by means of methods suited to the subject area; this may include *some type* of formal mathematics if it can be justified as contributing to the analysis—a pluralist methodology.

However, the plurality *of* methodologies is seen as being limited to different research interests, while united by a shared open-system ontology and epistemology (Lawson 2003). Thus, critical realists distinguish different schools of thought by their particular subject matter of interest: gender, institutions, class, and so on. Since social systems involve both power relations and moral conventions, ideology and moral judgements are part of the reality which economists analyse. Further, the purpose of critical realist economics is given as social transformation as a moral imperative.

The presentation here of the contributions to methodology emanating from Cambridge has in fact been an exercise in the Cambridge methodological tradition that we have drawn out from the historical account. The topic was approached with some idea of the conceptual issues, and a pattern was then identified from general reading, in the form of the middle way methodological approach. But the expression of that approach differed between different circumstances, and continues to evolve. Also, the consistency with which it has been applied was variable. In particular, the influence of Marshall and Neville Keynes (reflecting the dominance then of the Millian approach) constituted a divergence from the posited Cambridge tradition, and served to confuse the evolution of the revived tradition in Cambridge. Further, the influence of all the major figures covered here is coloured by the different interpretations of them by readers coming from different methodological perspectives (including within Cambridge). While a tradition has been identified here as Cambridge's important methodological contribution, this is inevitably a provisional judgement.

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3

Cambridge Theories of Welfare Economics

Rogério Arthmar and Michael McLure

1 Introduction

During the last decades of the nineteenth century in England, ideas related to socialism evolved from early agrarian pleas for land redistribution into the development of labour-exploitation theories of industrial society and the purported iniquity that derives from the private appropriation of wealth (Callaghan 1990: 1–61; Claeys 1987: 130–195). Concurrent with the spread of socialist doctrines and the rise of syndicalism, the prevailing liberal view of society underwent a profound adjustment of its own in Britain. Private actions began to be recognized for their impact on the community at large, while the long cherished *laissez-faire* doctrine came under pressure from the new belief that social problems could be mitigated through state policies. An evolutionary and practically oriented form of liberalism started to take shape, anchored in the principles that private property and authentic equality of opportunities could exist side by side, and that individualism entails social

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responsibility in a just society. To a considerable extent, this transition was facilitated by John Stuart Mill's substantial qualification of Bentham-inspired utilitarianism (Freeden 1986: 25–75; Baumol 1967: 180–196; Roach 1957) as well as by Henry Sidgwick's even greater qualification of utilitarianism, but more forcefully through the contribution of the British idealist movement, which partly emerged in opposition to utilitarianism, starting with the work of Thomas Hill Green in Oxford and followed by that of Francis Herbert Bradley, Bernard Bosanquet, and others (Mander 2011).

This broad transformation in the liberal perspective on society during the mid- to late Victorian era was closely related to developments in the British academic world, with questions about the welfare of the social aggregate starting to be considered as a field of inquiry that was as legitimate as the study of the production of wealth had been until then. Perhaps partly because of their direct link with British idealism, leading scholars from Oxford tended to adopt a strategy of direct action and involvement in politics for the government of the whole. Leading scholars from Cambridge, however, tended to feel more at home by engaging in a theoretical discourse on welfare that derived from a blend of mature utilitarian and marginalist ideas, complemented by a fair deal of ethics, history, and, occasionally, epistemology (Backhouse and Nishizawa 2010: 1–14). The work that emerged from this collective endeavour focused essentially on actual failures and unaccounted effects of the market mechanism, with distinct attention to the proper means by which such failures can be corrected (Groenewegen 1995: 302–342, 531–569; Maloney 1991: 165–185; Myint 1948: 120–141).

This chapter discusses the theories of welfare economics that emerged over three periods, each developing under the stewardship of the incumbent Cambridge Professor of Political Economy: the late nineteenth century, under Alfred Marshall's stewardship (1884–1908); the early twentieth century, under Arthur Cecil Pigou's stewardship (1908–1943); and the mid-twentieth century, under Dennis Holme Robertson's stewardship (1944–1957). Each of these three periods will now be examined in successive sections.

2 Late Nineteenth Century

During Marshall's tenure as Professor of Political Economy a number of Cambridge scholars contributed to ideas that either directly addressed issues of economic welfare or influenced subsequent developments in that field. Particularly relevant in this regard are Henry Sidgwick, for his *The Principles of Political Economy* (Sidgwick 1883 [1887]), Herbert Somerton Foxwell,

for his essay on the 'Irregularity of Employment and Fluctuations of Prices' (Foxwell 1886), Alfred Marshall, for his influential tome on the *Principles of Economics* (Marshall 1890 [1920a]), and John Neville Keynes, for his careful study of method in *The Scope and Method of Political Economy* (Neville Keynes 1891 [1897]).

Sidgwick and the Welfare-enhancing Art of Political Economy

In philosophical circles, Henry Sidgwick is famous for his book *The Methods of Ethics* (1874) [1901], which went through six editions and provided yet a further refinement of utilitarianism, although, as Backhouse (2006: 21) has pointed out, Sidgwick did not succeed in presenting a rational demonstration of why utilitarianism should be preferred to egoism. But in his most important incursion into economics, *The Principles of Political Economy*, Sidgwick succeeded in presenting a challenge to the alleged efficiency of a competitive economy in regard to both the production of wealth and its distribution. He contended that the prevailing view exaggerated the gains from competition as such gains could only be conceived in the realm of pure abstraction, and that it actually did not extend to the world in which people live. Once a criterion of social welfare is defined, though, Sidgwick pointed out that different conclusions could be reached and, in view of this, welfare considerations ought to move to the sphere of political economy as an art. In other words, political economy becomes a guide to statesmen to act in order to improve the well-being of society. In line with this particular approach, Sidgwick enunciated the two main goals of the art of political economy, namely: (1) to make the proportion of produce to population a maximum, as measured by the ordinary standard of exchange value, and (2) to ensure the proper distribution of income among members of the community based on the general principle of making the whole produce as useful as possible (Sidgwick 1883 [1887]: 83–84, 397, 519, 1891 [1897]: 39–41, 160). With this in mind, Sidgwick suggested that state intervention was a normal condition of modern industrial life, a proposition that he complemented by compiling a detailed catalogue of instances where public policies should be employed to promote welfare.

In regard to production, which concerns the first of the basic objectives of the art of political economy, Sidgwick introduces the crucial notion that the utility to society from the output of some economic enterprises could be different from the corresponding private utility. For instance, lighthouses, forests, and scientific research generate a substantial public advantage, respectively, for

navigation, rain precipitation, and industrial productivity, while they are economically unattractive when considered from the viewpoint of private utility. More generally, Sidgwick maintained that the sum of individuals' utilities may diverge from the aggregate value of goods measured at market prices because the price of goods corresponds to marginal utilities, whereas the utility derived from consumption reflects average utilities (Backhouse 2006: 22). In view of this, he considered that the state had a role in taking action to facilitate the provision of goods in short supply, where under-provision is a consequence of marginal utility falling below average utility. Conversely, in the case of monopolies, parallel railways, and predatory fishing, for example, the private gain involved in these activities is significantly larger than the corresponding public utility, so state intervention is called for to nationalize monopolies, to regulate the railways, and to assure the survival of sea life (Sidgwick 1883 [1887]: 437–485).

As for the distribution of income, which relates to the second of the basic objectives of the art of political economy, the most important initiative concerns the constitution of a national system of education, out of public funds. This basic educational structure would be supplemented by a network of universities aimed at advancing knowledge and improving the skills of the labouring classes. In doing so, Sidgwick interrelates improvements in the distribution of income with enhancements in productivity through education and skills development. Emigration schemes would also be operational in bringing about gains for the community by placing the labourers in more favourable conditions regarding the access to land. The progressive undertaking of private activities by the state, in addition, would reduce the share of national income distributed in the form of profits, thereby allowing its redistribution to the aggregate remuneration of labour. The legal prescription of a minimum wage for all kinds of occupations, however, would have to be accompanied by some kind of mandatory exertion, as required by the Poor Laws, in order to preserve the incentive to work among able-bodied men. Finally, a system of compulsory insurance might be devised to secure the working population against infirmity and old age. Additional redistributive policies suggested by Sidgwick consisted of the supply of cheap credit for peasants and the institution of a system of public health for the poor financed by taxes on the wealthiest (*ibid.*: 499–544).

Foxwell on Periodic Mismatches, Speculation, and Welfare

Herbert Foxwell's agenda for social reform arose as the outcome of his critical view of the liberal doctrine and the repeated emergence of unemployment

when that doctrine becomes policy. During the 1870s in England, belief in the infallibility of the market came under severe attack on methodological, historical, and moral grounds. The English Historical School, represented by, among others, William J. Ashley, John K. Ingram, and Thomas E. Cliffe Leslie, claimed to have developed a new basis for economics in which science would be built from the facts of social life instead of being constructed from an individualistic philosophy underscoring alleged universal laws arrived at by deductive introspection (Koot 1977; Coats 1954).

In the article 'The Economic Movement in England' (Foxwell 1887), Foxwell largely took the side of the Historical School and presented a wide-ranging picture of the troubled state of economics in Britain at the time, claiming that the success of liberalism across the country had been motivated by the economic prosperity that had taken place after the repeal of the Corn Laws in 1846. The downfall of the competitive *laissez-faire* dogma, as Foxwell saw it, came with the economic depression of 1874 and the startling growth of monopolies and big corporations. Moreover, along with the progressive rise of labour as a significant political force, a renaissance in socialist literature and in Christian social doctrine was beginning, awakening people to the social catastrophe of poverty and the urgency to properly tackle it (*ibid.*).

Foxwell had already published the essay 'Irregularity of Employment and Fluctuations of Prices' (Foxwell 1886), where he pinned down the uncertainty of employment as the paramount evil afflicting the British working classes. The alleged smooth displacement of labour and capital from one activity to another during economic dislocations, as claimed by the supporters of *laissez-faire*, had been historically marked instead by real suffering and immeasurable waste in terms of both skills and wealth. To avoid the recurring misfortunes of free competition, observed Foxwell, it needed to be controlled as a force of nature. Otherwise, all attempts at welfare improvement would be doomed from their very start: 'The condition of a large mass of people is so precarious and the state of industry so unsettled that social reforms, which must be gradual and slow, have no time to mature; and the situation alters before the proper remedy can be applied' (*ibid.*: 21).

Foxwell put forward two broad kinds of state intervention conceived to address the ultimate causes of unemployment, that is, speculation and the periodic mismatches between supply and demand. The first type of measure, which he called organization, required public control of the market through decentralized agencies (trade boards, municipal authorities, and voluntary associations), along with some kind of modern guilds of trade to stabilize economic activity by means of production quotas, quality control, and the prospecting of new markets. In addition, a Bureau of Labour should be created

to assist working people during their productive life, while long-term wage contracts were to be expanded in number and, in times of crisis, public works implemented to fight off unemployment. The second form of intervention projected by Foxwell consisted of making known to the public the country's relevant economic data in order to curb speculative frenzies. This particular goal could be achieved by the state's regular compilation of statistics relative to aggregate production, national income, consumption, employment, general prices, and banking (Foxwell 1886: 70–96, 1888 [1919]).

Marshall, Surplus and the Short- and Long-run Aspects of Welfare

Alfred Marshall's nearly lifelong commitment to economics grew out of his concern with the social problems of his time, the most conspicuous one being the resilience of poverty amid modern advances in wealth and productivity, as pointed out in the opening pages of *Principles of Economics* (Marshall 1890 [1920a]: 1–4; see also Whitaker 1975; Pigou 1956: 82–83). The central analytical idea in Marshall's pure theory of welfare lies in his general concept of surplus, which accrues to both consumers and producers. The first category, consumer's surplus, comprised the excess of total utility or satisfaction afforded by a commodity over the real value of what the individual paid for it. The second one, producer's surplus, had an analogous meaning, being defined as the accumulated excess of utility provided by the infra-marginal units of labour or saving over their respective reward, be that in the form of wages or interest (Marshall 1890 [1920a]: 830–831).¹

Setting aside the complex difficulties associated with estimating surpluses (see, however, Groenewegen (2010: 26–32) and Myint (1948: 142–172)), the idea in its simplest form allowed Marshall to qualify the doctrine of maximum satisfaction associated with a position of equilibrium between demand and supply. While he recognizes the existence of gains from trade between two individuals, and that this may be reflected in surplus analysis, Marshall argues that this does not mean that satisfaction is always maximized in the aggregate by free competition. First, it ignores questions of distribution of wealth by treating the welfare implications of a dollar in the hands of the rich as the same as a dollar in the hands of the poor, a proposition which Marshall

¹“The excess of the price which he would be willing to pay rather than go without the thing, over that which he actually does pay, is the economic measure of this surplus satisfaction. It may be called consumer's surplus” (Marshall 1890 [1920a]: 124). For the historical reception and theoretical implications of this particular proposal, see Groenewegen (2010), Dooley (1983), and Myint (1948: 142–172).

rejected. Second, and most important to him, for those commodities experiencing increasing returns, it may be possible to improve welfare by expanding output beyond the amount obtained under free competition. This particular policy may be facilitated by consumers transferring some of their surplus to producers to fully offset the reduction in producers' surplus that the expansion in output causes (Marshall 1890 [1920a]: 472–473).

In addition to his 'surplus'-based theory of welfare economics, Marshall also developed a broader and peculiar approach in his interpretation of social problems which, for the sake of clarity, can be split into two distinct lines of reasoning. The first one may be called practical, or involving short-term considerations, while the second one has a strong historical leaning, when he considers the subject from a long-run perspective. It is better here though to first recapitulate Marshall's views about the evolution of Britain's economy in modern times and, after that, to look into what he understood as feasible in the short run to remedy certain long-run trends affecting social welfare.

For Marshall, competition could be destructive or creative. Historically, he said, its negative aspects had been most noticeable during the initial phases of the Industrial Revolution. That had been the case because the first entrepreneurs were harsh and often ignorant, focused just on running their gloomy factories with the sole purpose of maximum profit. The workingmen, for their part, had to face ruthless competition for extenuating job posts without the protection of any kind of association. Moreover, the 1795 Act granting outdoor relief according to the price of bread and the number of children resulted in a rapid expansion in the poorest segment of the population, in turn resulting in a fall in wages. After the Napoleonic Wars, the Corn Laws and the action of the law of diminishing returns on the soil prevented a more steadfast advance of the British economy. The Factory Acts and the repeal of the Corn Laws in the middle of the nineteenth century, however, opened up a new phase of development for England's industry and her people (Marshall 1890 [1920a]: 1–48, 1969).

During the nineteenth century, Marshall explained, the remarkable progress in transportation, mechanization, and knowledge proved to be the basic force propelling the long-run rise in the national dividend. This process allowed a continuous cheapening of staple foods, fuel, communications, and transportation that enhanced the living condition of all ranks of society and most strikingly of the poor.² The eventual success of material prosperity in permanently

²The main drift of this study of Distribution then suggests that the social and economic forces already at work are changing the distribution of wealth for the better: that they are persistent and increasing in strength; and that their influence is for the greater part cumulative' (Marshall 1890 [1920a]: 712).

eradicating poverty, however, would require a new type of behaviour from capitalists and workers in order to modify the structural conditions of the demand and supply of labour. More to the point, the well-to-do would have to cut their consumption of superfluities in order to save more and keep the demand for labour unabated or directly help the most needy through taxes or acts of economic chivalry. The poor would have to postpone marriage to reduce procreation in order to provide comfort and guidance to their offspring so that these children could become well-remunerated skilled workers in the future (Marshall 1873, 1890 [1920a]: 220–236, 1907).³

Since the process of moral reconstruction would take time, Marshall supported a series of short-run initiatives which he saw as helpful in restraining poverty. First, the expansion of trade unions was seen by him as a sure welfare-enhancing phenomenon for the protection they offered to the labourer against the employers' bargaining power and also for establishing standard wages across many trades. Cooperative enterprises, particularly in the marketing of staple foods, were deemed by him as another form of association beneficial for the workers. Secondly, a system of universal education with open playing spaces was imperative in order to grant all youngsters not just knowledge and a healthy upbringing but essentially that strength of character required to make them responsible parents in maturity. Thirdly, the state should intervene to stipulate housing regulations and to provide aid in medical and sanitary matters for the poor. Furthermore, urgent measures needed to be taken to protect children from the poorest segment of society, the *residuum*, guaranteeing them a dignified future and thus putting a definitive end to the perpetual cycle of poverty (Marshall 1873, 1890 [1920a]: 689–722, 1969; see also Elliott 1990; Reisman 1990; Jensen 1987).

Although convinced of the Benthamite principle that an additional unit of wealth added more to the happiness of the poor than to the rich, Marshall defended income redistribution by a quite different route. On the one hand, a substantial part of the consumption by well-off people was spent on articles of fashionable luxury which they could easily do away without sacrificing their standard of living or even their savings. On the other hand, Marshall saw a well-fed and highly educated population as a key condition to the growth of wealth, considering such an approach as the best investment a nation could realize with its capital (Marshall 1873, 1890 [1920a]: 220–236, 1926). At this point, his long- and short-run factors of welfare improvement come together. The material progress in the living standard of the lower classes, associated

³As expressed by Groenewegen: 'Self-help, encouraged if necessary with initial assistance, was the main thrust of Marshall's views on social welfare and poverty' (Groenewegen 2010: 37).

with the provision of an adequate system of education, would enhance society's productive power and, therefore, reinforce the long-run trend towards a richer and morally stronger nation, rendering unnecessary in the future any schemes for social enhancement (Marshall 1890 [1920a]: 689–722, 1926: 197–262). This idea is further articulated in the *Principles*, when Marshall reflects on the true sources of wealth:

The older economists took too little account of the fact that human faculties are as important a means of production as any other kind of capital; and we may conclude, in opposition to them, that any change in the distribution of wealth which gives more to the wage receivers and less to the capitalists is likely, other things being equal, to hasten the increase of material production (Marshall 1890 [1920a]: 229).

John Neville Keynes on Theory in Relation to Concrete Economic Problems

Inspired by the controversy about the true nature of economics from the last decades of the nineteenth century in England, John Neville Keynes published *The Scope and Method of Political Economy* in 1891. The book does not touch on substantive issues in economic welfare as such, but it does address the subject rather indirectly, from a conceptual perspective as it relates to the broader question of the scope of economic thought relative to other social sciences. Neville Keynes's main concern was to establish a sort of compromise among the competing visions of economics at the time by asserting that much of the ongoing disputes had grown out of misunderstandings about the dual role of the deductive and historical methods in economic inquiry. He considered that the appropriateness of each particular approach depended on the material available, the stage of the investigation, and the objective in view. With this in mind, Neville Keynes split the scope of political economy into three major methodological categories, namely: (1) a positive body of deductive knowledge about the general laws of economic action or what *is*; (2) a normative or regulative science, dealing with the criteria of ideal conditions or what *ought* to be; and (3) economics as an art, or the assessment of specific means to achieve pre-established ends.

Although insisting on the idea that each one of these fields of study within political economy could be pursued in isolation, Neville Keynes agreed that a strict separation among abstract, ethical, and moral considerations would not be practicable in many situations. Besides, the acceptance of positive economics

would not invalidate the contention that it should be often accompanied by normative qualifications. Or, in his own words:

It may be pointed out how enormous is the influence exerted upon the well-being of mankind by the modes in which wealth is produced and distributed; and stress may be laid upon the fact that those human activities, which constitute the subject-matter of the economist's investigations, have an ethical significance, which is at least as worthy of consideration as their economic significance (Neville Keynes 1891 [1897]: 46).

On matters such as these, Neville Keynes reveals a strong intellectual link with Sidgwick (Backhouse 2006: 35). As regards the art of economics, which may concern the welfare implications of public policy, Neville Keynes justifies its validity by drawing on Francis Bacon's dichotomy between fruit and light by noting that economists have a characteristic disposition to pursue not just light-bearing science but fruit-bearing science as well by directing their studies to useful channels (Neville Keynes 1891 [1897]: 51). A generation later, Pigou followed Neville Keynes by also adopting the distinction between fruit- and light-bearing sciences (Pigou 1932: 3–5). Once the discussion enters this particular terrain, though, where explicit initiatives are to be evaluated according to their effectiveness in achieving a desired state of the world, economic forces are no longer supreme and ethical, social, and political concerns come also to the forefront of the argument. No solution to concrete economic problems, therefore, could be taken as complete without incorporating ethical and other reflections. If this exclusion had been common in the past, warned Neville Keynes, it was no longer held as good practice by contemporary economists (Neville Keynes 1891 [1897]: 55–63).

3 Early Twentieth Century

The first point to recognize is that twentieth-century developments in the Cambridge approach to welfare built upon the foundations that were laid in the nineteenth century by Sidgwick, Foxwell, Marshall, and Neville Keynes. The influence of Sidgwick and Marshall is common knowledge, but Foxwell's interest in fluctuations in employment and prices as well as Neville Keynes's distinction between 'truth' and 'light' and his associated articulation of the difference between the science and art of economics are all too evident in twentieth-century developments.

Pigouvian Market Failure and Welfare Analysis

Arthur Cecil Pigou first addressed welfare issues related to the notion of utility with reference to Marshall's concept of surplus, discussed in his 'Some Remarks on Utility' (Pigou 1903), where it is argued that an individual's scale of utilities could only be assumed as constant if all other consumers keep unaltered their spending on the same commodity, and in 'Producers' and Consumers' Surplus' (Pigou 1910 [2002]), in which the technical apparatus for the analysis of public and social costs and benefits is for the first time clearly articulated. These ideas, and others that emerged under the influence of Sidgwick (see O'Donnell 1979), found their way into Pigou's first major contribution to welfare economics, *Wealth and Welfare*, originally published in 1912 and later transformed and extended as *The Economics of Welfare* (Pigou 1932), which went through four editions in his lifetime (Collard 2014; McLure 2012). Like Marshall, Pigou was mainly concerned with the narrower and material part of welfare where money was the measuring rod of utility, as this provided a clear basis for careful deliberation about the role of public agents in correcting market failures. According to this approach, Pigou advanced three fundamental criteria to further economic welfare: (1) an increase in the national dividend without reducing the absolute share of each group; (2) a reduction in income inequality, everything else remaining unaltered; and (3) a decrease in the variability of the national dividend, especially in that part accruing to poorer families (Pigou 1912: 20–32).⁴

As regards the first criterion, Pigou's focus was on the national dividend, a notion that derived from Marshall but was clarified by Pigou after reflecting on Irving Fisher's views on the issue. However, it is sometimes seen as indicative of a certain distance between Pigou and Marshall emerging because the focus on variation in national dividend allowed Pigou to largely set aside Marshall's surplus analysis.

In Pigou's view, an increase in the national dividend would not, a priori, have a detrimental effect on the share accruing to labour compared to non-labour categories of income, so that total utility would most likely increase as a consequence of an expansion in the national dividend. The reasons he presented in support of this belief were threefold. First, an increase in the capital stock ('waiting'), requisite for the growth in wealth, under the constancy of technical conditions, would diminish the reward of capital with respect to labour. Second, in the presence of

⁴ Pigou's third criterion of welfare was dropped in subsequent editions of the book but received a comprehensive treatment in *Industrial Fluctuations* (Pigou 1927), where businessmen's errors of forecast due to alternating waves of optimism and pessimism were posited as the main cause of industrial cycles.

a gain in productivity due to some innovation, the most unfavourable scenario would be the one of labour-saving devices. Even in this case, in spite of its negative short-run effect on wages, the parallel increase in non-labour income would occasion a faster accumulation of capital in the long run, bringing down profits and restoring the distributive balance in the economy. Lastly, Pigou held that since most innovations affect goods consumed by working families, this continuous process would end up increasing their real earnings (Pigou 1912: 78–95).

As a general rule, Pigou indicated that the pursuit of self-interest would lead to the highest possible national dividend when net private returns attained a common level across economic sectors. This general conclusion, however, is significantly altered when the marginal social net product resulting from the action of private agents, rather than the marginal private net product, is given its due weight in a world dealing with welfare propositions. This fundamental concept within Pigou's theory of welfare concerns the aggregate contribution to the national dividend by the addition of a unit of capital in some line of production, account being taken of all the effects of the expanded activity on other members of the community besides the owners of the invested resources. These resulting effects comprise what the literature has designated as externalities, which is considered to be the 'single most enduring contribution' of *Wealth and Welfare* to the analysis of the deficiencies affecting a competitive economy (McLure 2012: 103). Pigou's own definition, formulated in *The Economics of Welfare*, reads as follows:

The marginal social net product is the total net product of physical things or objective services due to the marginal increment of resources in any given use or place, no matter to whom any part of this product may accrue. It might happen, for example, as will be explained more fully in a later chapter, that costs are thrown upon people not directly concerned, through, say, uncompensated damage done to surrounding woods by sparks from railway engines (Pigou 1932: 134–135).

In some instances, as Pigou explained, a discrepancy between these two economic measures might demand some kind of state intervention to constrain or stop the unchecked action of self-interest in order to advance society's material welfare. Situations like these could arise in certain types of long-term contracts as, for instance, when the farmer, before returning land to its owner, takes out from it more capital than he actually invested. Also, discrepancies between private and social net returns involving third parties would require some form of public action, generally by favouring or punishing particular forms of investment. Taxes also might be used to discourage

building in crowded areas or bounties granted to agriculture in order to keep families in the countryside. Auctioning licences to conduct public services could be a convenient way, advised Pigou, of handling natural monopolies once periodic revision of regulations were specified in order to adjust them to changing market conditions (Pigou 1912: 246–289). Aslanbeigui and Oakes (2012), though, underline the point that care must be taken when generalizing Pigou’s position on the application of his welfare theory, including the notion of externalities derived from the distinction between private and social costs, to public policy problems. The various editions of *The Economics of Welfare* raise many complications with regard to policy options, including the possibility of entering into private contract arrangements and, as a result, the appropriate welfare-enhancing policy responses should be arrived at on a case-by-case basis.

Moving to the second criterion of welfare, Pigou drew on the law of diminishing utility, predicated on interpersonal comparisons of utilities, and the declining propensity to consume, to assert that income redistribution favouring the poor would in fact increase the well-being of society. In the presence of these conditions, as he stated, ‘it is evident that any transference of income from a relatively rich man to a relatively poor man of similar temperament, since it enables more intense wants to be satisfied at the expense of less intense wants, must increase the aggregate sum of satisfaction’ (Pigou 1932: 89). In terms of practical policies, Pigou investigated the pros and cons of a system of mutual insurance against sickness, unemployment and other hazards, redistribution of the national dividend from the rich to the poor via wage adjustments and taxation, and, lastly, the establishment of a national minimum that provides for accommodation, food, sanitary convenience, health, and even some leisure (Pigou 1912: 346–398).

In the 1930s, Pigou’s approach to welfare economics endured a serious assault on its very foundations by Lionel Robbins (1932) and John Hicks (1939), who deemed unscientific and meaningless efforts at comparing the utility of different individuals (see also Aslanbeigui 1990).⁵ Impervious to these criticisms, Pigou held to his convictions. In his later years, he replied that such a procedure was crucial to economics, since this assumption made it possible to devise concerted actions designed to improve the welfare of nations: ‘In all practical affairs we act on that supposition. We cannot prove that it is true. But we do not need to do so ... The burden is the other way. To

⁵When reassessing the status of welfare economics in the 1970s, Hicks (1975) retracted his former position, accepting that useful theory must be normative, as Pigou had always maintained.

deny this is to wreck, not merely Welfare Economics, but the whole apparatus of practical thought' (Pigou 1951: 292).

After the Second World War, Pigou's welfare theory was the target of another serious, and largely successful, challenge by Ronald Coase (1960), who argued that instead of taxing the causative agent in situations of harmful collateral effects from private actions, the parties involved would be able to settle their differences in monetary terms when property rights were well defined and transactions costs low (see also Medema 2009: 101–124).

John Maynard Keynes and Welfare

John Maynard Keynes's *General Theory of Employment, Interest and Money*, and the role played by the notions of liquidity and effective demand in equilibrating the aggregate economy, has been so exhaustively discussed in the economic literature that little needs to be said here. However, it should be noted that the late nineteenth-century Cambridge contributions to welfare economics, and indeed Pigou's (1912) association of reductions in industrial fluctuations with a gain in economic welfare, make it abundantly clear that Keynes contributed significantly to the development of Cambridge welfare economics.

In brief, Keynes held that one general feature of modern economies was that the marginal aggregate propensity to consume is positive but less than unity. Consequently, an increase in income is associated with a smaller increase in expenditure on consumption. The resulting gap in effective demand might, or might not, be filled by investment spending. Outlays on new capital equipment, in turn, would be determined by the interaction between the scale of the marginal efficiency of capital and the prevailing interest rate, the former depending on the state of long-term expectations and the latter on the state of liquidity preference and the supply of money. The final result of the mutual interplay among these factors could well be a situation of equilibrium that may be below full employment. From a dynamic point of view, the Keynesian theory admitted that aggregate output and the employment level would be subject to an intrinsic volatility, with wide fluctuations in investment magnified by the action of the multiplier, the magnitude of which was inversely related to the marginal propensity to consume (Keynes 1973a: 109–123).

To the extent that Cambridge economics followed Neville Keynes and Pigou in considering economic theory more valuable for the fruit that it generates than the light that it sheds, Maynard Keynes's work is particularly significant for its ready application to public policy. In this regard, Keynes's

nexus between public expenditure, the multiplier, effective demand, and the full employment level of output had direct short-period relevance to welfare-enhancing policy in terms of Pigou's first criterion (increase in the national dividend) and third criterion (decrease in the variability of the national dividend) for economic welfare enhancement. But Keynes's work had relevance for Pigou's second (reduction in income inequality) criterion too, as, in the last chapter of *The General Theory*, Keynes referred to the unfair distribution of wealth and income as one of the outstanding faults of modern societies.

In Keynes's theoretical framework, any initiative designed to stimulate saving during times of less than full employment will only discourage investment expenditure by reducing consumption. So, policies to support the redistribution of incomes, such as progressive taxation, may favour investment in capital formation in the short period through an associated increase in consumption. Furthermore, the increase in the multiplier resulting from a policy of income redistribution to the poor would lessen the amount of investment necessary to achieve a position of full employment. Notwithstanding this, Keynes believed that some moderate income inequality was still essential since money-making activities were required not only to assure the continuity of the capital accumulation process but also to keep man away from reckless and cruel forms of self-aggrandizement which could be dangerous to society (Keynes 1936 [1973]: 372–384, 1973b: 128–131, 161).

In a radio talk on poverty in 1934, Keynes stressed that the safest course to enhance welfare in the long run consisted in promoting employment along with greater aggregate income. The best way to reach this ideal trajectory involved a combination of increased consumption with a parallel growth in investment. As the capital stock becomes more abundant, its reward would decline progressively to a level so low that profits would become an acceptable form of income to the whole society:

The right course is to get rid of the scarcity of the capital goods – which will rid us at the same time of most of the evils of capitalism – whilst also moving in the direction of increasing the share of income falling to those whose economic welfare will gain most by their having the chance to consume more (Keynes 1973a: 491).

For Keynes, then, a long-run policy towards full employment, with some redistribution of income and low interest rates, would be the surest way to eliminate the capital scarcity factor from profits, making this kind of gain an exclusive remuneration of individual effort and organizational skills. To get there, progressive taxation, easy money, and the socialization of a number

of investment possibilities would be the most appropriate mix of policies to bring the economic system to its maximum potential by avoiding waste and poverty through a constant state of full employment (Keynes 1936 [1973]: 372–384). As is well known, Keynes directly inspired many at Cambridge to follow in his path, and their work also has welfare implications. However, those contributions are beyond the scope of this particular chapter.

4 Mid-Twentieth Century

Dennis Robertson took the Chair of Political Economy at Cambridge following Pigou's retirement in 1943. Robertson had been a student of Marshall and Pigou and started his academic career by researching the nature of industrial fluctuations, particularly from the viewpoint of changes in supply conditions. He advanced an overinvestment theory of cycles which encompassed not only large-scale innovations, crop variations, and the indivisibility of capital goods but also a lack of coordination among entrepreneurs with respect to their decisions to invest, a shortcoming aggravated by the volatility of expectations (Robertson 1915, 1926). Also, Robertson had a deep interest in monetary economics, particularly the determination of the interest rate, which he explained through a loanable funds theory. The actual cost of financial capital, to Robertson, reflected the productivity of capital goods and society's cash-holding habits on the side of the demand for money, while savings decisions and pure credit creation by the banks would provide the supply of liquid resources (Robertson 1934, 1936).

A Revival of the Marshallian Approach to Welfare

With respect to the theory of welfare, Robertson assumed the overtly Marshallian form of utility theory to defend Marshall's approach in the article 'On Sticking to One's Last' (Robertson 1949 [1954a]). In Robertson's opinion, the suppression of utility from the discussions on welfare theory, as recommended by Robbins and Hicks in the 1930s, had hampered economists' ability to provide useful advice on substantive questions affecting society. This regrettable situation resulted from economists being deprived of a simple and powerful analytical tool in the evaluation of how legislation or other forms of intervention would impact people's material well-being. Without the concept of utility, economists had embraced distinct and fancy criteria of social improvement which emboldened some to conceive unfeasible schemes of delivering Heaven on Earth. Instead, said Robertson, economists should

be humble and aim just at presenting a fair case to society, wishing their arguments were properly understood and knowing that a final decision on any matter regarding welfare would most likely take other factors into consideration (ibid.: 60–65).

In ‘Utility and All That’ (Robertson 1951 [1954b]), Robertson frontally attacked the so-called Hicks–Kaldor compensation principle of welfare optimality. According to this criterion, anchored in the Paretian tradition of thought, a change in income distribution could only be said to have increased welfare if those individuals whose situation had improved were able to monetarily compensate the ones whose condition had worsened and, notwithstanding this, still be left with some gain. The main flaw of this line of reasoning, argued Robertson, was the fact that any evaluation of what actually happens depends on what might happen, but most likely will not happen anyway. ‘Potential’ does not mean ‘actual’ and given this, the new income distribution under scrutiny would end up being assessed by an ethical standard foreign to economics. This limitation, however, does not affect the analysis based on the cardinal utility assumption, which allows the economist to go further away and reaffirm the Pigouvian conclusion that a less unequal income distribution increases overall welfare (ibid.: 30–41). Formal elegance, advised Robertson, was no substitute for common sense when it comes to social issues: ‘But when we reach these regions of ultimate decision, I find it difficult to believe that mathematics has much to say that is not said more briefly in that aspiration of the book of Common Prayer – that we may have a right judgement in all things’ (ibid.: 41).

As Robertson reaffirmed the validity of employing the notion of cardinal utility in welfare economics, he pointed out as well a possible inconsistency in Pigou’s approach to the subject. In *Wealth and Welfare*, it is said that the growth in the national dividend brings forward an increase in welfare whenever it does not reduce the share of the national dividend accruing to the poor. Robertson, however, warned that if distribution is affected by the overall performance of the economy, it may happen that a more equal distribution in income could be accompanied by a contraction in the national dividend, leaving the final effect on welfare undetermined (ibid.: 30–37; Boianovsky 2014).

Finally, in the article ‘Utility and All What?’ (Robertson 1954), Robertson kept pointing out that the neglect of cardinal utility had critically weakened welfare economics. This time he was critical of Robbins. Economists, said Robertson, can and should provide valuable advice about the effects of economic changes on human happiness, particularly on those policies which impact income distribution. To omit oneself from such a noble duty, as suggested by Robbins, was actually a defeatist position which lowered

the standing of economics within society. Additionally, this passive attitude left the field clear for others to offer advice on crucial matters involving the application of scarce resources among alternative uses, a problem which Robbins himself had taught was at the core of economic science (*ibid.*).

5 Conclusion

Cambridge welfare theories represented a fundamental step forward in the development and professionalization of economics in Britain. First, this body of work enlarged the scope of economic analysis to encompass a whole range of issues which until the last quarter of the nineteenth century had been largely neglected or treated on a non-systematic, often ad hoc, basis. Such a change in attitude required not only acknowledging the existence of recurrent flaws in a market-structured economy, but also the idea that state-conducted action could fare better in correcting these issues than private interests operating in isolation. This new mindset about the nature of a market economy, however, had to be analytically constructed, and this was exactly the void that Cambridge theories of welfare were intended to fill. Of course, this common enterprise did not materialize through an express uniformity of methods when dealing with the multiplicity of troubles being addressed. Nevertheless, and here is the second important aspect of the matter, the development of a specific field of inquiry on welfare theory meant that the quest for an autonomous economic body of knowledge would not, as Marshall and Pigou relentlessly fought against, prioritize strict abstraction ahead of the development of a body of theory designed to contribute to the betterment of society.

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4

Cambridge and Development Economics

Shachi Amdekar and Ajit Singh

1 Introduction: Classical Political Economy and the Early Influences on Development Economics

In painting a backdrop for the emergence of ideas related to development economics, it is useful to briefly consider pre-classical and classical notions of human progress, though these will not be explored in depth in this chapter. The ‘developmentalist’ tradition, as Ha-Joon Chang (2014) describes, is not a school of thought per se; it was no such unified movement with core ideas, defined founders, or any specific population of following. Rather it was ‘dispersed, with multiple sources of inspiration and with a complicated intellectual lineage’ (ibid.: 113), including physiocratic ideology. Debate and discussion on moral philosophy was at its height during the seventeenth and eighteenth centuries in an enlightened Western Europe,

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and the influence of science and reason over a staunchly Catholic society was paving the way for increasingly self-determined possibilities for *superlative, infinite, and perfect* human progress. The doctrine of human perfectibility was an ideal since Antiquity and hugely influenced by the Aristotelian–Augustinian heritage (a conflicting, often chaotic set of ideas of infinite human progress and the inevitable Armageddon). Nevertheless, French, English, and German moral philosophers of the eighteenth and nineteenth centuries were inspired by the movement in emphasis from Catholic, obedientary perfectibility (perfect submission to a deity) towards technical perfectibility (maximum efficiency in specialised tasks) and teleological perfectibility (perfect attainment, thus creating perfect human satisfaction) (Passmore 1969).¹

In 1794, with the publication of his *Sketch for a Historical Picture of the Progress of the Human Mind*, Condorcet (like the British theorist, William Godwin, both of whom Malthus famously rebuts in his *An Essay on the Principle of Population* (1798)) made open and unfettered claims for mankind's absolute perfectibility.² However, a more pessimistic (Malthusian or 'dismal') tradition of developmentalist ideas came in the form of Genevan philosopher Jean-Jacques Rousseau (1712–1778),³ whose early discourses are concerned with inequality and whether this phenomenon arises under 'natural laws' (Rousseau 1755 [1992]). David Hume came to share Rousseau's views on the limits of natural progress and perfectibility. Together they sowed the seeds of what was to become the 'Order of Progress', emphasising the necessity of technology, science, and social organisation in manipulating nature to attain something *better* or *optimal* if not akin to perfectibility.

Though not a Cambridge man, another important later thinker who greatly influenced the analysis of economic, political, and social phenomena was Karl

¹ For instance, Fontenelle found influence upon the publication of his *Digression sur les Anciens et les Modernes* (1688) in which he highlighted the controversial possibility of infinite *growth*, by the accumulation of intergenerational knowledge of mankind, and the ability, therefore, to amass human capital. In publishing *Théodicée* (1710), Leibniz was noted for his idea of universal perfectibility and plenitude within a pre-established divine equilibrium of harmony. The work of the likes of Buffon took a decidedly Western, and immediately colonial view of human progress, creating a 'white man's burden' in calling the European the pioneer of civilisation and superiority.

² Condorcet refers to human well-being as a function of the problems of seasonal subsistence and the limited 'means of satisfying his wants' (Condorcet 1794 [1955]), although he divides progress into ten epochs of technological history which permit adjustment to the prevailing ideas and preserve equal and perfect growth (Rist 2008: 3). In his work *Political Justice* (1793), Godwin asserted that a thriving human population measures perfect conditions, citing intellectual capacities as a distraction from reproductive capacities when facing population pressures.

³ Rousseau spent some years in exile in Staffordshire, under the protection of Scottish philosopher David Hume (in turn a close companion of Daniel Malthus, father of Thomas Robert).

Marx. Marx's work, which combined early ideas on social progress in classical political economy with the evolution of capitalism, has been an important influence on the Cambridge School over the years. This was particularly the case with respect to the Cambridge controversies in capital theory in the 1950s to the 1970s (Cohen and Harcourt 2003). Marx tied together the idea of perfecting human society and inclusive material well-being with class structures endogenously formed within society itself. He describes this phenomenon in the following terms: 'Capital grows in one place to a huge mass in a single hand, because it has in another place been lost by many. This is centralisation proper, as distinct from accumulation and concentration' (Marx 1867: 586). Marx also observed in relation to the forces of competition that these 'act as external coercive laws for the individual capitalist, which compel him to keep his capital to preserve it. But he cannot, except by progressive accumulation' (ibid.: 555). Hence the basic law of capitalism is described, 'accumulate accumulate, that is Moses and the Prophets' (ibid.: 652). Within the Cambridge controversies over capital growth theory, equilibrium theory, and ideology in modelling, the Cambridge School cited Marxian influences. For instance, the meaning of capital—and as a corollary the means of measuring its accumulation—was argued by the English 'Cantabrigians' to lie in the analysis of capitalist classes.

It is against this backdrop—this swarm of interlocking ideas on the prospect of mankind's progress, future perfectibility, and measurement of this perfectibility—that we trace how Cambridge theorists influenced ideas on the nature of development economics. In this light, we might consider development economics from its very foundations in the doctrine of human perfectibility as an instinctively appealing aspect of what was to become political economy. This chapter is, in some respects, a prequel of the conventional post-war narrative on development economics, as it is commonly known today. It cannot claim to include all those Cambridge men and women who contributed to the evolution of development economics. However, it focuses specifically on how this story drew in Cambridge academics and how this influenced the origin and elaboration of some of the defining motivations, methods, and ideas of development economics in early political economy. We follow the Malthusian trail of multidisciplinary thinking from its informal origins and motivations in responding to Christian doctrines of human perfectibility (the idea that human society can be improved and perfected) and post-Enlightenment ideas of optimising (not perfecting) human welfare. We go on to describe the Marshallian ideal of economics as a subject for the betterment of social ills, and how this paved the way for the eventual establishment of the profession of economics at Cambridge. This approach

depicts a positive and rich contribution from the Cambridge tradition to the evolution of development economics from traditional ideas concerning the *economic, sociocultural, and political study of human progress*.⁴

2 Paley and Malthus: The First of the Cambridge Development Economists

We begin with the observation that notions of human *perfectibility* are today immediately associated as the impetus for the work of Malthus, who Keynes famously regarded as ‘the first of the Cambridge economists’ (Keynes 1933 [1972]: 78). This was not idle praise. Malthus made some of the earliest major contributions to the subject of development economics in terms of both his ideas and methodology, and also to many other areas of economics. Malthus’s influence was vast; he was a long-time personal friend of Ricardo. Their respect for each other’s work was mutual, although they did not always agree with each other. The work for which Malthus is most famous is not considered rigorous in its methodology and argument by today’s standards of data availability, but it indicates some of the earliest high-quality scholarship in development economics and provided many real-world applications to issues in the field. Perhaps more importantly, his work in political economy was the impetus for contemporary theories in fields related to long-term economic sustainability, including demography and statistics, poverty, welfare, agriculture, macroeconomic policy, environmental economics, and evolutionary biology.

Keynesian historiography of economic thought ties together Malthus and William Paley in that each could claim to have been the first Cambridge economist—at least ‘in a sense’ (ibid.: 79, fn. 2). The intellectual climate in Cambridge around the late eighteenth century favoured theological and philosophical ideas, which formed the basis of an emerging subject, political economy. Waterman (1996) remarks upon the rather limited early involvement of the Cambridge tradition in this emerging field. He reports little evidence from Cambridge University and college library records that there was any substantial interest in political economy (including Smith’s *Wealth of Nations* 1776) until Malthus’s *Essay* (1798) brought the subject into the spotlight with his provocative message asserting the ominous imperfectability

⁴Including all abstract ideals of material well-being, progress, economic growth, social justice, personal growth, ecological equilibrium, capabilities, dignity and fulfilment, political independence, democracy and political participation, and opportunity (Sen 1999).

of mankind. In examining their respective influences on the development economics discourse, however, it is useful to consider Malthus and his predecessor Paley side by side. On the one hand, this is because they were amongst the first to speak of and *popularise* economic ideas in economic language in Cambridge and in Britain generally.⁵ On the other hand and most significantly, they personally influenced one another a great deal. As such they might rather be considered the first of the Cambridge *development economists*.

Malthus's earliest influences were significant and entrenched in the Church of England, utilitarianism, and liberal thought and pacifism. He arrived as a student at Jesus College, Cambridge, in Michaelmas Term 1784, at a time when 'the University was just stirring from a long sleep, and Jesus, which had been among the sleepest, was becoming a centre of intellectual ferment. Malthus probably owes as much to the intellectual company he kept during his years at Jesus as to the influence and sympathy of his father' (Keynes 1933 [1972]: 79). Indeed, Daniel Malthus, whose own family hailed from Cambridgeshire, took a great deal of interest in the education of his precocious son, and tutored him himself along with Richard Graves⁶ and Gilbert Wakefield.⁷ Moreover, as mentioned above, Daniel Malthus, who was a gentleman of means, was a personal friend of both Hume and (when he was in England) Rousseau. Moving within the same circles, it may be presumed that there was some spillover of ideas and academic influence on the younger Malthus. While at Jesus, Malthus praised his new environment of scholarship and intellectual curiosity, noting in a letter home: 'I think it seems rather the fashion to read' (Malthus quoted in *ibid.*: 78). His peers included the likes of natural historian E.D. Clarke, while Coleridge arrived some years later. The work of Paley had a vast influence over Malthus due to the still-prevailing tutorial systems at Cambridge, which promote a culture of intellectual discipleship. Malthus's Tutor was the social reformer, William Frend, who had been taught by Paley himself. Moreover, as Keynes observes in his *Essays on Biography* (Keynes 1933 [1972]), Paley's *The Principles of Moral and Political Philosophy* was published in 1785, Malthus's first year at Cambridge and is likely therefore to have been a significant intellectual stimulus for him.

⁵ Though having highlighted problems in political economy as a key area of intellectual debate, it was Smith who solidified this interest and garnered attention throughout Britain.

⁶ The author of *The Spiritual Quixote*, a satirical work on Methodists.

⁷ A heretical clergyman and a Fellow at Jesus College, Wakefield was famously arrested in 1799 for 'expressing a wish that French revolutionaries would invade and conquer England' (Keynes 1933 [1972]: 78).

Morality, in the sense of responsibility and duty in society, is a common theme running through the work of both Paley and Malthus⁸—a theme which continues in the work of contemporary development economists including Amartya Sen, Thomas Piketty, and Ha-Joon Chang. Paley's *Principles* can be interpreted as a treatise on the moral obligations and rights of the liberal individual to civil government, the constitution, the administration of justice, and the establishment of the Church of England. In Paley's work, this aspect of principled social cohesion is tied to the concept of human betterment and progress. Its chapters refer to now-familiar themes in development economics and international relations—'Population and Provision' and 'War and Military Establishment', for example (Paley 1825). Waterman (1996) considers Paley's influential work to have been 'the origin of this characteristically "Cambridge" way of looking at economic reality, so sharply distinguished from "classical" thinking of either the nineteenth or the twentieth century' (ibid.: 674). The 'immortal' (Keynes 1933 [1972]: 79, fn. 2) tome itself is primarily an undergraduate textbook in moral science, which reflects the content of approximately one-third of the core curriculum taught at Cambridge during the eighteenth century. It was designed to pass the intellectual baton of studying human betterment, drawing students such as Malthus into an emerging and complex subject and defining political economy from the perspective of moral philosophy. Paley's work was to complement biblical and classical readings, and intended to 'form the minds and the moral sensibilities of clergymen, magistrates and legislators' (Waterman 1996: 674), the then-accepted institutions of human progress.

Malthus took an early interest in the relationship of demography with the problems of political economy. His first essay, *The Crisis, A View of the Recent Interesting State of Great Britain by a Friend to the Constitution* (1796), written when he was 30 years of age, indicates this interest which in turn could be traced back to Paley's 'Population and Provision' ideas. Extracts from this unpublished work reproduced in Keynes's *Essays on Biography* (Keynes 1933 [1972]) show Malthus comparing his thoughts with Paley.⁹ In 1798, Malthus published his magnum opus, *An Essay on the Principle of Population* (1798). In this text on the relationship between supply of subsistence and demand, Malthus argued that the demand for food increased at a geometric rate to meet the needs of an increasing population while the supply of food was only

⁸Who incidentally were both ordained ministers.

⁹'On the subject of population [Malthus wrote] I cannot agree with Archdeacon Paley, who says, that the quantity of happiness in any country is best measured by the number of people. Increasing population is the most certain possible sign of the happiness and prosperity of a state; but the actual population may only be a sign of the happiness that is past' (Malthus quoted in Keynes 1933 [1972]: 83).

able to grow at an arithmetic rate. The *Essay* was a responsive work, not only to Malthus's indirect intellectual influences in Cambridge and at home, but also directly to Godwin's¹⁰ doctrine of human perfectibility, which described a future age of perfect equality and human happiness. In conversation the latter was frequently defended by Daniel Malthus, which prompted Malthus's intellectual attack (Keynes 1933 [1972]: 84). Malthus's *Essay*, in the 'sheer immodesty of [its]...propositions' (Winch 1987: 3), hypothesised against perfectibility: improving living standards and society is *limited* by certain laws of nature whereby population—when unchecked by misery and vice (and in later editions of the *Essay* by moral restraint)—tends to equal or surpass existing means of subsistence. With such a pessimistic conclusion, this bestseller gave rise to huge controversy and enjoyed a certain notoriety, illuminating problems in political economy for the general public.

Malthus has several claims to being the first of the Cambridge *development economists*. He was a consummate scholar. He collected first-hand data on various aspects of population, gathered on his journeys to Sweden, Norway, Finland, and Russia—wherever British citizens were allowed to go before the Napoleonic Wars. Malthus was an empirical statistician, 'fascinated not with the inevitability of human demise but why humans do not die off in the face of such overwhelming odds' (EconLib 2008). His *Essay* was an instant success, strongly criticised by non-economists upon its first publication. However, so irrefutable and simple was his illustrative side-by-side comparison of the arithmetic and geometric series model that most people were immediately silenced by the quantitative comparison. Malthus's demographic data was so compelling that it was easy to lose sight of his main conclusion, which was that since the human race has not starved to death, economic choices must be at work and it is the duty of an economist to analyse these choices and search for optimality in these choices. Malthus examined a whole range of other issues in his textbook *Principles of Political Economy* in 1820. It was the first text to describe a demand schedule as distinct from a supply schedule, an innovation in which 'political philosophy gives way to political economy' (Keynes 1933 [1972]: 84). Although his knowledge of industry was not so developed, Malthus provided the first systematic explanation of a whole range of economic and political phenomena.

In the history of development economics, Malthus's contributions exceed those of a mere demographer. He used population theory to question the views of Godwin, Condorcet and the like—who collectively supported a general Romantic post-Enlightenment movement of a utopian egalitarianism that

¹⁰ Also born and raised in Cambridgeshire.

would happen under natural law. Malthus's empiricism succeeded in popularising the subject with his controversial bashing of these early theoretical, deductive, and idealistic conceptions of human progress. Paley was himself converted to Malthus's side despite having previously argued that population decay was a sociopolitical evil, while even politicians, including Pitt, took a deferential interest in Malthus alongside Bentham. Indeed, early nineteenth-century ideas that were centred on individual welfare provided favourable conditions for the Malthusian message, which, like Smith and Ricardo, began from a utilitarian position. For instance, a Malthusian preoccupation can be detected in ideas associated with a developing and expanding society—the self-defeating impact of welfare laws on available housing, the fate of the independent labourer given government intervention in labour markets, and the propensity for upward pressure on the population due to adverse incentivisation. Yet within utilitarian thinking, Malthus's *Essay* promoted universal *optimality* rather than perfection, which became an underlying supposition of development economics.

3 The Influence of Science: The Marginal Revolution and Biological Evolution

The classical debates following Malthus's *Essay* reflected how

[t]he voice of objective reason had been raised against a deep instinct which the evolutionary struggle had been implanting from the commencement of life; and man's mind, in the conscious pursuit of happiness, was daring *to demand the reins of government* from out of the hands of the unconscious urge for mere predominant survival (Keynes 1933 [1972]: 85; italics added).

This is to say, increasing importance was being assigned to the active role of the state, society, and individual in improving, catalysing, and above all *optimising* human progress. This attitude was furthered by revolutions in natural science and the gradual decline of religious influence over academic establishments in Britain—including Cambridge. The purpose of this section is to place post-Malthusian ideas of the quest for human perfection or optimality in the context of intellectual thought. Taking a Keynesian line, we sketch the differences in approach between Malthus and Ricardo and the impact of this divergence on development economics and its continuity from the past, while shedding light on how concurrent revelations in science and evolution theory fed into the intellectual debate in Cambridge about optimising human progress and development.

Smith, Malthus, and Ricardo wrote around the late eighteenth and early nineteenth centuries and are credited as the pioneering triumvirate of classical economics; together with Locke, Hume, Bentham, Darwin, and Mill, they are praised for their ‘immense disinterestedness and public spirit’ (ibid.: 86). However, in their methods the three classicists were disparate, which engendered separate trails of thought and distinct families of idea and methodology. Smith, as we have mentioned, was the star of this emerging subject. The Ricardian approach found popularity and became the foundation for the abstract and mathematical conceptualisation of economic ideas during his time. Conversely, the degree of interest in the Malthusian approach to economic thought has arguably only been significant since Keynes himself generated the impetus for it in his *Essays in Biography* and then later in his *General Theory* (see Corry 1959).

Keynes considered Malthus’s *Essay* to have been greatly influential on him and ‘profoundly in the English tradition of humane science’ (Keynes 1933 [1972]: 86). He refers to Malthus’s *correct* line of approach to practical economic problems as compared to Ricardo,¹¹ and praises the *Essay*’s continuity from the eighteenth- and nineteenth-century classical works on moral philosophy and the human perfectibility debate. Methodologically, Keynesian analysis favoured a return to this Malthusian approach for the purpose of short-term problem solving based on practical, real-world analysis. Keynes protests over how, in the simplification of a multilayered, abstract, and complex concept, the widely adopted Ricardian ‘pseudo-arithmetic doctrines’ (ibid.: 103) departed from the facts, while Malthusian analysis was neglected. Malthus, he contended, held more real significance in his intuitions and ‘by taking up the tale much nearer its conclusion...[Malthusian theory]... had a firmer hold on what may be expected to happen in the real world’ (ibid.). To illustrate this idea of Malthus’s applied line of thinking, we might consider Malthusian and Ricardian analyses of demand: Malthus remains public-spirited, anonymously publishing *An Investigation of the Cause of the Present High Price of Provisions* (1800), in which his emphasis is placed on the observed macroeconomic criterion of ‘effective demand’ as it exists in reality, while the Ricardian method focuses on the underlying factors of demand and builds up theoretical foundations brick by brick. This more scientific approach enjoyed a new appeal during this period and lent itself well to the microeconomic analysis that would become the neoclassical school, and

¹¹ As is well known, Malthus and Ricardo were intimate friends, and despite their differences in approach to economics, with one an inductive and intuitive investigator, the other an abstract and a priori theorist, the two had ‘the deepest affection and respect for one another. The contrasts between the intellectual gifts of the two were obvious and delightful’ (Keynes 1933 [1972]: 95).

particularly the marginalist revolution pioneered by Walras, Menger, and Jevons, and solidified by Marshall (see Section 4).

In a sense, we might support Corry's dispute with Keynes's somewhat simplistic lament: 'If only Malthus, instead of Ricardo, had been the parent stem from which nineteenth-century economics proceeded, what a much wiser and richer place the world would be to-day!' (Keynes 1933 [1972]: 100–101). One in lieu of the other would not do either, if continuity of past debates and intellectual traditions are to thrive. The *mutual* debate between Malthus and Ricardo and their readership was fruitful to the progress of political economy for the purpose of studying human betterment, and may indeed have been crucial to it. However, from the perspective of development economics at Cambridge, we might concede that the decline of multidisciplinary Malthusian approaches of a posteriori induction (catalysed by the influx of mathematisation and scientification in post-Ricardian thought) briefly quashed the continuous tradition of 'humane science' in the subject of political economy.¹² That a posteriori macroeconomic observations dwindled in the 100 years between Malthus and Keynes is regrettable, because the moral and political–economic debate on human perfectibility and universal optimal well-being of mankind did not make much economic progress during that period.

Yet that is not to say that the idea was forgotten altogether, and nor was the work of Malthus and Paley. During the early nineteenth century (though not of course in the sphere of neoclassical economics), movements in the natural sciences were making a huge impact in intellectual circles in Cambridge. The publication of Darwin's *On the Origin of Species* (1859) made Britain the epicentre of ideas again with the theory of evolution, partly influenced by and in response to the Aristotelian natural theology debate and the work of Malthus's population doctrine and Paley's *Evidences of Christianity* (1794) and *Natural Theology or Evidences of the Existence and Attributes of the Deity* (1802), in which adaptation was deemed the design of a deity through natural laws. This was followed by Herbert Spencer's *First Principles* (1862), which took a new direction in philosophical endeavours at Cambridge: the combination of metaphysical agnosticism, evolutionary progress, and utilitarian ethics (see Keynes 1933 [1972]: 169–170).

¹² More balance between the Malthusian and Ricardian approaches is better implied in quoting Keynes's view that 'One cannot rise from a perusal of [the correspondence between Malthus and Ricardo] without a feeling that the almost total obliteration of Malthus's line of approach and the complete domination of Ricardo's for a period of a hundred years has been a disaster to the progress of economics' (Keynes 1933 [1972]: 98).

Movements towards scientific ideas and away from theology, in this way, broadened the accumulation of ideas on human capabilities and progress. In the wake of these new ideas, the notion of a higher power to direct human perfectibility ebbed away and furthered the more assertive idea that human potential is in human hands that hold the tools of liberal government policy and social responsibility. It was around this time, writes Keynes (*ibid.*), that Marshall entered Cambridge and was inspired to institute new ideas in political economy based on evolution. For example, colonialism was increasingly becoming an area where ideas of both social and biological evolution met with the familiar features of class, inequality, and stagnation. The likes of Theodore Morison, a Cambridge man, who spent much of his life in India initially as a tutor to the young Maharajas, and whose expertise on the Indian economy was such that his *The Industrial Organisation of an Indian Province* (1906) remains on the Tripos reading lists. On another note, the lowering of humanity to the same level as the earthly environment was considered both a snub and a shockwave: for the first time man needed to consider himself part of an ecological equilibrium, awakening questions on sustainable human progress that even today remain woefully unanswered in development economics. Malthusian conceptualisations of ‘misery and vice’, for instance, need not be a divine phenomenon but rather an indication that biological evolution needs to be matched with social evolution; the progress of evolution theory forced the pledge that our institutions need to change with the times and both keep up with *and* initiate human progress.

4 The Multidisciplinary Pursuit of Optimality: Alfred Marshall and the Establishment of the Economics Profession

Alfred Marshall is today celebrated as an outstanding economist and moreover the founder of the Faculty of Economics and Politics at Cambridge University in 1903. Yet upon examining his life, Marshall stands out first as a classical polymath and then as an evolutionist, very much in line with modern thinking in development economics. Born in Bermondsey, London, Marshall was educated at Merchant Taylors’ School, and attended St John’s College, Cambridge, where he excelled in mathematics and in 1865 was Second Wrangler, being immediately elected to a Fellowship. He spent some time as a mathematical master at Clifton College, Bristol, under the headmastership of John Percival. This opened his social circles to include H.G. Dakyns, J.R. Mozley, and Henry

Sidgwick, who became his early influences in moral philosophy—with which Marshall was fascinated. A mathematician, evolutionist, and a budding philosopher with a growing interest in the hindering problems of social inequality and poverty, Marshall was armed with all the multidisciplinary talent and breadth of ideas that his predecessors had possessed.¹³

Marshall's interest in economics was born out of a fascination with utilitarian ethics, such that the solution of economic problems was something of a higher calling—an exercise for 'man's higher faculties' (Keynes 1933 [1972]: 170). Human progress in an equal and liberal manner was a crucial precondition for human happiness and opportunity, in Marshall's opinion. He writes: "The economist can claim...that "the study of the causes of poverty is the study of the causes of the degradation of a large part of mankind"" (Marshall 1890: 3–4). The resounding notion of *potential* and *optimism* runs through Marshall's justification for his move into political economy, this having some elements of Sen's later capabilities approach:

I had doubts as to the propriety of inequalities of *opportunity*, rather than of material comfort. Then, in my vacations I visited the poorest quarters of several cities and walked through one street after another, looking at the faces of the poorest people. Next, I resolved to make as thorough a study as I could of Political Economy (Marshall quoted in Keynes 1933 [1972]: 171; italics in original).

Marshall was a mathematical economist but he preferred to use geometrical exposition to express his ideas. In the tradition of his classical predecessors, Marshall took a multidisciplinary approach in his quest, combining the ideas borne of metaphysics, evolution theory, and psychology:

[F]ascinating inquiries into the possibilities of the higher and more rapid development of human faculties brought me in touch with the question: how far do the conditions of life of the British (and other) working classes generally suffice

¹³The paradox of the economist, as Keynes put it, is that economics is at least in relative terms, an easy subject, at which very few excel: "The paradox finds its explanation, perhaps, in that the master-economist must possess a rare *combination* of gifts. He must reach a high standard in several different directions and combine talents not often found together. He must be mathematician, historian, statesman, philosopher—in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood; as aloof and incorruptible as an artist, yet sometimes as near the earth as a politician" (Keynes 1933 [1972]: 173–174; italics in original).

for fullness of life? Older and wiser men told me that the resources of production do not suffice for affording the great body of people the leisure and opportunity for study; and they told me that I needed to study Political Economy (Marshall quoted in *ibid.*).

In terms of relating biological evolution with social evolution and the economic capabilities of mankind, Marshall was specific in his distinction between modern and early civilisations. He observed that life in the latter ‘is pervaded almost unconsciously by a few simple ideas which are interwoven in that pleasant harmony that gives their charm to Oriental carpets’ (Marshall 1890: 13). Nevertheless, like Darwinian evolution, so-called Marshallian Evolution—which demanded that ‘Economic evolution is gradual’ (*ibid.*: xiii)—became a key characteristic of Marshall’s analysis on early development economics. He considered that the ‘influence of economic causes is pressed below the surface. There they work surely and slowly’ (*ibid.*: 14).

Perhaps a result of the prevailing British Raj in India, Marshall’s venture into political economy concerned the happiness and well-being of an entirely foreign population.¹⁴ Indeed, his views on India appear to have been well developed and consistent with his assertion that economic evolution is gradual. He even lectured civil service trainees at the University of Oxford for some time (Tahir 1990: 37). For instance, in formalising a theory entrenched in the spirit of Marshallian Evolution and ideas about economic modernity, Marshall refers to the Indian labour market in making his argument. Specialised trades, as Marshall purported, were the glue that held the economy together. He asserts that a key indicator of a modern civilisation is to have division of labour within these specialised trades. Marshall writes:

Some sort of division of labour is indeed sure to grow up in any civilisation that has held together for a long while, however primitive its form. Even in very backward countries we find highly specialised trades; but we do not find the work within each trade so divided up that the planning and arrangement of the business, its management and its risks, are borne by one set of people, while the manual work required for it is done by hired labour. This form of division of labour is at once characteristic of the modern world generally, and of the English race in particular (Marshall 1890: 37).

At the same time, Marshall was of the belief that colonial rule would benefit the Indian economy—if it were to avoid rapid transformation and create

¹⁴ Marshall already had an interest in development issues as demonstrated by his 1875 tour of the USA to study protectionism (see McCready 1955: 260).

an administrative structure conducive to his well-defined vision of evolution. Resource transfer is not then the issue that Marshall would have highlighted. Indeed, neither protectionism nor industrialisation (particularly together) would be necessary for Marshallian Evolution, as Pigou highlighted (Pigou 1920: 458).

Finally, Marshall was a visionary in what he hoped the new Faculty of Economics and Politics at Cambridge would achieve, this in turn helping to pave the way for modern research in development economics. His emphasis was on the goal of betterment, of development, and to this, methodology was secondary. In the *Principles* (1890), Marshall contributed to the emerging neoclassical school. Yet despite his mathematical background, he was pragmatic about the use of mathematics in economics, noting his preference for diagrams over algebra in his review of Jevons's *Political Economy* in 1872 (Marshall 1872). He was much more deeply concerned with the need for historical context for economic ideas, and even praised radicals and socialists for the 'shrewd observations and pregnant suggestions' (Marshall quoted in Keynes 1933 [1972]: 196) which lay beneath their less feasible ideas, from which economists and philosophers might learn a great deal. To Marshall, maintaining an open mind about the subject of human betterment, and various ideologies surrounding and interpreting it, was crucial to how he taught the subject to the likes of Maynard Keynes. As Keynes wrote:

[T]he bare bones of economic theory are not worth much in themselves and do not carry one far in the direction of useful, practical conclusions. The whole point lies in applying them to the interpretation of current economic life. This requires a profound knowledge of the actual facts of industry and trade (ibid.).

5 The Marshallians: Keynes and the Circus

It is arguable whether Maynard Keynes was a development economist according to today's terminology. Indeed, as Toye (2006) observes, Keynes's premature death in 1946 meant that he barely witnessed the 'official' post-war birth of the subject as it is defined today. He was widely considered to be a monetary economist (Hicks 1983), not especially interested in growth theory or underdeveloped countries (Patinkin 1984). Nevertheless, it has been observed that "Keynesian economics" has a great deal to do with the emergence of development economics...[even if] Keynes himself was little concerned with the problem of economic development' (Johnson 1976: 15). Keynes's indi-

rect contributions to the formative years of the subject can in this sense be considered substantial, even ‘pervasive’ (Chakravarty 1997: 69). We can contextualise Keynes’s work within the setting of an increasingly chaotic British Empire prevailing during his time, along with the Great Depression. Keynes exerted a considerable influence on his disciples at Cambridge, most notably the Circus. Many of them directly contributed to ideas in political economy and development economics, including Joan and Austin Robinson, and James Meade. Keynes was also influenced by Marshall’s idealistic and optimistic outlook, as demonstrated in *Economic Possibilities for Our Grandchildren* (1930) and picked up the Malthusian reins in his non-mathematical macroeconomic approach to addressing economic issues affecting human well-being as they manifest in real life.

During Keynes’s lifetime, intellectual thought on the progress of humanity was focused not only on the problems of the day (including the Great Depression) in Western Europe, but also included the narrative of overseas expansion and New Imperialism. The Cambridge tradition, by this point under Marshall’s leadership, was contributing to the education of those who wished to work to improve human progress and economic well-being both at home *and overseas*. The history of economic thinking on human progress in political economy (which was by now poised to bore development economics as a separate subject) was chiefly concentrated on economic stagnation, colonial impact, and in particular the future course of economic development in India. The Cambridge outlook on colonialism was ‘benign’ (Tahir 1990: Summary) and was occupied with paving an industrialising path for India to help bring about modernisation and capitalism. Debates in the Faculty concerned aspects of dependency and imperialism, which continue even today. For instance, alongside Austin Robinson, Joan Robinson took part in a 1920s debate on resource transfer between British India and the Princely states, producing a report, *The British Crown and the Indian States* (1928). Other than the obvious personal influences of Marshall and Pigou upon Keynes as his Tutors, Tahir (1990: Paper 2) draws parallels between Keynes and Marshall along with Morison: the three had some personal connection with India, which informed their writings in different ways: As noted, Marshall lectured Indian civil service trainees at Oxford, Keynes worked briefly at the India Office in London, while Morison had actual experience of the Indian economy and society. Tahir remarks, ‘All of them took British India for India’ (ibid.: 37).

As he freely admitted, Keynes’s interest in India was chiefly monetary (Keynes 1913 [1971]). He never visited India (nor any other developing nation, apart from holidays to Morocco and Egypt), despite the fact that

he drew from his experiences in the administrative management of India as an underdeveloped colony. His innovative theories and rationale on the gold standard and central banking in *Indian Currency and Finance*, for instance, were inspired by the time he spent at the India Office (1906–1908) and his service to various government committees and commissions on Indian currency (Chandavarkar 1986). Skidelsky (1983) highlights that in the benign Cambridge tradition of analysing the colonies, Keynes took little interest in the ‘human and moral implications of imperial rule or whether the British were exploiting the Indians’ (ibid.: 176) and instead focused on the practical monetary policy problems facing British India.¹⁵ He presented the first demonstration that separate countries require separate institutional models depending on socioeconomic needs, rejecting the Bank of England as an insufficient model for what would be the State Bank of India, as Indian progress also required provision for direct credit, remittances, and rediscount facilities to promote industry and agriculture.

Optimistic insights are to be found in *Economics Possibilities for Our Grandchildren* (1930), in which Keynes highlights population, civil peace, emphasis on science and technology, and observing the margins of production and consumption as key factors in the progress of human society for the generations. However, this aside, Keynes’s direct intellectual influence on development economics was somewhat intangible and occurred more subtly through the Circus and others who applied Keynesian concepts from *The General Theory* and *A Treatise on Money* to the challenge of optimising the process of progress and development in Britain and overseas. This included, for example, consumer welfare in the concept of liquidity preference and credit rationing, the optimistic multiplier concept of ever-increasing optimal growth, Joan Robinson’s notion of disguised unemployment as an indicator of economic potential and dignity in the labour force, Kaldorian ideas of growth, income and technical progress, and the growth models developed by Harrod and Domar (Pasinetti 2007). These concepts were largely applicable to Britain, as an industrialised nation with different problems of employment based on technical progress, an established financial sector, and limited propensity for, say, gold hoarding. Nevertheless, the Cambridge Keynesians enjoyed substantial influence in the world of *international* development economics as it was emerging.

¹⁵ Nevertheless, his sense of justice emerged in his ardent support of India’s case at the IMF conference at Bretton Woods in arguing that it was unthinkable that the country’s relatively large sterling balances (accumulated by British wartime debt to India) should be the price of instituting independence and self-government in India (Chandavarkar 1986).

To analyse in depth the contributions of the Cambridge Circus and Post-Keynesians to development economics would require a separate chapter or two. We choose in this chapter to present a snapshot of the work of Keynesian disciple, Joan Robinson, who not only retrieved Keynesian concepts and brought a more moral stance to them, but also had practical experience in the colonies (or today, the developing world). It remains difficult to find research material on India in the 1920s and 1930s. It is therefore fortunate that we have some research for this period through the efforts of Marshall, Joan Robinson, and Austin Robinson as well as Pervez Tahir, who completed a PhD at Cambridge in 1990 with the title ‘Some Aspects of Development and Underdevelopment: Critical Perspectives on Joan Robinson’. Joan and Austin had lived in the then Indian Princely state of Gwalior and participated in the controversy over the question of the division of resources between Gwalior and the Government of India. While in Gwalior, Austin wrote drafts of a memorandum covering the major applied economics topics found in Part II of the Economics Tripos, which Joan contributed to through discussion and possible minor revisions. In this sense, there can be no doubt that both Joan and Austin developed an active interest in India’s economic problems during their stay in Gwalior.

Joan Robinson displayed a relative lack of interest in applied or empirical work during her professional career. Theory was her domain (Harcourt 1984: 652). Nevertheless, Tahir pronounces her as being distinct from ‘the company of those economists who...had little experience of underdeveloped economies’ (Tahir 1990: 13), and she can be associated with the minor tradition of those who looked to these economies to understand the nature of global capitalism (Cohen and Harcourt 2003). In her and Austin Robinson’s contribution to *The British Crown and the Indian States*, she presented ‘a realistic, non-Keynesian view of unemployment in the underdeveloped countries, an emphasis on the population problem, optimism about export prospects under international co-operative arrangements ... [S]he tended to overemphasize the role of the state’ (Tahir 1990: Summary). In this sense, Joan Robinson embodied the trail of thinking as it emerged from Malthusian, Marshallian, and Keynesian ideas on how humanity might optimally progress, taking these ideas into a new industrial and global dimension. Giving importance to industrialisation, taking active measures for capital accumulation, and the role of a *hard state*, the Robinsons (like the other Post-Keynesians) drove the idea of human development as a phenomenon of optimisation and aspiration¹⁶ rather than a steady-state process under natural law.

¹⁶We can tie this idea of optimisation and aspiration to the work of the Cambridge School of Historiography, for which economic historians including the likes of R. Robinson, Gallagher, Cain, and Hopkins used regional and pluralist perspectives to examine post-colonial development.

6 Conclusion

Development economics represents a dynamic and often chaotic process of gradual social, political, and economic renovation. In this prequel to the institutionalisation of development economics, it is evident that the Cambridge tradition played a significant role in political economy, though perhaps it was not always tangible due to the nature of the evolution of development economics from the mainstream. Development economics can be demonstrated to have had its clear roots in the doctrine of human perfectibility and the ambitions for the universal betterment of mankind; political economy evolved from moral philosophy with this idea as its specific motivation, while Cambridge men such as Malthus, Marshall, as well as the Keynesians drove the debate forward. The rich backstory of development economics demonstrates the contributions of not only the Keynesian way of thinking and its influence on the modern definition of post-war development as we know it today, but also the Cambridge tradition and its influence on how the subject emerged from political economy and moral philosophy. A common regard today for the traditionally important principles of morality, (capital) accumulation and its relationship to inequality might be the point of convergence for historical ideas and contemporary ideas in development economics. The popularity of development economists such as Amartya Sen, Joseph Stiglitz, Partha Dasgupta, and Ha-Joon Chang demonstrates these parallels. We might then generalise Chakravarty's description of the Keynesian contribution to development economics to include the Cambridge contribution: it is 'impossible to state with any degree of categoricalness...because, with some exceptions, today everyone is a Keynesian [or rather, a Cantabrigian] in some sense or the other' (Chakravarty 1997).

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5

Cambridge and Econometrics

Jim Thomas

1 Introduction

In his *Editorial* in the first issue of *Econometrica* in 1933, Ragnar Frisch set out to explain what econometrics was and what it was not. He wrote (Frisch 1933: 1–4; italics in original):

A word of explanation regarding the term econometrics may be in order. Its definition is implied in the statement of the scope of the Society, in Section I of the Constitution, which reads: ‘The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics ... Its main object shall be to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences.’

But there are several aspects of the quantitative approach to economics, and no single one of these aspects, taken by itself, should be confounded with econometrics. Thus, econometrics is by no means the same as economic statistics. Nor is it identical with what we call general economic theory, although a considerable portion of this theory has a definitely

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quantitative character. Nor should econometrics be taken as synonymous [sic] with the application of mathematics to economics. Experience has shown that each of these three view-points, that of statistics, economic theory, and mathematics, is a necessary, but not in itself a sufficient, condition for a real understanding of the quantitative relations in modern economic life. It is the *unification* of all three that is powerful. And it is this unification that constitutes econometrics.

Thus, Frisch's description of econometrics involves the coordination of statistical data, economic theory, and quantitative analysis. The form of the quantitative analysis is not specified, but the early emphasis in the work of Haavelmo (1943) and the Cowles Foundation in developing statistical methods for estimating the parameters in sets of simultaneous equations came to be equated with econometrics, as is clear from the contents of such early successful textbooks in econometrics as Goldberger (1964) and Johnston (1964). The result, one can argue, is that econometrics became narrower than was envisaged in Frisch's *Editorial*, and my interpretation of econometrics will be broader than that covered in textbooks of econometrics. I shall include quantitative methods of analysis that are *relevant* to the investigation of the economic problem being considered.¹

To evaluate Cambridge's contribution to econometrics, I shall consider both microeconomics and macroeconomics. Under these two headings I shall examine the three components of econometrics broadly defined, namely (1) the relevant theory, (2) the relevant data, and (3) the appropriate quantitative analysis. Separate attention will be given to the work of the Department of Applied Economics (DAE) under Richard Stone.² This chapter will be a historic survey through to the 1960s, to cover the period in which Cambridge made significant contributions to the development of quantitative analysis.³

¹ This approach is consistent with Richard Stone's view of econometrics, for as Hashem Pesaran notes (Pesaran 2004: 1,167): 'In July 1945 Stone returned to Cambridge as the first Director of the newly founded Department of Applied Economics (DAE). As Director, Stone was given a free hand in initiating the Department's main programme of research. Encouraged by this he developed a research proposal organised around the aims of econometrics, which to him were defined much more broadly than its narrow textbook sense of today. He considered the ultimate aim of applied econometrics as increasing "human welfare by the investigation and analysis of economic problems of the real world", and believed that this lofty aim can best be achieved by the synthesis of three types of study: observations, formulation of testable hypotheses and the development of appropriate statistical methods'.

² The discussion will be brief in some sections in order to avoid repetition with material presented in other chapters.

³ Backhouse (1996) provides a good overview of changes in British economics during the period from 1945 to 1960 as part of a survey of the post-war period.

2 Economic Theories

Microeconomic Theories: To the extent that ‘It’s all in Marshall’, as Pigou claimed, then Marshall’s *Principles of Economics* must have a generic claim to provide the basis for much of the microeconomic theory underlying quantitative analysis.

Macroeconomic Theories: For the theory behind much macroeconomic modelling, whether the author would have approved or not, Keynes’s *General Theory* is clearly the main source that was then developed by various brands of Keynesians: Neo-Keynesians, New Keynesians, anti-Keynesians, and Post-Keynesians.⁴

3 Relevant Data

To investigate macroeconomic theories using conventional macroeconomic models one needs data on the variables in the model, and these are provided by National Income Accounts (NIAs).

The historical survey of the development of NIAs in Kendrick (1970) traces their origins in the UK back to Sir William Petty (1665) and Gregory King (1688). Moving forward to the twentieth century, accounts were produced by Arthur Bowley (1919, 1920) and Bowley and Stamp (1927) and although Bowley was at the London School of Economics (LSE) when the work was carried out, he was a Cambridge mathematician, a student of Alfred Marshall, who recommended him to the LSE, so there is a Cambridge link. These studies presented estimates of national income in terms of factor shares and also contained considerable information on industrial structure.

The work of Bowley and Stamp was updated by Colin Clark, a Lecturer at Cambridge from 1931 to 1937. In Clark (1932) he presented estimates of Britain’s annual national income for the period from 1924 to 1931 and also constructed estimates for the production and expenditure accounts. Some earlier estimates made by Clark had been used by Richard Kahn to estimate the multiplier in Kahn (1931) and Keynes was impressed by Clark’s results.⁵ Clark produced a major study on national income in Clark (1937)

⁴ See Dow (1998) for a detailed analysis of the various brands.

⁵ Tily (2009: 343, fn. 20; italics in original) reports that ‘On January 2 1933, Keynes wrote to Clark: “I have just finished reading your book carefully ... I think it is *excellent*. An enormous step forward. I hope it is selling all right”’. For a further evaluation of Clark’s work, see Groenewegen (2003).

but then left Cambridge for Australia and did little other work in this area. Nevertheless, with the outbreak of the Second World War, Keynes published *How to Pay for the War* in February 1940, making use of Clark's figures to make the calculations for his proposed income deferral policy to control excess demand during the war.

Among the academics drawn into government service as part of the war effort were James Meade and Richard Stone.⁶ Meade was based at Oxford but had been involved in discussions in Cambridge before the publication of *The General Theory*, and Stone had been taught by Clark while he was a student. They began working together on producing NIAs, with considerable encouragement and support from Keynes, and the publication of annual White Papers containing the estimates.

At the end of their war service, Meade went to the LSE and undertook the research in international economics that led to the award of the Nobel Prize (joint with Bertil Ohlin) in 1977. Stone returned to Cambridge, continued to work on NIAs, and played a major role in establishing internationally consistent standards. In 1984, he was awarded the Nobel Prize for Economics for this work.

While Keynes's strong interest in the construction of NIAs was for the guidance they could provide for policy-making, they have provided the raw material for the estimation of macroeconomic models.⁷ The NIAs represent one method of measuring the economic activities of a country over a period of time, but there are other ways of measuring economic activity and of extending the NIAs, such as input–output tables, social accounting matrices (SAMs), and computable general equilibrium (CGE) models.⁸

In parallel with his continuing work on the standardisation of NIAs, Stone considered how input–output tables might be used to provide a disaggregated analysis of the business sector to complement the information given by the NIAs (see Stone et al. 1949; Stone and Utting 1950; Stone 1951–1952). One of the more important developments was the concept

⁶Stone describes these events in his Nobel Prize autobiographical notes. These are reprinted in Måler (1992: 109–114). See also Deaton (1992) and Johansen (1985).

⁷In addition to the construction of current NIAs, historical British data for the years between 1688 and 1973 were published in Mitchell and Deane (1962), Deane and Cole (1962) and Matthews et al. (1982); these have gladdened the hearts of many cliometricians.

⁸Wassily Leontief constructed the first input–output tables for the USA in the 1930s (see Maragoni and Rossignoli 2014). The basic idea for the SAM may be traced back to the 1758 *Tableau Économique* of François Quesnay, but recent work stems from the work published in Meade and Stone (1941). See Kehoe (1998) for further details. An early example of CGE modelling is Johansen (1960). While the development of CGE models lies outside the period focused on in this chapter, Cambridge economists have made important contributions to this method of quantitative analysis of macroeconomic data and its disaggregation (see Cunningham et al. 1998; Barker 1998).

of transaction models, which involve a matrix of transactions and a matrix of responses. The former presents a record of transactions between the different sectors in an economy, while the latter introduces changes into the model and examines their effects.

The information in a set of NIAs could be transformed into a SAM, a square matrix with rows and columns corresponding to the sectors in an economy being considered. The entries in the rows represent receipts and the columns expenditures, yielding a complete picture of all the monetary flows in the economy. Stone (1954a) provided a link between the various methods of presenting economic data by showing that both the NIAs and input–output tables could be derived from a SAM by pre- and post-multiplication of the SAM by grouping matrices. This integration of three different ways of analysing economic information greatly increased the flexibility and range of economic analysis.

4 Relevant Quantitative Analysis

Microeconomic Analysis

An important contribution to demand analysis was Stone's work on the estimation of the linear expenditure system (LES) (Stone 1954b). One interesting feature of the LES is that it takes account of the fact that in allocating their expenditure consumers have prior commitments to meet, such as paying the mortgage, buying food, and paying utility bills, before responding to prices and allocating the rest of their incomes. Stone represented this situation by the equation

$$\hat{p}q = \hat{p}\bar{q} + b(\mu - p'\bar{q}) \quad (5.1)$$

where p and q represent price and quantity vectors, respectively. In Equation (5.1), \bar{q} corresponds to the quantities that consumers have to buy, b is a vector of constants that sum to unity, and $\mu \equiv p'q$ is total expenditure. Equation (5.1) may be rewritten as

$$q = \hat{p}^{-1} \left\{ b\mu + (bi' - I)\hat{c}p \right\} \quad (5.2)$$

where i is the unit vector, I is the unit matrix, and $\hat{c} = -\bar{q}$.

Deaton (1998: 600) gave the following evaluation of Stone's work:

Stone's achievement lay not in deriving the demand functions, but in thinking to estimate them. The demand functions [Equation 5.1 above], even if fitted to the data by least-squares, require non-linear optimization, and Stone invented a simple and not very efficient scheme, but one that allowed him to obtain parameter estimates and a good fit to interwar British data for a six commodity disaggregation of expenditures. This was a major breakthrough, not only in demand analysis, but also in applied econometrics in general. Indeed, much of demand analysis for a decade or so after Stone's paper consisted of applying better algorithms and faster computers to the fitting of Stone's model to different data sets.

Stone's solution to the non-linearity problem was to derive some parameter estimates from a simpler, restricted version of the model and then substitute these estimates into the full version of the model to estimate the remaining parameters and to iterate this process until stable values of the parameter estimates were obtained.

Macroeconomic quantitative analysis

There was a Cambridge contribution to the early empirical work in macroeconomics. In only the fifth ever article to be published with estimates of the parameters in a consumption function,⁹ Clark (1938) used quarterly time series data from 1929(Q1) to 1937(Q4) to estimate

$$(C/P)_t = \alpha + \beta(Y/P)_t + \gamma(Y/P)_{t-5} + u_t \quad (5.3)$$

The sixth article to be published that estimated consumption functions (Stone and Stone 1938) used both cross-sectional and time series data and experimented with a number of functional forms for different countries: For cross-sectional data:

$$\log c_i = \alpha + \beta \log y_i + u_i \quad (5.4)$$

$$\log c_i = \alpha + \beta \log y_i + \gamma (\log y_i)^2 + u_i \quad (5.5)$$

⁹ See Thomas (1989: 146).

For time series data:

$$C_t = \alpha + \beta Y_t + u_t \quad (5.6)$$

$$C_t = \alpha + \beta Y_t + \gamma t + u_t \quad (5.7)$$

$$C_t = \alpha + \beta(0.75Y_t + 0.25Y_{t-1}) + u_t \quad (5.8)$$

Stone returned to the subject in Stone (1942) where he used annual US time series data to estimate

$$C_t = \alpha + \beta Y_t + \gamma(t - 1935) + u_t \quad (5.9)$$

$$C_t = \alpha + \beta Y_t + \gamma(t - 1935) + \delta Y_t^2 + u_t \quad (5.10)$$

Further work by Stone and other colleagues will be presented in Section 5, where the work of the DAE is discussed.

Keynes and Econometrics

Most discussions of this topic concentrate on the Keynes versus Tinbergen controversy, but there were some interesting interventions by Keynes before that exchange in the context of two earlier empirical studies of the marginal propensity to consume (MPC).

Keynes and the MPC: In *The General Theory*, Keynes does provide an algebraic formulation for the propensity to consume, but in very general terms:

We will therefore define what we shall call *the propensity to consume* as the functional relationship χ between Y_w , a given level of income in terms of wage-units, and C_w the expenditure on consumption out of that level of income, so that

$$C_w = \chi(Y_w) \text{ or } C = W.\chi(Y_w)$$

The amount that the community spends on consumption obviously depends (i) partly on the amount of its income, (ii) partly on the subjective needs and the psychological propensities and habits of the individuals composing it and the principles on which the income is divided between them (which may suffer modification as output is increased) (Keynes 1936 [1973]: 90–91; italics in original).

Soon after the appearance of *The General Theory* in 1936, there were publications by Hans Staehle (Staehle 1937) and Elizabeth Waterman Gilboy (Gilboy 1938) which both accused Keynes of not taking the distribution of income sufficiently into account (see Thomas 1992).

Staehle, in the first published article to estimate a consumption function (see Thomas 1989), set out specifically to test two propositions: first, that the MPC declined as income increased, and secondly, that the distribution of income was important. He set up the regression equation

$$(R/Y_L)_t = \beta_0 + \beta_1(Y_L/P_w)_t + \beta_2 B_t + u_t \quad (5.11)$$

and using quarterly macroeconomic data for Germany and plausible proxy variables for consumption and the price level he obtained results consistent with his two hypotheses.¹⁰ Staehle argued that ‘the regression equation...shows, as everyone would expect, that the proportion of income spent on consumption goods increases when income ceteris paribus diminishes ... The more income is concentrated in the hands of receivers of large incomes, the smaller...will be the proportion of total income spent on consumption goods’ (Staehle 1937: 141).

In contrast to Staehle’s use of regression analysis, Gilboy’s approach was less technical: she plotted cross-sectional data on household expenditure and incomes on double logarithmic paper and fitted free-hand curves to estimate the elasticity of expenditure with respect to income. On finding that the best fitting curves were non-linear, she concluded that income distribution would affect consumption behaviour.

Keynes’s reacted to both articles (Keynes 1938a, b, 1939a). His main objection was that both critics had ignored his discussion of the MPC in *The General Theory*, which clearly allowed for income distribution to have an effect. Both critics accepted this point and in her response Gilboy (1939: 633–634) quoted from a letter Keynes had written to her in which he stated that:

I give reasons for expecting that, as a rule, a greater proportion of income will be saved, as real income rises. This is a mere statement of opinion, which requires more statistical examination than I have given it, not the law which I am taking as

¹⁰ Here R is an index of retail sales in nominal terms, as a proxy for consumption, Y_L is aggregate labour income in nominal terms, P_w is an index of wage rates, and B is a measure of income inequality defined on median incomes. If the MPC declines with increasing income and income distribution matters, then both β_1 and β_2 should be negative, which they were. Staehle published his data, and when I reran his regression it yielded stable estimates and performed well against the standard specification tests (see Thomas 1992).

my premise; ... My argument does not require, of course, that this rule holds good of every individual or even of every class, but only of the community as a whole; and it is subject to the usual *ceteris paribus* clause; though I should distrust any statistics which seemed to show the contrary as applying to a whole class.

Keynes versus Tinbergen: This controversy proceeded in three stages: (i) there was correspondence between Keynes and one R. Tyler at the League of Nations after Tyler had sent Keynes a proof copy of Tinbergen (1939) for comment. Keynes's negative comments were passed on to Tinbergen and there was some correspondence between Keynes and Tinbergen; (ii) the debate became public when Keynes published a critical review of Tinbergen's book in the *Economic Journal*, followed by an exchange of comments (Keynes 1939b, 1940; Tinbergen 1940); and (iii) the debate was extended when a number of econometricians wrote to Keynes following the publication of his review and the exchange of comments with Tinbergen.¹¹

While the debate ranged over many issues, the one that Keynes pressed most strongly was that when using time series to estimate the parameters 'the method requires not too short a series, whereas it is only in a short series, in most cases, that there is a reasonable expectation that the coefficients will be fairly constant' (Keynes 1973: 294). In addition to this general objection, Keynes raised the following questions and suggestions:

- How was the functional form to be chosen for a given theory? Was linearity a plausible assumption for economic models?
- Were the set of explanatory factors a complete or only a partial set?
- Were all the explanatory factors measurable? What about expectations and the state of confidence relating to the future?
- Were the explanatory factors independent of each other?
- Might there be a problem of simultaneity?
- Given that Tinbergen had data for 40 years, might he not have fitted his model to each decade and compared them with those obtained for the complete period?¹²

¹¹ For the details of the debate, see Garrone and Marchionatti (2004).

¹² If we contrast Keynes's negative reactions to the studies of the MPC and to Tinbergen's business cycle analysis, the issues targeted are very different. In the MPC exchanges, Keynes's major concern was to avoid too narrow an interpretation of the MPC and he did not comment on or criticise the statistical analysis used in the studies by Staehle and Gilboy. In his response to Tinbergen's work, his own theories are not in question and he was able to take a broader view of the dangers he saw in the application of multiple correlation analysis to economic theorising.

The early reactions of mathematical economists and econometricians to Keynes's criticisms tended to be hostile. Paul Samuelson in his obituary of Keynes (Samuelson 1946)¹³ wrote:

I must confess that my own first reaction to the *General Theory* was not at all like that of Keats on first looking into Chapman's Homer. No silent watcher, I, upon a peak in Darien. My rebellion against its pretensions would have been complete except for an uneasy realization that I did not at all understand what it was about. And I think I am giving away no secrets when I solemnly aver – upon the basis of vivid personal recollection – that no one else in Cambridge, Massachusetts, really knew what it was about for some 12 to 18 months after its publication. Indeed, until the appearance of the mathematical models of Meade, Lange, Hicks and Harrod there is reason to believe that Keynes himself did not truly understand his own analysis (ibid.: 187–188).

Lawrence Klein (Klein 1951) in reviewing Harrod's *The Life of John Maynard Keynes* (Harrod 1951) wrote:

In econometrics Keynes was even less well versed, his presidency of the Econometric Society in 1945 notwithstanding. His review of Tinbergen's celebrated study for the League of Nations was one of his sorriest professional performances. Many econometricians have remarked on Keynes's review with the comment that he simply did not understand the methods he was criticizing and failed to see at what Tinbergen was aiming, a type of comment Keynes frequently used to characterize his own critics. Ironically enough, much of Keynes's greatest support has come from econometric testing and applications of his theory. This work will continue in the future and may play a major role in perpetuating or extending the theory of employment (Klein 1951: 450–451).

More recently, commentators have tended to be kinder to Keynes. For example, Stone (1978), while not accepting Keynes's criticisms, provides a possible explanation of why Keynes was so violent at times in his criticisms of Tinbergen:

Up to a point the explanation may lie in Keynes's state of health. In 1937 he had had a severe heart attack; and the summer of 1938, when he received Tinbergen's proofs, must have been a particularly bad moment for him to be faced with an approach to economics so very different from anything he was accustomed to. But this does not justify the virulence of his remarks. While not pretending to know the full answer to the puzzle, I have three suggestions to offer.

¹³In the opening footnote, Samuelson writes that 'I owe much in what follows to discussions with my former student, Dr. Lawrence R. Klein' (Samuelson 1946: 187, fn.1).

First, Keynes suffered from an irresistible urge to overstate ... Both by temperament and by training he was heir to the great rhetoricians of the nineteenth century ... Second, as I have said, by the thirties Keynes's mathematics had become pretty rusty ... Third, in my experience Keynes's reaction to anything new was to look for the weak spots and shoot them full of holes (ibid.: 62–63).

More positive is Hendry (1980):

Forty years after Keynes wrote, his review should still be compulsory reading for all who seek to apply statistical methods to economic observations. Taken literally, Keynes comes close to asserting that no economic theory is ever testable, in which case, of course, economics itself ceases to be scientific – I doubt if Keynes intended this implication. However, his objections make an excellent list of what might be called “problems of the linear regression model”, namely (in modern parlance): using an incomplete set of determining factors (omitted variable bias); building models with unobservable variables (such as expectations), estimated from badly measured data based on index numbers (Keynes calls this the “frightful inadequacy of most of the statistics”); obtaining “spurious” correlations from the use of “proxy” variables and simultaneity as well as (and I quote) the “mine [Mr Yule] sprang under the contraptions of optimistic statisticians”; being unable to separate the distinct effects of multicollinear variables; assuming linear functional forms not knowing the appropriate dimensions of the regressors; mis-specifying the dynamic reactions and lag lengths; incorrectly pre-filtering the data; invalidly inferring “causes” from correlations; predicting inaccurately (non-constant parameters); confusing statistical with economic “significance” of results and failing to relate economic theory to econometrics. (I cannot resist quoting Keynes again – “If the method cannot prove or disprove a qualitative theory and if it cannot give a quantitative guide to the future, is it worth while?”) (ibid.: 396).

Perhaps Hendry is overly generous, but in the light of recent problems with economic modelling and forecasting, his case seems much stronger than that of either Samuelson or Klein.

5 Richard Stone and the Department of Applied Economics

The idea for a DAE was strongly supported by Keynes in November 1939, but it was only established after the Second World War in 1945. There had earlier been an initiative known as the Cambridge Research Scheme, of which the chairman of the steering committee was Keynes and with funding from the

National Institute of Economic and Social Research. Stone was involved with this Scheme, as were a number of other Cambridge economists. In November 1944, the Directorship of the DAE was offered to Stone, who took up his post in May 1945. Initial funding came from the Rockefeller Foundation prompted by Keynes.¹⁴

In relation to the programme of research to be carried out by the DAE, Stone said that, 'My idea was to set up an econometric program which would embrace work on facts, work on theories, and work on econometric and statistical methods needed to analyse the facts in the light of the theories' (Stone quoted in Stone and Pesaran 1991: 100). This was an ambitious programme and the extent to which the DAE carried it out under Stone is very impressive. Part of the success was Stone's wide range of international contacts, which made it easy for him to invite them to spend time in Cambridge (see Smith 1998 for details). Among those who visited were Afriat, Anderson, Brumberg, Cochrane and Orcutt, Cramer, Duesenberry, Farrell, Geary, Houthakker, Prais, Prest, Tintner, Tobin, and Watson.

Earlier sections in this chapter discussed Cambridge contributions to data collection and theory, but did not tell the full story of the contribution to quantitative analysis. This may be divided into extensions and improvements in quantitative techniques and the carrying out of applied quantitative studies.

The extensive use being made of time series data led to concerns about problems of autocorrelation, and this was explored initially by Cochrane and Orcutt in work that led to an early correction for first-order autocorrelation (AR(1)) (see Cochrane and Orcutt 1949). This work was then extended by Durbin and Watson (1950, 1951), who derived a bounded test for AR(1) that became the standard for many years. The increasing use of variables defined in terms of logarithms led to work by Aitchison and Brown (1957) on the log-normal distribution. In addition, Stone et al. (1949) did work on sampling and Roy (1950, 1951) examined the distribution of income.¹⁵ On the applied side, there was important work by Prais and Houthakker (1955) analysing family budgets, and work continued on the Growth Project.

In 1955, Stone returned from a sabbatical at Johns Hopkins University to a new position, the P.D. Leake Chair of Finance and Accounting and an end of his Directorship of the DAE. Deaton (1992: 486) suggests this was a painful event:

However, the Cambridge Keynesians (not Keynes, who had died in 1946) arranged matters so that Stone had to give up the DAE in order to take up the chair, though he made it a condition that he would retain a research group

¹⁴ See Stone and Pesaran (1991: 96–105).

¹⁵ Some of the research was made possible by work going on at Cambridge to develop digital electronic calculators that began to make large-scale number crunching exercises possible. See Smith (1998: 98–99 and the references given therein) for more details.

within the department. In his good-humoured way, Stone would later say that this was all for the best – his research was not restricted, and he was relieved of administrative burdens – but there is no doubt that losing the DAE was a source of much sadness.¹⁶

The new Director was Brian Reddaway, and he was less interested in econometric research. Smith (1998: 99–100) suggests that ‘He shared the general Cambridge doubts about the applicability of econometrics to complex economic processes, where numerous factors, many unmeasurable, interacted and relationships were likely to change through time’. While applied econometrics continued within the Growth Project, Reddaway’s interests led to important work on foreign investment and its effect on the domestic economy and on the Selective Employment Tax that had been proposed by Nicholas Kaldor.

The material presented so far shows that researchers working at Cambridge made major contributions to the development of all three components of quantitative analysis. Smith (*ibid.*: 88) suggested that there was a growing interest in the history of econometrics and that:

In this history the DAE plays a central role. Epstein (1987, p. 142) says that ‘the influence of this English work was comparable to that of the Cowles Commission in establishing directions for econometric research.’ The English work was initially done at the DAE, but Epstein, like Gilbert (1986), sees leadership of the English school of econometrics leaving Cambridge for London in the 1950s. Like Cowles, after a period of developing econometric methods the DAE turned its attention to other topics.¹⁷

6 Conclusion

The above discussion shows that Cambridge made important contributions to all three components of quantitative analysis, with Maynard Keynes and Richard Stone playing major roles in this development. It may well be argued that before 1960 Cambridge was the major centre for quantitative analysis in the UK, though the leadership in econometric theory did shift to London after 1960.

¹⁶However, when interviewed by Deaton and asked about how giving up his Directorship of the DAE had affected his work, Stone replied: ‘Very little: it really worked out for the best from my point of view. At first I was a little put out as I had come to think of the Department as mine’ (Stone quoted in Stone and Pesaran 1991: 105).

¹⁷This is a convenient spot at which to end this historical survey of the many contributions to econometrics made by Cambridge. What followed the move to London will be explored in a chapter ‘LSE and econometrics’ to be included in the forthcoming Palgrave companion to LSE economics.

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6

Cambridge in Mind: Economics and Psychology on the Cam

Vincent Barnett

What role does psychology play in generating or influencing economic and business behaviour? This seemingly innocuous question has produced an extensive stream of literature over many decades (for a well-known example, see Akerlof and Shiller 2009). In this chapter, the contributions of some notable Cambridge economists and psychologists to answering this question, from the last quarter of the nineteenth century up until the Second World War, will be investigated. Henry Sidgwick, Alfred Marshall, A.C. Pigou, and John Maynard Keynes are the four economists to be considered, and James Ward, G.F. Stout, and Charles Myers the three psychologists.

Although the historical examination of the psychological underpinnings of economic theory is a topic that has recently received some attention (see Cook 2006; Bruni and Sugden 2007), what is original in this chapter is that it will examine the work of Cambridge psychologists alongside the work of Cambridge economists, in order to better understand any potential links and/or overlaps in how the two disciplines developed within a city-specific environment. Cambridge is taken here to mean not just the University environment, but the wider cultural milieu of the city as well.

Sidgwick believed there to be such a thing as ‘the Cambridge mind’: certain aspects of subjects had more attraction for those in the Cambridge tradition than others (Sidgwick 1876a: 244). The special Cambridge interest in natural philosophy, as represented in the Newtonian tradition, ‘disposes the mind to

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be interested in hypothetical extensions of physical explanations to psychical phenomena' (ibid.: 245). This clearly suggested that psychology might have certain unique attributes within the Cambridge context.

One key aspect of the psychology/economics interaction was well formulated by Dennis Robertson in 1915 whilst he was discussing the 'much abused factor the "psychology of the business man"' (Robertson 1915: 38). The often implicit and sometimes problematic connection between psychological explanation and economic rationality was clearly articulated:

At one extreme in this matter stands Mr. Hull [G.H. Hull, author of *Industrial Depressions* (1911)], who regards the business man as an entirely rational and clear-headed expert, not compelled by 'an organic defect of human nature,' nor by 'celestial influences,' nor by 'mental waves' nor by 'sun spots,' but governed solely by plain, simple unadorned business considerations. At the other extreme stand those who appear to regard him as a wholly incalculable and irresponsible person, whose mental processes it is entirely beyond our power to analyse. But it is clearly possible to hold that while a business man's actions are often not rational, they are nevertheless very seldom wholly incalculable in the sense that they are not provoked by any external stimulus: they may be the result of non-rational or only semi-rational inference from the actions of other people or from other observed facts (Robertson 1915: 38–39).

Here a rationality continuum is articulated: business people as entirely non-rational, as semi-rational, or as wholly rational, with economists seen either as fully capable or entirely incapable of understanding 'the business brain'. Robertson evidently believed in the middle option, which was that business people were not always entirely rational, sometimes non- or semi-rational, but usually understandable by economists in relation to the business situation that they found themselves in.

However, up until the Second World War, considering the role of psychology in explaining the rationality that underpinned business behaviour was often a problematic theme: it was included in the works of some economists as an afterthought, or as a supplementary aspect that needed to be added so as to cover all the bases. Others did provide a more integrated analysis of the place of psychology in economic understanding, as is sketched below, but sometimes these psychological aspects were then lost in further elaboration, for example, in the process of mathematical modelling. Psychology was occasionally conceived of as something entirely alien to the calculative rationality of *homo economicus*.

Recent historical work has rightly indicated the importance of psychology to early neoclassical economics in the UK: experimental psychology was a ‘central component’ of neoclassical economics as articulated by Jevons and Edgeworth (Bruni and Sugden 2007: 149). Psychological laws relating to human wants were used to deduce economic theories, assuming self-interest and the pursuit of pleasure, that is, self-interest had a psychological component. There was a hierarchy of wants and sensations in relation to income levels that required psychological explanation. The formal separation of economics from psychology based on maximisation concepts was not fully completed until the 1930s and 1940s (ibid.: 146).

The question naturally follows: How did Cambridge neoclassical economists like Marshall utilise psychological concepts in their own work? As will be shown in this chapter, Cambridge economists took varying positions with regard to some aspects of the psychology/economics nexus, whilst on others, common features between them can be detected. But before attention is focused on economists of the Cambridge School, a sketch of the early development of psychology as a subject is required.

1 The Origins of Psychology

Psychology as a discipline focused on studying the ‘internal’ mind was not fully separated from philosophy until early in the nineteenth century (Hatfield 2003). Immanuel Kant, for instance, conceived of empirical psychology as part of applied philosophy, and he contrasted it to rational psychology or ‘I think’, which was part of the soul. Rational psychology is related to inner experience/pure self-consciousness, while empirical psychology added the relation of the inner self/soul to external objects of perception, as accomplished by means of ‘body’/the outer senses (Kant 1787 [1929]: 330, 664).

This dualistic distinction between a quasi-spiritual ‘mind-in-itself’, and then its environmental interactions through the body, was important to the early development of Continental psychology. However, Kant’s psychology was rather different to the approach adopted in Britain, where the Associationist School came to dominate across much of the nineteenth century, as exemplified in the work of Alexander Bain. Bain hypothesised that the ‘great fact of the mind’ was ‘association of ideas’ or ‘different impressions concurring in the mind’, which ‘adhere and make up a whole’ (Bain 1861: 266). Associationists argued that individual differences of mind were not innate but arose because of differences of experience and hence of association, and they

saw psychology as the science of mental phenomena divided into intellect, feeling, and will (Hatfield 2003).

One of the most important economists of the nineteenth century to adopt the associationist approach was John Stuart Mill (Fancher 1994). For Mill, sentiment and feeling arose through a process of association as ‘mental complexes’ (Wilson 1998: 213), and economic theory should be embedded within a broader approach that included the laws of psychology developed on associationist terms (ibid.: 235). Mill had an impact on Cambridge economists via his influence on Marshall (O’Brien 1981), but what of Cambridge psychologists as a ‘school’? Did they share any themes in common with their economist colleagues? In order to answer this question, a more detailed account of Cambridge psychology is required.

2 Psychology at Cambridge

The three most important figures associated with psychology at Cambridge at the end of the nineteenth century and the beginning of the twentieth that are relevant to the psychology/economics connection are James Ward (1843–1925), G.F. Stout (1860–1944), and Charles Myers (1873–1946).

Ward started out studying ‘moral sciences’ at Trinity College in 1870, was elected to a Fellowship there in 1875, and was subsequently chosen as Professor of Mental Philosophy and Logic at Trinity in 1897, remaining there until his death. He was a student of Henry Sidgwick (Bartlett 1925: 450). ‘Mental philosophy’ was a term that encompassed both psychology and metaphysics (Sidgwick 1876a: 245). Ward’s most important works were *The Relation of Physiology to Psychology* of 1875, three articles on ‘Psychological Principles’ in *Mind* in 1883–1887 that were later published as a book, and the *Encyclopaedia Britannica* entry on ‘Psychology’ in the 1886 and later editions.

Ward did not write anything that had a direct relationship to the subject matter of economics, but he was closely involved with shaping how psychology developed in Cambridge, and had a personal link to the Keynes family. For example, at the end of the 1880s ‘the Keynes and Ward families would picnic together on the river in the summer’ (Skidelsky 1983: 69). Maynard Keynes was probably too young to have absorbed anything from Ward at this time, but his father was not. Neville Keynes enjoyed an ‘early and lasting friendship with James Ward’, and it was Ward who was ‘probably influential’ in enabling him to switch from studying mathematics to the moral sciences in 1873 (Groenewegen 1995).

Consequently, *The Scope and Method of Political Economy* acknowledged the ‘importance of psychological premises to certain departments of economic enquiry’ and admitted that the ‘psychological analysis of motives is important’ to understanding the laws of capital growth (Neville Keynes 1917: 91, 205). Ward also consulted with Neville Keynes regarding Cambridge lecturing appointments (Wall 2007: 146). Moreover, when Marshall decided to switch his own focus of study away from psychology in 1871–1872, it was to Ward that he wrote explaining the reasons for his change of heart (Groenewegen 1995). Lastly, Ward was also on the working party appointed in 1901–1902 to inquire into increasing the opportunities for studying economics at Cambridge (Groenewegen 1988: 643).

Regarding his approach, Ward repositioned the idea of the self at the centre of psychology and made *attention* the active agency of the new personality psychology that was being formulated at the end of the nineteenth century (Cook 2006). Conscious attention assumed the primary focus rather than association, and Ward saw instincts as arising from psychological habits (Hatfield 2003). He has therefore been credited as ‘establishing a Cambridge school of psychology’ that diverged from the associationist approach that had been dominant in Britain up until that time (Cook 2006).

However, as Ward’s extensive list of publications demonstrated—including work on animal locomotion (1874), the nervous system of the crayfish (1879), Fechner’s Law (1876), and naturalism (1899/1902)—he was also very interested in the link between psychology and physiology, and this would become a key element of the British approach to psychology in this period. He was particularly interested in the relation between the nervous system and the mind (Wall 2007: 136), that is, in the physiology of brain activity, although he cautioned as to whether physiological understanding was sophisticated enough to provide definitive answers (Hatfield 2003).

Stout started as a student at St John’s College, Cambridge, in 1879, became a Fellow there in 1884, and then University Lecturer in the Moral Sciences in 1893 (Schaar 2013: 4), where he remained until 1896. His association with Cambridge was substantial and lasted for 17 years: he was at one point Sidgwick’s student (Wall 2007: 138). Stout’s most significant works were *Analytic Psychology* (1896) and *Manual of Psychology* (1898), the former being studied by Maynard Keynes for the civil service entrance examination in 1905–1906.

Stout saw himself as part of a wider British tradition. In *Analytic Psychology*, where antecedent Cambridge influences such as Ward were acknowledged, Stout referred to the ‘time-honoured procedure’ of Locke and Bain for investigating psychology and to the ‘English line of Empirical Psychology, especially

to Hobbes and Hume' (Stout 1896, volume 1: ix–xi). This English line was distinct from the Continental tradition as exemplified by Kant. Stout presented the 'traditional English method' of psychology as bringing systematic order to the facts of mental life by ascertaining the processes of developed consciousness, that is, it was descriptive and classificatory in nature. Kant's a priori distinctions would have no place in such an approach.

Myers had a much more direct link to economics than was provided by either Ward or Stout, being the author of various works in the field of industrial psychology such as *Mind and Work: The Psychological Factors in Industry and Commerce* (Myers 1921). Myers was appointed Lecturer in Experimental Psychology at Cambridge in 1907 and became the Director of the Cambridge Psychological Laboratory. In 1918, he gave lectures to the Royal Institute on 'Present-day Applications of Psychology, with Special Reference to Industry, Education and Nervous Breakdown'. In 1921, he created the National Institute of Industrial Psychology (NIIP) that aspired to 'introduce the methods of psychology into industry' (Forrester 2008: 40). He explained the focus of this work as follows:

Industrial Psychology...is concerned with the human factor throughout industry...[including] Industrial Physiology in so far as it is impossible to separate mental from bodily activities ... Its aim is to discover the best possible human conditions in occupational work, whether they relate to the best choice of a vocation, the selection of the most suitable workers, the most effective means of avoiding fatigue and boredom, the study and provision of the most valuable incentives to work...the best methods of work and training, the reduction of needless effort and strain due to bad movements and postures...defective routing, layout or organization (Myers 1929a: 9).

Although this was a wide range of concerns, the unifying element was that the latest advances in experimental psychology would be applied to them, the aim being to improve working conditions whilst increasing production efficiency. Myers conceived of these aims as positively reinforcing each other (increased efficiency would come about partly by improved working conditions), and he structured his work under four headings: vocational guidance, work performance, movement study and skills, and management issues.

One notable aspect of Myers's work on industrial psychology was its lack of direct or obvious influence on Cambridge economists, which is probably why not many historians of economics have considered it. Thus, there are few (if any) references to Myers's publications in the works of Marshall, Pigou, or Keynes and vice versa: it was as if industrial psychology at Cambridge was located in an alternative institutional world to industrial economics at the same University.

However, when the approach of industrial psychology is examined in more detail, similarities with economics at Cambridge do clearly emerge. For example, Myers outlined the following regarding methodology:

Industrial Psychology seeks out causes before it recommends remedies. It deals with the worker as a living mental organism, not as a blind, lifeless machine. Its methods are fundamentally those of Biology, not those of Mechanism ... [A] given effect may be the outcome of a large number of interacting causes in different parts of the entire organism (Myers 1929a: 10–12).

It is well known that Marshall evoked biology rather than mechanics as the scientific methodology appropriate for economics. But Myers went further than Marshall in delineating the importance of psychology to industry, explaining that of the ‘four main determinants of industrial and commercial efficiency—the mechanical, the physiological, the psychological, and the socio-economic—the psychological is by far the most important and fundamental’ (Myers 1921: v).

This was because intelligence in foreseeing demand (i.e. Keynes’s entrepreneurial expectations, although Myers did not identify it as this) and a sympathetic understanding of the standpoint of others (what today is called interpersonal intelligence) were more important as factors in the industrial process ‘than mere capital or mechanical labour’ (ibid.). Myers was positing the existence of an additional factor of production apart from land, labour, and capital: this factor was psychological intelligence, a key element of the mental constitution of entrepreneurs. As will be seen, this idea had a direct equivalent in Marshall.

For Myers, the link between economics and biology was not simply abstract and metaphorical, but concrete and direct as well. As he explained in *Mind and Work*, the scientific study of vocational guidance ‘must undertake a careful physiological and psychological analysis of (i) the requirements of different occupations, and (ii) the individual mental and physical differences among those intending to work at them. For the groundwork of the latter task...we are indebted to the experimental psychology of the laboratory’ (ibid.: 77). This latter aspect had a concrete link to Cambridge, as Myers ‘anonymously gave the money to build a laboratory for psychology in Cambridge, which...had opened in 1913’ (Forrester 2008: 38). Although a main focus of Myers’s work at this time (i.e. before and during the First World War) was abnormal psychology, by the end of the war his interest in industrial psychology had begun to blossom.

Myers was open about exporting psychological concepts to other subjects. In 1929, he published an essay entitled ‘Psychological Conceptions in Other Sciences’, in which he highlighted how physics had recently taken

a psychological turn with relativity and quantum mechanics. Moreover, he stressed the fundamentally uncertain nature of human efforts in making predictions in various subject areas, highlighting that, 'for Physics, as for Psychology and for Biology, the past is really different from the future, that the future of any organized (or individual) unit is unpredictable, and that the passage from one side of a mathematical equation to the other is strictly and actually irreversible throughout Nature' (Myers 1929b: 15). Thus, evolution was an essentially open-ended process in which the future never exactly resembled the past, even though it was derived from it. Again, some similarity to Marshall on time-irreversible evolution is apparent here (Egidi and Rizzello 2006: 675).

Perhaps the most obvious parallel, however, between Myers and Cambridge economists was with Keynes. Myers supported the need for government intervention in the economy that was targeted at ensuring productive employment and advocated worker participation in management processes. He argued as early as 1920 thus:

When capital has been paid a due reward for its services, the remaining profits must be equably divided among all concerned in its production. Thus capitalism and employment will come to be rigorously distinguished – employment including both management and labour. To this end we are clearly approaching, the division being no longer between management and labour, but between capitalism and employment (Myers quoted in Costall 1998: 158–159).

Although there are no references to Myers's work in Keynes's writings, this should not be taken to mean that Myers was not influential in wider circles. For example, Ramsay MacDonald addressed a meeting of the NIIP in 1929, where he argued (perhaps a little naively) that industrial peace could be obtained by adopting a Myers-like approach to fitting individual specialities to employee/employer needs (*ibid.*: 158).

To answer the question of what constituted the general Cambridge approach to the psychology/economics nexus, a wider comparison is required. As has been explained, in Britain after the First World War, the method of industrial psychology was experimental and the perspective was psycho-physiological: 'human factors' or fitting the person to the job. This was in contrast to the USA and Germany, where the method was psychological measurement, and the perspective was individual differences: personnel psychology or psychometric testing (Hollway 1994: 1,217–1,218). The US 'personnel psychology' approach reflected more the views (and needs) of the employer, whereas British industrial psychology placed the employee centre stage. As Myers was listed as the main example of the British approach, 'British' was used here as a synonym for Cambridge.

Another important difference was that physiology played a greater role in the British/Cambridge approach than it did in the US/German. It will be argued here that this physiological component of Cambridge psychology, expressed in the early works of Ward and in Stout's interest in psychophysical disposition (Stout 1896, volume 2: 289), had some influence on early Cambridge economics, although the pathways of impact were mediated. In addition, Stout's identification of an English 'empirical' tradition in psychology, and its corresponding associationist approach to understanding the mind, was also relevant.

3 Sidgwick

The Cambridge School of Economics was undoubtedly created by Marshall. However, 'it was not created by Marshall alone', as behind Marshall 'was Sidgwick...Marshall's "spiritual father and mother"' (Backhouse 2006: 40). As Sidgwick did not write a major work on psychology, his interest in it was indirect rather than direct, but this does not mean that it was unimportant. For example, he wrote an article on 'Idiopsychological Ethics' for *Mind* (Sidgwick 1887), and his philosophy was sometimes linked to that of Ward (Keynes 1936: 589).

Sidgwick had studied the origins of psychology as trends in Continental philosophy, and his work on this topic illustrated some common Cambridge themes. For instance, he disagreed with Kant's rational psychology as containing a 'fundamental misapprehension of the knowledge of self which we obtain through self-consciousness' (Sidgwick 1905: 144). Instead of asserting the idea that rational psychology obtained synthetic truths a priori, Sidgwick argued that a rigid separation between rational and empirical psychology was 'unthinkable' (ibid.: 148). He declared that the 'existent we' could not conceive itself as other than an existing object:

[A]ll the particularity of the mind, all that interests us in our thought of ourselves and other minds as relatively permanent objects of thought...we only know by inference from the transient and ever-varying element of inner experience ... I still know myself as *one* and *identical*, perduring through the empirical stream of thoughts, feelings, and volitions (ibid.: 150–151; italics in original).

Put simply, inner self could only be conscious in relation to some external and changing reality or empirical experience. Sidgwick thus disagreed with Kant's

dualistic soul/body distinction, but at the same time he recognised the limitations of the associationist psychology that was used by Mill (Sidgwick 1904: 166, 169).

What significance did this disagreement have for Cambridge economics? One answer related to Sidgwick's use of the idea of evolution, which posited the notion of an 'external and changing reality' as being determinate, and distinguished between different concepts of evolution and their relevance to the theory of rational conduct. More controversially he gave validity to non-rational or psychological forms of reasoning as they had evolved as habits and emotions, as against assuming that conscious 'rational' calculation always produced a superior result:

[W]hat ought we to do when Moral Sentiment comes into conflict with the conclusions of Rational Utilitarianism? Granting that both are really akin and spring from the same root, which ought we to obey, Reason or Instinct? ... [O]ur power of calculating pleasures and pains is so imperfect as to make it really rational in the pursuit of happiness, to disregard the results of conscious calculation when they are clearly in conflict with any of these embodiments of unconscious reasoning and outgrowths of ages of experience (Sidgwick 1876b: 66).

On the other hand, Sidgwick suggested that practical reason was itself an adaptation of the evolved organism to its circumstances, and thus 'if Instinct is really implicit (utilitarian) reason, it is better to perform the calculation explicitly'. Sidgwick's final position, albeit only implicitly rendered, was that neither reason nor instinct should be given a privileged status in deciding which should be followed in any instance—'a plausible case may be made out for either' (*ibid.*)—as both had proved to be fallible.

Sidgwick evidently understood that mental calculation was a complex phenomenon involving both conscious and instinctual components. An evolutionary account of reason therefore provided no case for or against any ethical doctrine as a basis for right conduct, as reason itself progressed over time. In going beyond a purely 'physicalist' approach to understanding mental phenomena, instead positing a complex interaction, Sidgwick was foreshadowing the criticisms of associationism that were articulated at Cambridge by Ward (Cook 2006; Sidgwick 1876b: 54).

How did this rejection of a purely 'conscious' understanding of rationality influence Sidgwick's economics? For one thing it made him more open to a non-individualistic understanding of behaviour. In an essay on economic methodology, he presaged (albeit loosely) a fundamental proposition of contemporary evolutionary biology—inclusive fitness—indicating that the individual maximization approach to explaining behaviour required modification. Sidgwick

explained that ‘ordinary economic man is always understood to be busily providing for a wife and children; so that his dominant motive to industry is rather domestic interest than self-interest, strictly so-called’ (Sidgwick 1885: 29).

By ‘domestic interest’ he meant ‘family interest’, that is, what evolutionary biologists today call inclusive fitness (the interests of an individual’s close family taken as a group), rather than the solitary individual’s self-interest ‘strictly so-called’ taken alone (Buss 2004: 14, 221). In ‘Idiopsychological Ethics’, Sidgwick considered a series of different motives that underpinned human behaviour as a hierarchy of operations, including passions, affections, and sentiments: the ‘love of gain’, categorised as a reflective derivation from appetite, was only one among many (Sidgwick 1887: 42). It is thus evident that he recognised a wider range of rationalities than is sometimes considered as purely economic, and psychological factors were part of this wider scheme.

4 Marshall

Marshall’s economic theory was more dependent on psychological ideas than is sometimes recognised; in order to demonstrate how, the links between the two will be sketched. His early writing most obviously in the psychological vein was ‘Ye Machine’ from 1868 to 1870 (Cook 2009: 108; Raffaelli 2006: 26). It has been characterised as Marshall’s attempt to begin composing a prelude for a general theory of psychology (Groenewegen 1995). Although he never completed the project, the young Marshall studied the associationist psychology of Bain, from whom he derived some of his views, which were carried over into aspects of his economics. For example, in ‘Ye Machine’, Marshall presented a model of a machine with moving internal parts that he used to illustrate how experiences of the outer world impressed themselves onto the inner ‘brain’ of the machine through the mediating external part called the ‘body’. Explaining how ideas, sensations, expectations, desires, habits, and instincts developed, he posited that external inputs caused internal sensations in the brain, which produced associated ideas; these then induced ideas of action, which caused bodily actions (Marshall 1868–1870 [2004]: 116).

A key aspect was that the brain was structured into parts, the cerebellum and the cerebrum. In the cerebellum resided the mechanisms associated with ideas, sensations, and so on. Ideas in the cerebellum caused corresponding ideas in the cerebrum, which in turn acted on other parts of the cerebrum and also back onto the cerebellum. This process of multiple mechanical linkages was a chain of reasoning, which in turn produced deliberations, volitions, and determinations (ibid.: 121): these connections often operated automatically.

Some have judged that ‘no direct connection is proved’ between ‘Ye Machine’ and Marshall’s economic thought (Raffaelli 2006: 29), but an obvious link was provided through the concept of individual personality. Scope for brain individuality was allowed through the character of the machine, which was revealed in how it selected between choices in relation to its ability to think through the consequences of its actions. Character was directly linked to cognitive ability, as greater ‘long-run’ reasoning produced different choices than lesser reasoning capacity in this area (Marshall 1868–1870 [2004]: 130). As will be seen, individual personality had an important place in Marshall’s economics, and it was echoed in Myers’s industrial psychology.

Marshall’s model of the brain was a dualistic one in which mental phenomena had distinct physical correlates (Cook 2009: 120). This formed part of the ‘traditional dualism of Cambridge philosophy’ (Cook 2006), yet was different from Kant’s soul/body division. Even so, the fact that Marshall allowed for individual variation demonstrated that he was not content to reduce mental phenomena to unitary operations. ‘Character’ was constituted from various cognate capacities with mechanical correlates, but as two machines were unlikely to be identical in this respect, individuality was preserved. Marshall was, from early on in his life, interested in understanding the mental underpinnings of difference, even if this was initially explained mechanically.

He gave up on his early interest in psychology, it has reasonably been suggested, because he lacked the skill in experimental psychology that would have been necessary to take the next step in developing his psychological understanding (Groenewegen 1995). However, it is possible to argue that he did not abandon his interest in psychology; rather it was ‘displaced’ into his understanding of aspects of economics, as was evident from his most famous work in this field.

Marshall began his *Principles of Economics* by providing a definition of economics that portrayed it as in part a psychological science. Economics concerned itself ‘chiefly with those desires, aspirations and other affections of human nature, the outward manifestation of which appear as incentives to action’, or with ‘mental states’ observed indirectly through their effect, rather than in themselves (Marshall 1890 [1920]: 15). Economists should be involved with ‘measuring a mental state...by its motor force or the incentive which it affords to action’ (ibid.: 16). A Marshallian definition of economics could therefore be the indirect measurement of mental states through their effects on (and within) the ordinary business of life.

The key aspects of these mental states were wants and desires, which were large in number and various in kind: ‘[M]uch that is of chief interest in the science of wants, is borrowed from the science of efforts and activities’

(*ibid.*: 90). Underpinning these activities were psychological drives, for example, the need for sustenance, recreation, procreation, and so on. The economist

does not attempt to weigh the real value of the higher affectations of our nature against those of the lower ... He estimates the incentives to action by their effects ... [H]e does not ignore the mental and spiritual side of life ... [T]he economist... must concern himself with the ultimate aims of man, and take account of differences in real value between gratifications that are equally powerful incentives to action and have therefore equal economic measures. A study of these measures is only the starting-point of economics: but it is the starting-point (*ibid.*: 16–17).

However, economists were not interested in motives considered individually, that is, in the motives of each person separately, which was the realm of psychology. Instead, they were concerned with motives as they were manifested in larger groups such as classes, nations, districts, trades, industries, and so on (*ibid.*: 26), that is, how motivations were combined within institutions to produce group behaviour.

Marshall provided a clear account of the nature of the motives that economists should study. They came under two basic categories, egoistic and altruistic, and both were very important. Economists deal

not with an abstract or ‘economic’ man; but a man of flesh and blood...influenced by egoistic motives in his business life...but who is also neither above vanity and recklessness, nor below delight in doing his work well for its own sake, or in sacrificing himself for the good of his family, his neighbours, or his country; a man who is not below the love of a virtuous life for its own sake (*ibid.*: 26–27).

As such, economists should attempt to understand, and then model and predict, how different egoistic and altruistic motivations interacted within and between the groups outlined previously, as members attempted to fulfil their economic requirements.

So far, the analysis presented has been on one level, that is, of mental states and their ‘external’ effects on behaviour. But Marshall delved more deeply into the ‘internal’ nature of mental states and brought his early work on psychology to bear more directly as follows:

The physiological basis of purely mental work is not yet well understood; but what little we do know of the growth of the brain structure seems to indicate that practice in any kind of thinking develops new connections between different

parts of the brain ... The mind of the merchant, the lawyer, the physician, and the man of science, becomes gradually equipped with a store of knowledge and a faculty of intuition, which can be obtained in no other way than by the continual application of the best efforts of a powerful thinker for many years (ibid.: 251–252).

Thus, in their daily activities, merchants and entrepreneurs accumulated knowledge and intuition through experience, which was imprinted on brain physiology and revealed itself in practice by means of ‘psychological’ reflex habits, or ‘hunches’ and ‘feelings’, for example, in relation to the ‘search for best-practice technology’ (O’Brien 2006: 625). This idea of psychological habit had a direct equivalent in Ward.

In addition, Marshall demonstrated a good understanding of brain structure for the period:

It seems that the excise of nerve force under the immediate direction of the thinking power residing in the cerebrum has gradually built up a set of connections... between the nerves and nerve centres concerned; and these new connections may be regarded as a sort of capital of nerve force. There is probably something like an organized bureaucracy of the local nerve centres: the medulla, the spinal axis, and the larger ganglia generally acting the part of provincial authorities, and being able after a time to regulate the district and village authorities without troubling the supreme government (Marshall 1890 [1920]: 251, fn. 1).

Marshall characterised accumulated ability (e.g. merchant trading ability and entrepreneurial investment ability) as a form of neurological capital (‘capital of nerve force’), or a physiological development that has a physical basis in changes to brain chemistry brought about through experience and association. He applied the same principle to manual work, as physical dexterity was controlled by the brain and was similarly accumulated.

Here the analysis had parallels with that of Myers on task specialisation within factories and the importance of psychological intelligence to entrepreneurial capability. This emphasis on mental capacity was not limited to the *Principles*. In *Industry and Trade*, Marshall identified what he called the ‘business faculty’ that was needed in successful business leaders, the chief requisites of which were as follows:

- (a) judgment, prudence, enterprise, and fortitude in undertaking and carrying risks: (b) an alert acquaintance with appropriate technique; and some power of initiating advance: (c) a high power of organization; in which system plays a great part, but ‘always as a servant, never as a master’: (d) a power of reading character

in subordinates; together with resolution, tact, trust and sympathy in handling them: (e) prompt diligence in assigning to each the highest work of which he is capable, or can be made capable within a moderate time (Marshall 1919: 355).

Marshall explained that all these qualities were needed in the managerial head of a business 'of even moderate size'. By implication, not everyone possessed the required 'business faculty' to be successful entrepreneurs.

Bringing the various threads of the analysis together, it is evident that obtaining an accurate understanding of the human mind was very important to Marshall's economics. The mind was conceived of as 'an organized system of connections, many of them developed over time through interactions between internal structure and external phenomena and events' (Loasby 2006: 371). In addition, individual minds differed in important ways, partly in terms of their internal structures and partly through their experiences and associations, and various business functions could be understood (at least in part) through such mental differences. Marshall's economics also contained a Myers-like concern for the role of industrial psychology: '[H]uman faculties are as important a means of production as any other kind of capital' (Marshall 1890 [1920]: 229).

This emphasis on mental capacity within business connected to the approach of the Marshallian School of Economics more generally: '[T]he ideas of the school was industrial economics in the broad sense; that is, the... study of the phenomena of the organization of production in firms, grouped in industries or clustered in special districts' (Becattini 2006: 615). In order to understand 'the nature of the firm', the mental qualities of the different actors within it (inventors/entrepreneurs, line managers, and workers) had to be understood, as well as how they interacted within the firm as an institution: these interactions were ultimately interactions of mind.

5 Pigou

Pigou is one of the Cambridge economists least known for using psychology within economic theory. Even so, he did posit the idea that industrial fluctuations could be generated by errors of undue optimism and pessimism, and within this approach were found both explicit and implicit psychological projections.

For Pigou, industrial fluctuations were caused by a 'psychological reflux from the actual experience of good and bad fortune, good fortune breeding errors of optimism, bad fortune errors of pessimism' (Pigou 1927 [1967]: 73). Business events such as good and bad harvests, technological inventions, mineral discoveries, industrial disputes, and so on all had associated psychological impacts. But the main cause of errors of optimism or pessimism was that the

economy was not stationary, nor did it move in a continuous line, rather ‘productivity and desire move in jerks’ (ibid.: 74).

Pigou linked this discontinuity with a potential for mental miscalculation. Forecasting errors occurred whenever a linear path for future growth was assumed, or when changes in evolving psychology were assumed to be continuous. In fact, movements in the economy and business psychology were discontinuous, the latter exhibiting a form of ‘psychological interdependence’ that created herd behaviour via the spread of epidemics of confidence (ibid.: 86). As to the relative importance of psychological factors, Pigou judged that the optimistic or pessimistic error factor was ‘of the same order of magnitude as that of the monetary factor’ (ibid.: 220), or the influence of bank credit.

There is one important aspect of Pigou’s use of psychology which is less frequently emphasised but which is crucial to understanding how it was being employed. In a non-stationary state ‘peopled by perfectly intelligent persons psychological causes...could not exist, since they imply error’ (ibid.: 36). For Pigou, ‘psychological’ factors were inherently mistaken or illusory in nature to some degree, that is, a psychological influence necessarily provided a degree of distortion or a misrepresentation of reality, although not necessarily completely so. This approach can be seen in his analysis of ‘cumulative movements’ in the economy. He characterised ‘cumulation via psychology or expectations’ as ‘much more important’ than mechanical cumulation (Pigou 1941: 243) and showed that psychological projections were subject to over- or underestimation (‘less rosy glasses’) in response to the changing context.

Although there are no references to Cambridge psychologists in the Pigou works discussed here, his use of the idea of psychological error had a direct parallel in the work of Cambridge psychologists like Stout, who had written explicitly on the nature of error. For Stout, error occurred when what was merely apparent appeared to belong to an independent reality (Stout 1902 [1930]: 259), that is, it involved a form of mental confusion, inadvertence, or ignorance. Thinking of mere appearances was a way in which the presentation of unreal things could have real effects, just as, for Pigou, psychological errors in business could have real effects on the economy, despite being based on distorted premises.

6 Keynes

The influence of psychology on Keynes’s ‘mature’ economic theory has been considered elsewhere by the author in an examination of the works of Stout and James Sully, which Keynes studied for the psychology component

of the civil service entrance examination in 1905–1906 (see Barnett 2013: 274, 2015: 308). He read Stout's *Analytic Psychology* as a representative text: Cambridge psychology had a wider resonance within Cambridge University. Both Stout and Sully considered the concepts of expectation (as active motivator) and propensity (in relation to habit), and in first encountering these concepts in 1905, the young Keynes absorbed their psychological connotations before choosing academic economics as his career path.

However, the link between Keynes and Cambridge psychology goes much further than Stout. For example, Keynes knew of Ward's work, as is evidenced by the references to him in Keynes's biographical essays. In his portrait of Marshall, Keynes explained:

Like his two colleagues, Henry Sidgwick and James Ward... Alfred Marshall belonged to the tribe of sages and pastors; yet, like them also, endowed with a double nature, he was a scientist too ... [I]t was to the first side of his nature that he himself preferred to give the pre-eminence. This self should be master, he thought; the second self, servant (Keynes 1924: 321).

This paragraph contains multilevel allusions to psychology. First, Keynes was connecting a Cambridge psychologist (Ward) with two Cambridge economists (Sidgwick and Marshall) and stating that they had character features in common. The second more subtle reference was to character splitting or the dual nature of the human personality, which (according to Keynes) all three individuals had exhibited. Morton Prince published a book called *Dissociation of a Personality* in 1906, which was an original study of what was then called multiple personality.

The third layer related to what Keynes had been discussing prior to the quotation above, namely Marshall's early interest in 'the more progressive study of Psychology', and its 'fascinating inquiries into the possibilities of the higher... development of human faculties' (ibid.: 171). Keynes recognised that for Cambridge economists like Marshall, economics and psychology were intimately connected, but he went further, proposing that the period after 1874–1875, with Foxwell examining the Cambridge Tripos, should be referred to as 'the years of James Ward and J.N. Keynes' (Keynes 1936: 590). In linking his father to Ward, Keynes was acknowledging the influence that Ward had wielded at Cambridge as an institution and that 'mental philosophy' was a wide-ranging discipline.

It is more conventional to make connections between Keynes's Bloomsbury influences and the work of Sigmund Freud (Dostaler 2007), but the broader interest in psychoanalysis at Cambridge has been less recognised by historians

of economics. W.H.R. Rivers, appointed Lecturer in the Physiology of the Special Senses at Cambridge in 1893, drew on Freud's concept of repression in his own work (Forrester 2004: 3–4). Although the psychoanalytic approach was demarcated from psychology even in the 1910s, there was an important degree of overlap between the two subjects. The fact that this intersection had a branch located in Cambridge was significant, even if, in the case of Keynes, this link was mediated via Bloomsbury.

The most well-known reference in Keynes's writings to Freud is to the 'Freudian theory of the love of money' in *A Treatise on Money* as to why gold satisfied subconscious instincts and served as a symbol of the standard of purchasing power (Keynes 1930 [1971]: 258). Freud had written: '[G]old itself stinks. I think that the association is that "miserliness" is "dirty"' (Freud 1954a: 240) and confirmed the 'connection between gold and faeces' (Freud 1954b: 403). Keynes described gold as being seen as a 'furtive Freudian cloak'.

The Freudian analogy only makes sense from one perspective; however, as if gold is a symbol of faeces, then this is the opposite of a stable standard of value. But it is appropriate for those opposed to maintaining a gold standard, as it symbolised the 'miserly' nature of metallic reserves and that the return to a bygone currency system was equivalent to returning to an earlier 'anal' stage of psychological development (Forrester 2004: 19). In *A Treatise on Money* Keynes seemed unsure whether gold was really a 'Freudian cloak', as he judged: '[W]e need not be curious to enquire' (Keynes 1930 [1971]: 259).

He was more forthcoming in a letter to *The Nation and Athenaeum*: the case for considering Freudian theories seriously depended on 'the appeal which they make to our own intuitions as containing something new and true about the way in which human psychology works' (Keynes quoted in King 2010: 12). For Keynes, what was original in Freud was not the specific psychoanalytic ideas themselves, but how the psychoanalytic method provided insights into the mechanisms by which psychological factors generated behaviour, for example, through the role of the subconscious. Freud's tripartite division of the mind into ego, superego, and id provided a structure by which 'hidden' factors could influence the conscious mind, and this mechanism was an important part of Keynes's employment of psychology in his economic analysis (Barnett 2013: 230–233, 2015, 325–329).

Although the concept of 'psychological poverty' appeared in *A Treatise on Money* (Keynes 1930 [1971]: 176), the most well-known use of psychology in Keynes's economics was the 'fundamental psychological law' in *The General Theory* that individuals increased their consumption as their income increased, but not by as much as the income increase (Keynes 1936 [1973]: 96). It has been suggested that he obtained this 'law' from the

income/expenditure analysis of Jens Warming (Laidler 1999: 251). Keynes outlined that it came from two sources: a priori knowledge of human nature and the facts of experience, the latter being more plausibly attributed to Warming's analysis.

The 'human nature' in question was evidently psychological in origin, since whether to consume or to save income beyond a subsistence level was a choice undertaken through mental decision-making processes. Two other fundamental psychological factors—the liquidity attitude and expected capital asset yield—made up the first of *The General Theory's* three ultimate independent variables that determined the level of employment. In using the concept of expectation directly in association with psychology, this brought Keynes full circle, back to Stout's account of expectation as he had first encountered it whilst studying for the psychology part of the civil service examination (Barnett 2013: 274, 2015: 313–314).

7 Conclusion

Has this chapter confirmed or disproved Sidgwick's notion of a 'Cambridge mind' that held certain interests in greater regard than others? In one sense it has: psychology at Cambridge through this period had certain features that distinguished it from the Continental approach and also from the earlier British tradition. This was also found with industrial psychology, which had special affinities with Cambridge both institutionally and intellectually. In turn, this had a mediated (if moderate) influence on some aspects of Cambridge economics.

However, on another level, Cambridge psychology was part of a wider tradition of British psychology 'from the seventeenth century onwards' (Wright 1944: 481), whereas Cambridge economics has conventionally been seen as separate from economics as found in other UK universities, for example, compared to the LSE approach in London (Becattini 2006: 612). The examples of Marshall and Keynes demonstrated that economists took specific ideas from the earlier associationist tradition and also the Cambridge School of Psychology, without necessarily adopting the underlying viewpoint of either.

Simon Cook has argued that Marshall's approach to psychology was 'out of step' with the psychology developed within the Cambridge Moral Sciences Tripos, and consequently it could be seen as differing from that promoted by Ward (Cook 2006), and this is accurate to a large degree. However, Cook's portrait of Cambridge psychology is incomplete as he does not discuss the role of Myers in developing, albeit sometime later, the field of industrial

psychology in the 1920s, where methodological similarities between Cambridge psychology and Cambridge economics were most evident. Given the time factor involved, the influence most likely ran from Marshall to Myers.

Another thread of the discussion was differing conceptions of the nature of psychological explanations within economics, as was demonstrated by comparing Pigou with Keynes. For Pigou, a psychological factor meant an illusory one, that is, some type of mental mistake of comprehension or processing. For Keynes, however, psychological factors, while they could be illusory, were not necessarily so: in some circumstances, abiding by psychological reasoning could be appropriate and even 'rational' (Barnett 2015: 325–326). Hence for Pigou 'psychological' was a synonym for non-rational, whereas for Keynes 'psychological' could mean either rational or non-rational, depending on the situation.

Finally, it is useful to compare the nature of the psychological factors that were employed. For Sidgwick, the opposite element to rational calculation was instinct, and hence psychological factors as elements of instincts were part of the evolutionary heritage of humanity. For Marshall, psychology had a major connection to physiology, and hence psychological factors had an underlying physical/neurological basis to them. For Pigou, psychology was created through mental processes of distortion, and no underlying 'real' basis to them was provided. For Keynes, psychology had both physiological and Freudian/unconscious components, that is, the psyche had levels of operation, and the interactions between these levels partly explained the nature of behaviour.

Despite these differences in understanding how psychological factors operated, some common Cambridge threads can be discerned, not least in the fact that psychology was considered as important by all four economists, although in varying degrees. In descending order, the ranking was Marshall, Keynes, Sidgwick, and then Pigou. In the cases of Marshall and Keynes, the influence of the associationist tradition was discernable. In Marshall, this came most directly from Bain, and in Keynes, it came mediated via Marshall and more directly from Stout. Even so, it is plausible to conclude that economics and psychology on the Cam had some common points of intersection from the last quarter of the nineteenth century until the Second World War. The story after 1945 would, however, be a very different one.

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7

Post Keynesian Economics in Cambridge

John E. King

1 Introduction

In this chapter I draw on the substantial secondary literature on the first generation of Cambridge Post Keynesians, which was itself informed by the extensive unpublished correspondence and other manuscript evidence left by Richard Goodwin, Richard Kahn, Nicholas Kaldor, Joan Robinson and Piero Sraffa, together with their correspondence with the American Post Keynesian, Sidney Weintraub. First, in Section 2, I deal with some tricky questions of terminology. Then, in Section 3, I outline the work of the Cambridge Post Keynesians before the death of John Maynard Keynes in 1946. Section 4 assesses their achievements in the next 10 years, while Section 5 is concerned with the reasons why the ensuing decade and a half did not constitute a Golden Age for them, as might reasonably have been predicted in 1956. Ending my story in 1973—the publication date of the important but unsuccessful introductory textbook by Joan Robinson and John Eatwell—in Section 6, I try to explain the failure of the Cambridge Post Keynesians. A brief epilogue is provided in Section 7.

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2 What's in a Name?

First of all, there is a question of terminology. Luigi Pasinetti's 2007 book *Keynes and the Cambridge Keynesians* is divided into three parts. Book one, 'Keynes's Unaccomplished Revolution', sets out the author's views on the revolutionary potential of Keynes's *General Theory* and on the failure of Keynesian economists to realise that potential. Book two, 'The Cambridge school of Keynesian economics', contains biographical sketches of Sraffa, Joan Robinson, Kaldor, Kahn and Goodwin and adds some critical reflections 11 years after the death of the last of these great 'Cambridge Keynesians' (Goodwin, who died in 1996). In the third and final part, 'Towards a production paradigm for an expanding economy', Pasinetti sets out his own theoretical system, which is intended to fill the large gap left by Keynes and his Cambridge disciples (Pasinetti 2007).

Pasinetti covers very much the same ground as Geoff Harcourt's book on *The Structure of Post-Keynesian Economics*, which was published a year earlier and (despite its title) is devoted almost entirely to Cambridge economists (Harcourt 2006a). The appendix that Harcourt devotes to 'biographical sketches of the pioneers' contains brief accounts of the contributions of Keynes, Sraffa, Joan Robinson, Kahn and Kaldor—and Michał Kalecki, who was not a Cambridge economist but (as we shall see in Section 3) might well have become one. Not surprisingly, Harcourt's review of Pasinetti's book was detailed and enthusiastic (Harcourt 2009).

Pasinetti himself begins by quoting Keynes's well-known 1935 letter to George Bernard Shaw: 'To understand my state of mind, however, you have to know that I believe myself to be writing a book on economic theory which will largely revolutionise—not, I suppose, at once but in the course of the next 10 years—the way the world thinks about economic problems' (Keynes 1973: 492, cited by Pasinetti 2007: 6). Pasinetti then outlines the sequence in which Keynes developed his revolutionary new theory between 1930 and 1932, invoking Thomas Kuhn's 1962 *Structure of Scientific Revolutions*. On Pasinetti's interpretation, Keynes's critical break with orthodoxy was his rejection of the traditional 'exchange paradigm' in favour of an alternative 'production paradigm'. Only in this context, Pasinetti argues, can the principle of effective demand be correctly understood (Pasinetti *ibid.*: 23–24).

Mainstream theorists like John Hicks had managed successfully to 'accommodate' Keynes by 'trying to insert the Keynesian innovations into the

prevailing paradigm', Pasinetti suggests. In this they were assisted by 'some of the more conservative-minded young economists of the Keynesian group, especially Harrod and Meade' (ibid.: 30). The effect was profound: '[I]t meant reducing Keynes's intended revolution to be no revolution at all' (ibid.: 33). The true Cambridge Keynesians—to repeat, for Pasinetti these were Robinson, Kahn, Sraffa, Kaldor and Goodwin—'made a remarkable effort to carry Keynes's revolution to a reasonably advanced stage' (ibid.: 34), but they failed to do so for a variety of reasons that are described in the second part of Pasinetti's book. Thus, Pasinetti concludes, 'the genuine Keynesian revolution...has so far remained unaccomplished' (ibid.: 50).

The attentive reader will have noticed the small but significant difference in the titles of the two books: Cambridge *Keynesians* (Pasinetti) versus (Cambridge) *Post-Keynesians* (Harcourt). Both have their problems. The former is too inclusive, since it could fairly be claimed not only by theoretical supporters of Old, neoclassical, 'bastard' or 'IS-LM Keynesianism' (Harcourt 2006a: 4), like James Meade, but also by applied economists such as Brian Reddaway and Richard Stone, and individualists such as David Champernowne, who were all happy to accept the label 'Keynesian' but would have been much less comfortable being closely associated with Harcourt's 'Post-Keynesian' pioneers.

The latter is anachronistic, since it was only in the mid-1970s—when the intended revolution was already 40 years in the making—that the term 'Post-Keynesian' ceased to be used in a purely chronological sense to refer to work published after 1936 and came to be associated instead unambiguously with a particular interpretation of what the 'Keynesian Revolution' was or ought to have been. Thus, Kenneth Kurihara's edited volume *Post-Keynesian Economics* contained several papers written from a neoclassical Keynesian or IS-LM perspective (Kurihara 1954), and around this time both Kaldor and Robinson were using the term in its chronological sense. The term 'Neo-Keynesian' was also often adopted in the 1960s and early 1970s to describe what later became known as 'Post Keynesian' theory, and Robinson flirted with the idea of an 'Anglo-Italian school' that recognised the contributions of Sraffa and several of his younger Italian colleagues, including Pasinetti (King 2002: 9–10).

In deciding on the title of this chapter, I have (more or less) followed Harcourt rather than Pasinetti, but not without misgivings, both substantive and grammatical. The two are connected. There are four ways of writing 'Post Keynesian', with or without a hyphen, with the 'p' in lower case or capitals. My grammar school English teachers would probably have insisted that the only

correct version was ‘post-Keynesian’, and this (as Marc Lavoie has reminded me) is the usage that the Cambridge pioneers invariably adopted. It was not popular in North America. When Paul Davidson and Sidney Weintraub launched their journal in 1978, they called it the *Journal of Post Keynesian Economics*, using ‘Post Keynesian’ (capital letters, no hyphen) to emphasise the doctrinal rather than the chronological meaning of the term. This is the form that I have always employed. ‘Post-Keynesian’ is arguably wrong on both counts (and was only adopted by Harcourt at the insistence of his publishers), but it is clearly preferable to Pasinetti’s ‘Cambridge Keynesians’, for the (doctrinal) reasons already stated.

3 While Keynes Lived (Pre-1946)

The early history of the Cambridge Post Keynesians is—mercifully—rather less complicated. It begins in 1930, with the publication of Keynes’s *A Treatise on Money* and the emergence of an informal discussion group consisting of some of his younger colleagues that came to be known (after the complex intersection in the Charing Cross Road in Central London) as the Cambridge Circus. Its members included Kahn, Austin and Joan Robinson, Sraffa and Meade, but not the rather younger Kaldor, who was still a student at the London School of Economics (LSE). They attempted, eventually successfully, to persuade Keynes to abandon the Wicksellian theoretical foundations of the *Treatise*, in which macroeconomic adjustment takes place through movements in the rate of interest and the price level but not in the volume of output and the level of employment. In the circumstances of 1930–1931, with the Great Depression intensifying and unemployment rising sharply in every capitalist country in the world, this was easier than it would have been a few years earlier.

The work of the Circus is recorded in some detail in the two principal biographies of Keynes (Moggridge 1992: 531–536; Skidelsky 1992: 531–536), in earlier work by Harcourt (1985, 1994) and in the reminiscences of two of the participants (Kahn 1984; Robinson 1978). It ceased to operate in mid-1931, having accomplished its mission. Between then and the publication of *The General Theory* in February 1936, the young economists extended their work on the fundamentals of macroeconomic theory and continued to exert some influence over Keynes. The contributions of Harrod and Meade in the mid-1930s do not belong here, since both were now at Oxford rather than Cambridge (as Harrod had always been), and the peculiar nature of Sraffa’s work will be discussed below. Most important were Richard Kahn

and Joan Robinson. Kahn's great contribution to the writing of *The General Theory* is recorded in the chapter devoted to him in this volume. Robinson's *Essays in the Theory of Employment* were substantially complete by the end of 1935 (Robinson 1937). Somewhat uneven in quality, they dealt with the labour market, the macroeconomics of the long period, the open economy and issues of economic policy and methodology. At their best, these essays are 'full of insight and imagination, shedding light on obscure corners of the *General Theory*, probing some of its crucial weaknesses, and anticipating many of the awkward questions which Sidney Weintraub, Paul Davidson and their North American colleagues were later to ask of the neoclassical "Keynesian" synthesis' (King 1996: 165).

As for Sraffa, he mounted a brief but incisive attack on the Wicksellian economics of Friedrich von Hayek, denying the existence of a single 'natural' rate of interest and questioning both the operation of any mechanism that established macroeconomic equilibrium at full employment and the validity of marginalist theory more generally (Hayek 1931, 1932; Sraffa 1932a, b; Roncaglia 2009: 30–34). Thereafter Sraffa retreated into himself, concentrating on the editorial work on David Ricardo for which he became famous but publishing nothing on this or any other topic until volume I of Ricardo's *Collected Works* finally appeared in 1951.

Sraffa's views on *The General Theory* remain a matter of some controversy. Based on a close examination of his unpublished papers, Heinz Kurz concludes that Sraffa was severely critical of Keynes on several theoretical questions. His objections related, first, to the argument in *A Treatise on Money* that the price level of consumption goods was determined independently of the price level of investment goods; second, to the notion of the marginal efficiency of capital set out in *The General Theory*; third, to the view that the money supply was controlled by the banking system, and therefore determined exogenously; and, fourth, to the liquidity preference theory of interest. 'In important respects', Kurz concludes, 'he felt that Keynes had granted too much to received economic theory. Keynes's new theory exhibited several loose ends and contradictions and retained in new garb marginalist concepts that Sraffa deemed untenable' (Kurz 2010: 201).

I have two reservations about this conclusion. First, while these issues are significant they are not really central to the main concerns of *The General Theory*. They do not go to the principle of effective demand, the explanation of involuntary unemployment or the critique of Say's Law (all different ways of summarising Keynes's fundamental message). Each one of Sraffa's criticisms could be accepted without seriously damaging the core of Keynes's argument, and indeed his third—endogenous money—has been developed by Post Keynesian

theorists like Kaldor and Basil Moore to strengthen the revolutionary core of Keynesian macroeconomics. Whether Sraffa himself realised this, and what he thought of that analytical core, cannot be easily inferred from Kurz's account of the archival evidence.

Second, Sraffa's criticisms have little or no bearing on the main policy implications of *The General Theory*, variously characterised as cheap money (Tily 2007), counter-cyclical fiscal policy based on 'functional finance' rather than 'sound finance' (Lerner 1943) and a substantial degree of direct regulation of private sector investment by the state, the 'somewhat comprehensive socialisation of investment' alluded to by Keynes himself in *The General Theory* (Keynes 1936 [1973]: 378). Again, Sraffa seems to have remained silent on all these questions, in print and out of it. He was certainly a man of the Left, and Pasinetti assumes that he was also a Keynesian on matters of macroeconomic policy, but there is again no direct evidence, either way.

Meanwhile, an outsider had briefly joined the ranks of the Post Keynesian pioneers. Once described by Geoff Harcourt as probably the best all-round economist of the twentieth century (ahead of Keynes), the Polish émigré Michał Kalecki turned up in Cambridge in November 1937, looking for work. The brief success and eventually conclusive failure of his quest are described in some detail in the recent biography of Kalecki by Jan Toporowski (2013: Chapters 11–14). Kalecki had already achieved a great deal before his arrival in Cambridge, not least the publication of a remarkable mathematical model of the trade cycle in the newly established journal *Econometrica* (Kalecki 1935), and Keynes described him as 'something of a genius' (Keynes quoted in Toporowski *ibid.*: 109). But he was also a foreigner, had no teaching experience and no university degree (merely a diploma in engineering). Kalecki spoke dreadful English and had few, if any, social graces; being Jewish would probably also not have helped him (though it seems to have done Kahn and Sraffa no great harm).

At all events, Kalecki was not a strong candidate for a college fellowship. Instead he needed a research job, and at the beginning of 1938 Cambridge had none to offer. Keynes, Kahn and Robinson did their best to help him find employment elsewhere, and Sraffa tided him over with a loan. Finally, at the end of the year, the Cambridge Research Scheme began operation, with Kalecki as the only full-time employee. He already had some enemies, not least the powerful chair of the Faculty Board, Dennis Robertson, who regarded trade cycle theory as his personal property and did not welcome interlopers. The microeconomic focus of the research that he was called on to perform did not suit Kalecki, and by mid-1939 criticism of his work from Arthur Bowley, Richard Stone and even from Keynes himself produced a response from Kalecki of 'barely suppressed outrage' (*ibid.*: 133).

He soon left Cambridge for the more congenial surroundings of the Oxford Institute of Statistics, where he worked most productively during the war years before leaving Britain in 1945 to spend the remainder of his career briefly in Canada and then in the USA and Poland. Oxford's gain was very clearly Cambridge's loss, and in Section 6 I shall speculate on the benefits that the Cambridge Post Keynesians might have obtained if Kalecki (who died in 1970) had remained there for the final three decades of his life.

The General Theory was a poorly structured and in some ways also a poorly written book, which left many questions unanswered and many theoretical issues unresolved. Keynes was aware of the problems that this posed, writing a summary and clarification of the arguments for his American readers (Keynes 1937) and promising Ralph Hawtrey that he would produce a set of 'footnotes' to the book dealing with the more important criticisms that had been made. It is tempting to believe that these 'footnotes' might have been expanded into a substantially revised second edition of *The General Theory*. Keynes's ill health—he suffered a severe heart attack early in 1937—and his work as a more than full-time public servant during the Second World War prevented him from even beginning work on any such project before his death in 1946. We can only guess at what he might have achieved, and what subsequent difficulties might have been prevented. The contributors to the two-volume 'second edition' of *The General Theory*, edited by Harcourt and Riach (1997), offer some valuable suggestions.

The war did have one beneficial effect for the Cambridge Post Keynesians. This was the arrival of Nicholas Kaldor, with the relocation of the LSE to Cambridge. Kaldor had already abandoned his support for Hayekian macroeconomics, and between 1940 and 1944 he made important contributions in a Post Keynesian vein to both theory and policy, publishing an ambitious model of the trade cycle that attempted to formalise the informal insights that he had found in Chapter 22 of *The General Theory* and producing a series of detailed empirical studies that estimated the necessary fiscal policy conditions for maintaining full employment after the war (see King 2009: Chapter 3). A war Circus began to operate, involving Kaldor, Robinson, Sraffa and (when he could escape from official duties in London) Kahn. Kaldor and Robinson became especially close. They cooperated on plans for the socialist reconstruction of Britain in peacetime, adding a microeconomic dimension to the existing programme for a macroeconomics of full employment without inflation (Harcourt and Kerr 2009: Chapter 5; King 2004).

Sraffa's involvement in the war Circus was more social than intellectual. We have seen that there are grounds for believing that he was secretly sceptical about some aspects of *The General Theory*. There is even less evidence concerning

Keynes's opinion of the Sraffian system. Here the responsibility is entirely Sraffa's. Deeply, obsessively secretive, Sraffa after 1928 seems to have disclosed nothing to anyone (except the mathematician Abram Besicovitch) about the formal rehabilitation of classical political economy that he would eventually publish 32 years later as *Production of Commodities by Means of Commodities* (Sraffa 1960). We do not know what Keynes made of the early disclosures (nor indeed how detailed they were), and we can only guess what he would have made of *Production of Commodities* had an early draft been offered to him before his death.

We can, however, make an informed guess. Sraffa believed (correctly) that he had fatally undermined the Marshallian theory of value and distribution. This belief could be inferred, more or less, from his well-known 1926 *Economic Journal* article, which had been published by Keynes himself as editor (Sraffa 1926). Yet a strong case can be made that Keynes remained a Marshallian right down to *The General Theory* and beyond, as Paul Davidson and other 'fundamentalist Keynesians' have always maintained (see Hayes 2006 for a very cogent restatement of this view), along with authors as diverse as Leijonhufvud (2006) and Kurz (2010: 185). Certainly, Keynes had very little time for Ricardo. Sraffa must have known all this, and he must have disapproved of it. As for Keynes, it is hard to see him responding favourably to Sraffa's system and its 'prelude to a critique'—of Marshall, above all others.

From his arrival in Cambridge in 1927, Sraffa was one of Keynes's closest friends. Just how much time they spent in each other's company in the late 1920s and 1930s will never be known, and the folklore on this question is not entirely convincing: as one who has spent more than his fair share of hours trawling through the second-hand bookshops of Melbourne (population four million), I find it hard to believe that Keynes and Sraffa could have occupied very much time in this way in interwar Cambridge (population 50,000), as is sometimes claimed. If they really never did speak about economic theory—or policy—on their excursions, it can only have been as the result of an (unspoken?) agreement not to jeopardise a valued friendship by potentially acrimonious debate. As Bob Cord reminds me (personal communication, 4 June 2015), this was something of a Cambridge tradition: A.C. Pigou refused to talk about economics unless it was absolutely necessary.

Pasinetti's verdict is that it 'seems reasonable to conclude that it is difficult to find clear evidence supporting the view of a relevant influence, on scientific grounds, of Keynes on Sraffa; and at the same time that it is equally difficult to find clear evidence of a substantial influence of Sraffa on Keynes' (Pasinetti 2007: 194). But this misses the crucial point, which is not about influence.

Did Keynes and Sraffa actually *agree* on fundamentals? That is the central issue, which Pasinetti simply avoids. I shall return to it in Section 6.

4 After Keynes: The First 10 Years (1946–1956)

At the end of 1946, eight months after Keynes's death in April of that year, the status of the Cambridge Post Keynesians was as follows. Kahn was a Fellow and also the Bursar of King's, and a University Lecturer; he would be appointed Professor in the Faculty of Economics in 1951. Sraffa was a Fellow of Trinity, and had been since 1939. Joan Robinson was a University Lecturer in Economics, and while she was promoted to Reader in 1949 she would not be elected to a Fellowship in any college until the 1960s. Kaldor's principal residence was in Cambridge, but it was not his place of employment. In 1945, a proposal to appoint him to a University Lectureship had been rejected by four votes to two, Sraffa and the philosopher Richard Braithwaite voting in favour and Dennis Robertson, along with Gerald Shove, C.R. Fay and J.W.F. Rowe, voting against (Marcuzzo and Rosselli 2005: 16, fn. 35). Kaldor went off to work for the United Nations Economic Commission for Europe in Geneva and only returned to live in Cambridge in October 1949, as a newly elected Fellow of King's. In the same year, Goodwin arrived in Cambridge to work with Richard Stone at the Department of Applied Economics, soon becoming a University Lecturer and Fellow of Peterhouse.

As the initial rejection of Kaldor suggests, there was a strong undercurrent of opposition in Cambridge to the ideas of the Post Keynesian pioneers. It was led by Robertson, who had been appointed Professor in 1944 on the retirement of another of their opponents, A.C. Pigou. Nevertheless, the decade after 1946 saw some remarkably intense theoretical work by the Post Keynesians and, from two of them, a torrent of important publications. The most prolific was Joan Robinson. As her biographers tell us, it is 'quite extraordinary on how many fronts she was advancing virtually simultaneously' in this decade (Harcourt and Kerr 2009: 79). Robinson's bibliography records articles on Marxian political economy, the trade cycle, capital theory and the analysis of economic growth, the last culminating in what was intended to be her masterpiece, the 435-page *Accumulation of Capital* (Robinson 1956).

Kaldor, too, began to publish theoretical papers on the trade cycle and economic growth, and his service as a member of the Royal Commission on the Taxation of Profits and Income in 1950–1955 inspired a very well-received

book in which he advocated the introduction of *An Expenditure Tax* (Kaldor 1955). Crucially, he expanded a 1950 encyclopedia entry on the relative income shares of wages, profits and rent into an important article in the *Review of Economic Studies* on 'Alternative Theories of Distribution' (Kaldor 1956).

Neither Goodwin nor Kahn published a great deal in this decade. Throughout his career, in fact, 'Kahn himself wrote comparatively little', as John Eatwell recalled, 'his ideas appearing most often in the writings of others' (Eatwell 2000: 336). Meanwhile, with the indispensable assistance of Maurice Dobb, Sraffa had at last published the ten volumes of his majestic edition of *The Works and Correspondence of David Ricardo*, which appeared between 1951 and 1955 (the 11th volume, containing the index to the previous 10, was delayed for another 18 years). He was still ruminating on his own contribution to the revival of classical political economy, which was eventually published as *Production of Commodities by Means of Commodities* more than 30 years after he had begun work on it (Sraffa 1960).

The year 1956 should have been the *annus mirabilis* of the Cambridge Post Keynesians, with the appearance of Robinson's book and Kaldor's influential article filling some of the most important gaps in *The General Theory*. Instead it became something of an *annus horribilis* as these two strong personalities clashed, initially at a great distance while Kaldor toured the world on a one-year sabbatical that took up the entire calendar year and then, for the next five years or more, at close quarters in Cambridge (King 1998: 416–429). Robinson was aggrieved—and Kahn even more so, on her behalf—that Kaldor refused to regard her book as a major analytical breakthrough, while he complained that she had given far too much ground to the enemy. 'But the main point on which we differ', he told her in an angry letter in 1960, 'is your thorough-going neoclassicism which, despite all your protests, permeates your thinking on this whole business. On the matter of the rate of profit, the choice of techniques and the amount of capital per head, your position is thoroughly orthodox and entirely wrong' (cited in *ibid.*: 425). After 1962 their correspondence more or less ceased, and although fences were mended many years later when Kaldor was the prime mover in obtaining an Honorary Fellowship at King's for Robinson, and his obituary for her was suitably laudatory (Kaldor 1984), they never cooperated again. If one necessary condition for the success of a school of thought is the existence of a recognised leader, or leadership team, as the editor of this volume has convincingly argued (Cord 2013), then after 1956 there was never any prospect that the Cambridge Post Keynesians might satisfy it.

5 The Golden Age That Wasn't (1956–1973)

The split between Kaldor and Robinson was all the more tragic in that they continued to agree both on the most fundamental issues on which they had already worked (growth, distribution and capital theory) and on the new questions of methodology to which they soon turned (path dependence, hysteresis, the limitations of equilibrium modelling and the role of history). The next 15 years were a period of great intellectual achievement for both Kaldor and Robinson (see King 2002: Chapter 3). The intense and sometimes venomous ‘Cambridge controversies in the theory of capital’ that erupted after the publication of Sraffa’s slim volume in 1960 embroiled leading theorists from Cambridge, Massachusetts, notably Paul Samuelson and Robert Solow, in a desperate attempt to salvage the neoclassical theory of capital from the deep criticisms that first Robinson and then Sraffa had made in print. By 1966, it was clear that Cambridge, England, had won (Harcourt 1972), though in the second decade of the twenty-first century you could be forgiven for not realising it, as the great majority of the economics profession continues to use neoclassical capital, distribution and growth theory as if the Cambridge controversies had never happened (Cohen and Harcourt 2003).

The era of cheap air travel that began in the mid- to late 1960s led to increasingly close contact between Cambridge economics and the rest of the world. Among the Cambridge Post Keynesians, Kaldor and Robinson especially became great travellers, with the former’s 1956 world tour taking in India, Japan, Chile and the USA. American visitors to Cambridge on sabbatical leave in this period included Samuelson and Solow, the most prominent of the neoclassical Keynesians, and also Kenneth Arrow. There had always been foreign research students, with talented young economists arriving from as far away as Australia since the 1930s (Millmow 2015), and they became even more numerous in the post-war period. Some of them stayed. The two intellectual historians with whom this chapter began were, of course, themselves immigrants. Harcourt came back to Cambridge in 1963–1966, 1972–1973 and 1980, and again for a much longer spell that began in 1982 and ended with his return to Australia in 2010. Pasinetti was a University Lecturer (later Reader) and Fellow of King’s from 1961 to 1976 before he moved back permanently to Italy.

Their hosts were not, however, always as welcoming as they might have been. Harcourt has often recalled his harsh treatment by Kaldor when the callow young colonial boy dared to visit his potential PhD supervisor out of term (e.g. in King 1995: 168–169). In this case, the overseas student recovered

from being sent away with a flea in his ear and became a satisfied long-term resident, but others were less happy. The subsequently eminent trade theorist Murray Kemp, who was no blushing violet, recalled how, in 1959–1960,

Cambridge was a great disappointment, as there was no provision for graduate work and I was postgraduate. There were six Australian graduate students, PhD students. I think it was the experience of each of them that they'd made a huge mistake in coming to England. They all had nominal supervisors but had no real guidance and no course work (Kemp quoted in Coleman 2005: 7).

Harcourt responded angrily to Kemp, denying that he was the only Australian of that cohort to successfully complete his degree and reporting on the seminars and lectures that had been organised for them, with the active participation of Kahn and Robinson. 'Sraffa', he conceded, 'was a little aloof as he was still recovering from falling off a mountain and cracking his skull' (Harcourt 2006b: 147).

Sraffa's injury seems to have had long-term consequences. One subsequent visitor to Cambridge from Italy offered me the following recollections:

In 1969 I went to Cambridge to meet Maurice Dobb. I carried a letter of introduction from the cultural section of the Central Committee of the Communist Party of Italy. He introduced me to Sraffa. My question was: 'to become a functioning communist one must know economics, as Marx realised. Hence, should I come here to study or stay in Rome?' Answer by Sraffa, paraphrasing: 'forget it'. And then, quoting him: 'in mathematics [I was studying maths at the University of Rome then] you learn, you learn, you learn; in economics you talk, you talk, you talk'. End of Sraffa's contribution. Dobb began to disagree with him, and said: 'This is not what Joseph wants. He wants to know whether studying contemporary economics is worthwhile from a Marxist point of view.' No response from Sraffa. Later at coffee I asked him if I could get some working papers or lecture notes from him, and he said he had none. End of story. Dobb then took me for a walk and suggested I stay in Italy: 'you will get much more there than here', he intimated (Joseph Halevi, personal communication, 29 June 2011).

Five years later Halevi returned to Cambridge for three months to use the University library and work on Kalecki. He was officially sponsored by Kaldor and by Bob Rowthorn, and had some useful discussions with Pasinetti. For the most part, however,

It was awful! Kaldor told me right away that Kalecki was bad and his oligopoly theory was bunk, and that I should change my thesis into one on Kaldor himself ... With Joan Robinson it was fight at first sight. I dared ask her whether she thought that either the Kaldor-Pasinetti or the Kalecki consumption functions were too passive. What would happen, I asked, if we brought in a Modigliani-Ando type of consumption function? These were genuinely naïve questions, motivated only by trying to find out what was objective and what was subjective in what was then modern theory (Marx would have done exactly the same). Answer by Robinson, verbatim: ‘This is a free country. You can always leave and go to the United States’ (ibid.).

It was not only Italians who suffered. Some indication of the difficulties that the Cambridge Post Keynesians caused themselves can be seen from the chequered history of their early contacts with the pioneering American Post Keynesian, Sidney Weintraub, who had spent the academic year 1938–1939 at the LSE, where he had been on friendly terms with Kaldor. When, in 1960, Weintraub sent Kahn a copy of his recently published paper on wage inflation, which was sharply critical of IS-LM Keynesianism (Weintraub 1960), he received an unexpectedly patronising reply recommending that he read Kahn’s evidence to the Radcliffe Committee and three papers by Joan Robinson dating from 1937, 1943 and 1958. ‘The message was brutally clear. Cambridge had (of course) got there first. Weintraub was nothing more than a colonial interloper’ (King 2008: 144). This was nothing new; Kahn’s (sadly unpublished) letters to Robinson in 1933 during his US lecture tour read like the reactions of a visitor to a high-class zoo on encountering its wide range of exotic but amusing animals. (They are in the Richard Kahn Papers at King’s College Archive Centre, Cambridge University: RKP 13/90/1.)

Weintraub was sufficiently upset by Kahn’s response that he referred to it many years later in correspondence with both Geoff Harcourt and Jan Kregel. His own reply was passed on by Kahn to Robinson, who offered a rather backhanded apology that included this revealing statement: ‘On the main question this is evidently a “semantic misunderstanding”. For you “Keynes’ theory” means some latter-day nonsense, while to us, the old guard, it is a coherent system of analysis in which the wage-price relation is an essential part’ (Robinson quoted in King 2008: 145). The dismissal of IS-LM as ‘some latter-day nonsense’ (in 1961!) testifies to the Cambridge Post Keynesians’ long-term neglect of the Old Keynesian or neoclassical synthesis, which they evidently regarded as being beneath contempt.

Fences were eventually mended, with Weintraub visiting Cambridge and Robinson meeting up with him at the University of Waterloo in Canada,

where he had a two-year appointment in 1969–1971; she had grandchildren nearby. But the Cambridge arrogance was apparent again in the story of Paul Davidson's appalling treatment at the hands of Robinson and Kahn in 1970 or 1971. They walked into his seminar several minutes late, Robinson took over the blackboard and spent the next 15 minutes explaining where Davidson was wrong, and they then walked out again without allowing him to reply (King 1994: 367–368).

This cannot have encouraged transatlantic cooperation. (Would you send your best students to do postgraduate work at Cambridge after you had been told that story? Would you encourage younger colleagues, at a critical stage in their careers, to apply for jobs there?) Once again the Americans proved forgiving. When Davidson and Weintraub started the *Journal of Post Keynesian Economics* in 1977, Kaldor was one of ten members of the Honorary Board of Editors, along with Harcourt; Robinson had been offered the honour, but declined. Of the 53 members of the 'Managing Board of Editors' however, only three (Wynne Godley, Jan Kregel and Pasinetti) had Cambridge connections.

6 The Age of Decline (1973–2016)

By this time the decline of Cambridge Post Keynesianism was already evident. The British system of compulsory retirement at age 67 saw Sraffa removed from the University payroll in 1965, followed by Robinson in 1971, Kahn in 1972 and Kaldor in 1975. Of course, this did not stop them writing for publication (and it would have been unnecessary to achieve this in the case of Sraffa, who had already stopped), but it did further reduce their ability to influence appointments, course content and the general climate of opinion in Cambridge economics. Famously, the retirement of Kahn required only one capital letter to be changed on the door of his professorial office, as he was replaced by the devoutly neoclassical general equilibrium theorist Frank Hahn. The second generation of Cambridge Post Keynesians was not favoured in the promotion process. Efforts to obtain a chair for Pasinetti and for Kaldor's brilliant young protégé, Adrian Wood, both failed, resulting in their departure from Cambridge, Wood going to Sussex and Pasinetti returning home to Italy.

The relatively few remaining Post Keynesians in Cambridge did, at long last, have an introductory textbook to teach from, with the publication of *An Introduction to Modern Economics* by Robinson and Eatwell (1973). Sadly, it proved to be both a commercial and a pedagogic failure. Robinson's intention had been to write a book that would capture the American market and knock

Samuelson off his perch. But she had little or no experience of first-year teaching, and she proved unwilling to respond to comments from the small group of highly able students who had been recruited to comment on early drafts of the text. The book proved to be extremely idiosyncratic in both structure and content, so much so that—unlike Samuelson's *Economics*—it did not really feel like a textbook. Reviewers deemed it too difficult for beginners, and it sold poorly. A second, paperback, edition appeared in 1974 but it fared no better and the book soon sank, almost without trace (King and Millmow 2003; Harcourt and Kerr (2009: Chapter 10) offer a rather more favourable assessment).

Its failure was more a symptom of decline than a cause. The global wave of student radicalism did not long outlive the 1960s. The tide was turning against all forms of heterodox economics in the early 1970s, and even the watered-down IS-LM Keynesianism of the neoclassical synthesis was coming under sustained attack from Monetarism Mark I (the product of Milton Friedman) and Mark II (the brainchild of Robert Lucas). Moreover—to change metaphors—the centre of gravitation in academic economics had long since shifted across the Atlantic to the USA. Even if the Cambridge Post Keynesians had been united, well organised, well led and consistently hospitable to outsiders and considerate towards potential allies, it seems unlikely that the second generation could ever have enjoyed the same success as the pioneers.

These pioneers must nevertheless shoulder some of the blame. Pasinetti offers a much less favourable judgement on his Cambridge Keynesians than Harcourt's appreciative account of his Cambridge Post Keynesians. They failed to take the neoclassical synthesis seriously enough, Pasinetti maintains, and therefore did not attack it with sufficient vigour (Pasinetti 2007: 29–33, 60). They did not recognise the extent of the revolution in economic theory that was required; even Joan Robinson was ambivalent on this critical issue (*ibid.*: 38, fn. 11). At the same time they were too destructive, and failed to provide enough constructive theoretical development (*ibid.*: 199). The Cambridge Keynesians were too divided among themselves to form a coherent school (*ibid.*: 62). This was reinforced by 'the Cambridge *prima donna* syndrome' (*ibid.*: 46, fn. 18), and by their exclusiveness towards outsiders, illustrated by the famous 'secret seminar', which was not in any sense a secret but was strictly 'by invitation only' (*ibid.*: 201). Such conduct had some undesirable consequences, in particular, contributing to 'the impatience of many brilliant young economists, who resented what they regarded as a sort of intellectual imposition, which they felt to be unacceptable'. This resentment was an

‘instinctive reaction to the unwise behaviour of those who should have acted as responsible masters, but didn’t’ (ibid.: 203; cf. Pasinetti 2005: 839).

I suspect that Pasinetti’s criticisms are entirely correct, but they do not go quite far enough. For one thing, he is much too kind on the question of the work rate of the Cambridge Keynesians. Kaldor and Robinson are certainly above reproach in this regard, publishing prolifically and travelling the world to attend conferences and to give guest lectures in which they propagated the new ideas. At the other extreme, Sraffa’s inability to write on anything other than classical political economy for most of the final half century of his life is all too well known. Pasinetti does pass a severely adverse judgement on Goodwin, and with good reason: the American published very little in his 28 years as a Fellow of Peterhouse, and seems to have spent most of his time painting. Pasinetti does not offer an explanation for this strange behaviour; possibly the ambience in his College, brilliantly parodied in Tom Sharpe’s comic novel *Porterhouse Blue* (Sharpe 1976), was partly to blame. Since Goodwin was one of only two of the five Cambridge Keynesians with any serious mathematical training—the other being Kahn, who had a degree in physics—the opportunity cost of his paintings was appreciable, as was demonstrated by the fine theoretical work that he did eventually produce at the University of Siena in the 1980s. By then, of course, it was much too late. Goodwin’s bizarre decision, at age 67, to apply for a chair at Siena through an open competition with candidates decades younger than he was, possibly indicates some remorse at all the wasted years, though Pasinetti does not speculate on this.

Neither does he comment on the unfulfilled promise that was Richard Kahn in the final half century of his life. In 1929–1931, Kahn had produced two outstanding pieces of economic analysis, on the short-period theory of the firm and on the multiplier. Between 1930 and 1935 he worked closely with Keynes, making a valuable contribution to the final version of *The General Theory*. But Kahn published very little of substance between 1935 and 1984, when the text of his 1978 Mattioli Lectures appeared; even his 1929 dissertation came out in English only in the year of his death (Kahn 1984, 1989). After 1945, at least, he could not claim the pressure of other duties as an excuse: he had stopped undergraduate teaching on his appointment to a personal chair in 1951, which he held until his retirement in 1972 (Pasinetti 2007: 72). What on earth did he do with himself in all those years? Pasinetti also reports Kahn’s growing unhappiness: ‘[H]e was saddened by the turn that mainstream economics had taken. In his last few years he was a very deeply disappointed man’ (ibid.: 77).

Pasinetti also understates his case when it comes to the personal relations between the Cambridge Keynesians. ‘Exclusiveness’ was only part of

the problem. Intellectual arrogance was undoubtedly another, and helps to explain why none of the Cambridge Keynesians ever took the trouble (before 1973) to write a systematic and comprehensive critique of the neoclassical synthesis, or even of its IS-LM component.

The Cambridge Keynesians' inability to collaborate with anyone must also have hindered the formation of a cohesive school. After 1945 they never published joint papers with each other, and only very rarely with third parties as co-author. One reason for this—as we have seen, and again not mentioned by Pasinetti—was that Kaldor and Robinson fell out in a very big way in the late 1950s, and their relations thereafter were characterised by deep antagonism. In the critical years (roughly, 1955–1975), they were totally incapable of forming a common front on any issue, intellectual or administrative.

Would things have turned out differently if Michał Kalecki had stayed in Cambridge? Might he have provided the unifying leadership that the Cambridge Post Keynesians so badly needed, and failed to find, after the death of Keynes? Probably not: Robinson just might have accepted Kalecki's leadership, but it is very hard to see Kaldor doing so. Jan Toporowski reminds me that Kahn remained 'irredeemably Marshallian and devoted to the genius of Keynes's theoretical breakthrough', while 'Kalecki would have fallen out with Goodwin and Sraffa over wages theory'. In any case, after 1936 the Polish theorist himself became 'more and more conscious of his own priority and wanted it acknowledged' (personal communication, 5 May 2015). None of the Cambridge Post Keynesians would have been at all happy with that.

The growing disharmony in the Post Keynesian camp cannot have helped in the recruitment of a second generation of young theorists capable of continuing the work of the pioneers. Precisely who was deterred from moving to Cambridge, when and under what circumstances, remains unclear. Pasinetti provides just two names: Harry Johnson and Robin Marris (Pasinetti 2007: 40, fn. 14). It is difficult to imagine Johnson as a loyal Cambridge Keynesian under any circumstances, but Marris might have become a valuable recruit. There is a very interesting oral history project here for someone, interviewing other economists with Keynesian sympathies who before the mid-1970s had been deterred from pursuing a career in Cambridge by the behaviour of the Cambridge Keynesians; but it had better be undertaken soon, while the individuals concerned are still with us.

Finally, there are a number of unanswered questions of a more mundane kind. Did the first generation of Cambridge Keynesians pull their weight in matters of university administration? Did they try to influence academic appointments, but fail? Was the college system a handicap, restricting the ability of the Faculty of Economics to appoint on merit rather than Porterhouse-style

sociability, or an advantage, offering a degree of decentralisation in decision-making that made it easier for an embattled minority to survive? Was there organised opposition to the Cambridge Keynesians and, if so, when and from whom? Who were the potential recruits to the Keynesian cause who applied for teaching jobs but were turned down? Did the notorious weaknesses of the Cambridge PhD system in this period identified by Kemp (poor supervision, intellectual isolation and lack of appreciation by senior staff) act as a significant deterrent to graduate students, especially from North America? Administrative histories of university departments are usually smugly self-congratulatory, or deeply tedious, or both. In the specific case of economics at Cambridge, however, where important issues of intellectual history are at stake, a systematic scholarly study of the period 1950–1975, conducted by a political scientist (or possibly by a social anthropologist), would be worthwhile.

7 Epilogue

In 2016, there remains a significant Post Keynesian presence in Cambridge, although it is an embattled minority rather than a potentially hegemonic force and has little or no remaining influence in the Faculty of Economics. In institutional terms, it is well represented in the Political Economy Society, the publisher since 1977 of the *Cambridge Journal of Economics*, an ecumenical heterodox journal in which Post Keynesian contributions are often found. The Department of Land Economy also provides a hospitable, multidisciplinary environment for prominent Post Keynesians like Philip Arestis and John McCombie (Hein and Lavoie 2015); Arestis organises the St Catharine's Political Economy Seminar that meets on most Wednesday evenings in term time. The national Post Keynesian Study Group is used as a clearing house for notifications of other meetings, publications and resources, and there seems to be no vestige of the former Cambridge exclusiveness (see <https://www.postkeynesian.net/>).

The *Cambridge Journal* publishes work by feminists, institutionalists, Marxians and radical development theorists, many of whom also have Post Keynesian sympathies. All schools of heterodox economics have come under increased pressure in recent years, in Cambridge and elsewhere, from an increasingly monolithic and intolerant mainstream, bolstered by the British government's Research Assessment Exercises that have systematically devalued the work of non-mainstream scholars (Lee 2009: Chapter 9). Thus, the future of Post Keynesian economics in Cambridge is uncertain, as are the prospects of the completion of Pasinetti's unfinished revolution.

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8

Cambridge and Economic History

Martin Daunton

1 Introduction

Sir John Clapham retired from the Professorship of Economic History at Cambridge in 1938—a highly productive scholar who did not create a ‘school’ of economic historians or establish a set of questions on which the next generation could work. Consequently, there was no obvious successor in Cambridge amongst his colleagues or students. The senior economic historian in post was Charles Fay (1884–1961), another pupil of Marshall, who taught economic history at Cambridge before moving to the Chair of Economic History in Toronto in 1921, returning to Cambridge as Reader in economic history from 1931. Fay published extensively, but his interests were too diverse, and his difficult personality, shell shock, and breakdowns counted against him (Groenewegen 2012: Chapter 10; Gault 2011).

A more credible candidate was Eileen Power, who studied history at Girton College, Cambridge, from 1907 to 1910. She was inspired by Ellen McArthur who ‘graduated’ in history in 1885 and took an active part in the suffrage movement. McArthur’s interest in economic history was encouraged by William Cunningham, with whom she collaborated on *Outlines of English Industrial History* (1895). Cunningham was deeply hostile to Marshall on the methodology of economics and on trade policy—and also on the place

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of women in the University. Cunningham was chairman of the Council of Girton College and supported the study of economic history there. By contrast, Marshall was chairman of the Extension Syndicate which organised adult education classes, and he tried to block McArthur's appointment as an Extension Lecturer in 1893 on the grounds that lecturing to male audiences would ruin a woman's character. When Cunningham stopped lecturing in economic history in 1902, McArthur provided a course of lectures for students at women's colleges in Cambridge, and some male colleges asked if their students could attend. Her main scholarly contribution was articles on wage assessments in the fifteenth and sixteenth centuries, and on women petitioners to the Long Parliament, but her lasting contribution to the subject was her generosity in leaving most of her estate to the University to fund a prize in economic history, which has also been used since the 1970s for a studentship and a lecture series (Berg 1992: 315; Erickson 2004).

McArthur's students included eminent female economic historians, including Lillian Knowles who taught economic history at the London School of Economics (LSE) from 1904 until her retirement as Professor in 1926. Power followed in her steps to postgraduate study at the LSE, returning to Girton College from 1913 to 1921 as Director of Studies in History, before moving to a Lectureship in Economic History at the LSE in 1921, where she was promoted to Professor in 1931. When Helen Cam, a historian at Girton, suggested that Power apply for the Professorship in Cambridge, she turned down the suggestion:

[I] do find the LSE a much more stimulating place to work in and London a more congenial place to live in than Cambridge ... It would be an honour to hold Clapham's chair (tho' not more of an honour than to hold Mrs Knowles's) – and I know that it would be a good thing from the point of view of the position of women for one of us to get it (Power to Cam, 6 January 1938, quoted in Berg 1992: 326; see also Berg 1996, 2004).

The successful candidate was her former student, research assistant, colleague, and husband, Michael Moisse Postan, who had a major role in transforming economic history in Cambridge.

2 Postan and Economic History

Postan was very different from Clapham. His characteristic output was articles that posed explicit questions and then used (admittedly modest) empirical work to present striking interpretation that provoked argument and created a sense of

intellectual excitement. The editors of *The Economic History Review* remarked that he was ‘direct, alert, and perceptive, with an impish humour, quick on his feet intellectually, deploying an incomparable intelligence’ so that there was ‘always a sort of magic about him’. Eric Hobsbawm remembered the impact of his lectures:

Looking somewhat like a red-haired Neanderthal survivor and speaking through a heavy Russian accent, he was nevertheless so brilliant and compelling a lecturer that he filled Mill Lane at nine o'clock in the morning ... Everyone of his lectures – intellectual-rhetorical dramas in which a historical thesis was first expounded, then utterly dismantled, and finally replaced by his own version – was a holiday from interwar Cambridge insularity.

He brought the thinking of Marx, Weber, and other Continental thinkers into the Faculty of History (Flinn and Mathias 1982: v–vi; Hobsbawm 2001: 136; Habakkuk quoted in Miller 1983: 544).

Postan was an outsider, born in Tighina in Bessarabia in 1899 to a Jewish family. He attended school locally and in Odessa and enrolled at university in St Petersburg in 1915 to study natural sciences and sociology, moving to Odessa to study law and economics. He served in the army in 1917, and on demobilisation he entered Kiev University. He was involved with radical socialism and the Jewish autonomy movement in the Ukraine, writing *Treatise on the Political Organization of Autonomous Minorities* in 1919, though his Jewish identity was not subsequently of any importance. He was out of sympathy with events in Russia and he left at the end of 1919, spending some time in the Balkans and Central Europe before arriving in London in 1920. His early years in Russia shaped some of his later career. John Habakkuk pointed out that ‘there were many perceptions about peasant society and early industrialisation which came naturally to someone educated in Russia before 1918’—and Postan was sceptical about Marxist interpretations of history and apologists for the Soviet Union (Miller 1983: 543–544; Mell 2012: 561–562; Flinn and Mathias 1982: iv).¹

In October 1921, Postan entered the LSE; after completing his master's degree in 1926, Power secured his appointment as a research assistant. They became close intellectual collaborators and married in 1937. Although Postan specialised in medieval economic history, he was encouraged by R.H. Tawney to ‘engage in some modern history as well’, and he followed this advice in his teaching and writing. He taught at UCL (1927–1931) and LSE (1931–1935), forming a circle of friends closely involved in politics, including Hugh Gaitskell and Evan Durbin (colleagues at UCL). In 1934, Postan took over the editorship of *The Economic History Review*, a position he held until 1960.

¹ Personal knowledge of Chimen Abramsky and Anna Abulafia suggests that Postan was older and that he was a Menshevik.

In 1935 he was appointed to a Lectureship at Cambridge where he made an immediate impact, bringing in the words of Habakkuk, ‘an entirely fresh vision of economic history’ (Miller 1983: 544–546).²

This sense of excitement led to his election to the Professorship in 1938—a post that Power had in mind for him as early as 1931, when she pointed out that the chairs in London and Oxford would not be available:

It is a snag that you are not a Cambridge man; but as far as I can see there aren't going to be any Cambridge men available, for Clapham has failed to train up any successor of the right calibre ... I shall never say this to anyone but you, because it would be most unsafe, but I have had it for some time in my mind. It depends entirely on how big a reputation you can amass in the next 7 years, & on how we manage Clapham.

The plan was successful, somewhat to Power's surprise, for ‘I never thought the Committee would have the sense’. Postan held the post until 1965, apart from his absence during the war as head of the Russian section of the Ministry of Economic Warfare from 1939 to 1942, and then as part of the team to write the history of the war which led to his volume on the history of war production in 1952 (Power to Postan, 29 January 1938, and Power to Camb, 6 February 1938, quoted in Berg 1996: 192, 197; Miller 1983: 548–549).

In fact, Postan's election was not entirely surprising for Power ruled herself out and he had acquired a considerable reputation through a number of important articles. At this stage, Postan's views on the medieval economy were closely aligned with those of Power, and initially focused on trade in the fifteenth century. Their work attacked the views of Werner Sombart and the German Historical School on the pre- or non-capitalist nature of the medieval economy as an ‘epoch whose economic system contrasted in every detail with the rational and acquisitive system of modern capitalism’. Postan and Power disagreed and were more persuaded by Henri Pirenne that there were different types of capitalism in different periods of the Middle Ages rather than a stable, non-acquisitive community. Pirenne was a powerful influence on Power and on Marc Bloch, and all three influenced Postan (Mell 2012: 562–563; Power and Postan 1933; Postan 1933: 212).

In contrast to Sombart's pre-capitalist view, Postan stressed ‘the organic part which credit played in medieval trade’ and ‘the precocity of financial capitalism in the Middle Ages’. He rejected the view of German Historical Economists that credit was little used in the Middle Ages—a line adopted by

²On the need to study modern history, see Postan (1971: 57) and on his friendship with Gaitskell (*ibid.*: 169–182).

Cunningham. Postan disagreed: '[C]redit commonly entered into the commercial practice of the Middle Ages', and there was 'an irreducible common factor' in medieval and modern capitalism which 'makes it possible to treat them as two species of the same genus' (Postan 1928: 234, 236, 238, 261, 1930, 1933: 212–214, 216–217, 223). He pursued this line of reasoning in his criticism of the supposed rise of a money economy. In his view, money was used from the start of documented history and could not be used to explain any later phenomenon; the crucial point was the extent to which people turned to buying and selling, which depended on varying circumstances such as Africans producing for the market because of colonial taxation, or Russian peasants because of state policies of capital accumulation. Hence 'increases in the relative volume of money transactions could reflect a whole variety of economic changes and were little more than passing, and sometimes recurrent, historical phenomena, which combined with other phenomena to create unique and unrepeatable historical phenomenon'. There was 'not one rise of money economy but several rises of several money economies', with retreats in some times and places. The rise of money was not itself an emancipating force: without large reserves of free landless labour and the legal safeguards of a liberal state, the expansion of markets was likely to lead to an increase in labour services to meet demand for agricultural produce—as in East Germany with the growth of grain export markets, and in England with the development of agricultural production for the market in the thirteenth century (Postan 1944: 130, 134; see also Postan 1937).

Postan started to turn away from a concern with medieval credit, money, and trade to understand changes in manorial structures and the commutation of labour services in the context of long-run economic trends. He adopted a Malthusian approach of demographically driven expansion and contraction (Brenner 2008: 540). His essay of 1937 on labour services was his first major contribution on the agrarian history of England, explaining variations in labour services by the needs of the demesne—land directly farmed by lords. Partial or complete commutation of labour services in the twelfth century was reversed in the thirteenth century when rising population and land hunger led to high prices of food, a shift in the balance of power towards manorial lords, a growth in demesnes to supply markets, and the reimposition of labour dues. In the fourteenth and fifteenth centuries, population and prices fell, less land was cultivated, holdings were vacant, and trade declined. The fifteenth century was a period of agricultural recession with a decline in markets for produce and higher costs as a result of labour shortages so that demesnes were no longer profitable. Peasants and landless labourers benefited, for they had a stronger bargaining position to force landlords to commute labour services

for cash, and gained from lower land prices and higher wages (Postan 1937, 1939a). Such an approach was central to Postan's later work: a stress on cycles between resources and population, and on the way that trends could operate in seemingly contradictory ways depending on circumstances.

Postan continued to develop his thinking on demographic and agrarian structures, arguing against the common assumption that the Black Death was an interlude in rising population. In his view, the population of England started to fall from the opening of the fourteenth century, about 30 years before the arrival of plague as a result of 'the inherent tendencies of population on the Malthusian level of existence'. The increase in population in the thirteenth century led to land reclamation, a movement onto marginal soils, a shift from pasture with a decline in livestock, and consequently a drop in manure needed to maintain the fertility of soil. Agrarian society was 'balanced on the margin of subsistence' with 'an extreme degree of rural overpopulation' and small holdings, which meant that it was susceptible to shocks of poor harvests caused by the vagaries of weather and disease. The result was 'long periods of reckoning when the marginal lands, no longer new, punished the men who tilled them with recurrent inundations, desiccations and dust storms'. Postan thought that previous levels of population might not have been regained until the Industrial Revolution. These trends had a varying impact on different levels of rural society. During the period of over-population, village smallholders and labourers formed a rising proportion of the population and were more sensitive to harvests. However, with the fall in land values and rents, the losers were large landowners, and the beneficiaries were the smaller peasants who could buy land and become kulaks, with an opportunity for landless families to acquire property (Postan 1950: 146, 1962: 248; Postan and Titow 1959: 402, 409–410; Miller 1983: 553–554).³

Postan's approach was unlike Clapham's cautious unwillingness to adopt a position; as Edward Miller remarked, Postan showed 'a taste for speculative thought not altogether common among historians, even economic historians'. He was influenced by both the transformations in Russia and the Soviet Union—the emancipation of the serfs in Tsarist Russia and the destruction of the kulaks by Stalin formed an undercurrent in his thinking—and by the economic crises of the interwar period, which made him reject the assumptions of the Historical School and liberalism of 'continuous ascent' from barbaric primitivism to the modern world (Miller 1983: 546; Postan 1939a: 160–161). Neither was he sympathetic to the Marxist interpretation of a transition from feudalism to capitalism that was propounded by Maurice Dobb in the Faculty of Economics at Cambridge, a man who inspired a post-war generation of Marxist economic history.

³ The summation of Postan's analysis is in Postan (1966).

This debate over the transition from feudalism to capitalism started in 1940 with a disagreement among Communist historians over the English Civil War: Did it mark the start of the capitalist mode of production, or had capitalism emerged earlier with the war provoked by a feudal counter-revolution? Dobb took the former line which was the position of the Communist Party. In 1946, the Communist Party Historians Group was established, with Dobb as the senior member and younger members including Christopher Hill, Hobsbawm, E.P. Thompson, Rodney Hilton, and John Saville who went on to play a leading role in British economic and social history. Dobb's *Studies in the Development of Capitalism* appeared the same year, providing a Marxist interpretation of English capitalism since the fifteenth century. History was a succession of class systems, each with its own mode of extracting income for its ruling class. By the seventeenth century, some craftsmen had enough capital to become merchant capitalists, and subordinated production to capital; the emergence of industrial capitalism required the destruction of restraints on trade which had been designed to benefit merchant capitalists, who turned to the monarchy for support. The outcome was the Civil War. The book was greeted with excitement by Marxist historians, with Hilton commenting that Dobb 'demonstrated in a most striking way the superiority of the Marxist approach to historical problems over the bourgeois eclecticism which nowadays passes as a substitute for proper analysis' (Hilton quoted in Shenk 2013: 112). Postan was not sympathetic, and he declined to have a review of *Studies* in *The Economic History Review* (Hobsbawm 2004; Shenk 2013: 71, 99–115). Dobb's class interpretation differed radically from Postan's Malthusian model in which different levels of agrarian society were affected by demographic variables.

A major concern of Marxist historians was the 'primitive accumulation' of capital for industrialisation. In 1935, Postan published an article on recent trends in capital accumulation which had a different approach. He suggested that there had never been an absolute shortage of capital in the past but that savings were hoarded. In the late eighteenth and early nineteenth centuries, industrialists had to rely on their own savings, reinvested profits, and their friends and relatives so that '[c]apital...was intimately bound up with human personality' (Postan 1935: 5). The situation changed in the nineteenth century with the emergence of joint-stock banks and shares, and a single capital market dominated by the rate of interest. Postan saw a decline from this peak by the 1930s, so that capital was no longer so mobile or divisible, or so responsive to the rate of interest: international capital flows were controlled by governments, and domestic investment was forced into directions it would not otherwise have gone. Further, there was a decline in 'pure investors', people who put their money to profitable use by the sole criterion of interest. New

sources of capital came from compulsory saving through the state by means of taxation and social insurance; from the reserves of joint-stock companies; and 'provident' savers in savings banks, friendly societies, and insurance companies. Postan worried that this capital was not responsive to interest rates, and could be misallocated. As he said, 'there is no failure like success': prosperity created provident savings and self-financing corporations, so that capital was not used to its best effect (ibid.: 12). This short article pointed the way to many subsequent works by economic historians on patterns of investment since the Industrial Revolution. Postan's approach to capital did not rest on a Marxist account of exploitation of labour by capital or a conflict of classes, but on changing institutional patterns with a concern about the stultifying effect of the demise of personal ownership of capital.

Postan developed his views on economic history and economics in a lecture in 1935 and his Inaugural Lecture at Cambridge in 1938. He felt that historians had become concerned with facts for their own sake, leading to the danger of antiquarianism. Postan argued that 'If history is to regain its place in the general intellectual movement of our time, it must restore to the full its erstwhile connection with social generalizations', without falling victim to a second danger of grand theories of social development. History was distinct from both antiquarianism and 'general sociology', considering the past in terms of general problems without abstracting from real situations to develop propositions independent of particular circumstances.

Postan was highly critical of theoretical economists who 'tried to solve the largest possible problems from the least possible knowledge'. He complained that

The price of deduction is abstraction: the logical rigour and consistency of economic propositions is a direct consequence of the fact that the fundamental concepts, the original assumptions and the successive stages of economic argument are all treated in isolation from the rest of [the] social environment (Postan 1939b: 18).

A case in point was Keynes's *General Theory* which explained employment and interest by the propensity to consume, and the preference for more or less liquid savings:

But how much do the economists know about them? Do they know or have they explained the complex social process which throughout history has determined the employment of income and its allocation to consumption or rather to consuming classes, or have they tried to discover what social forces lurk behind liquidity preferences? (ibid.: 23).

Postan wanted an approach on the lines of his analysis of capital, not as an abstract, uniform concept but through understanding its nature in precise social and institutional settings. What was needed was a concern for the 'whole combination of social forces' that was irreducible to mathematical formulae, 'an interrelation so multiple as to make the work of abstraction impossible and undesirable':

What gives facts of history, or all social facts, their worth as evidence, and their value for causal analysis, is their existence, their tangible and verifiable reality. Only tangible and concrete phenomena can be fitted into a social setting and demonstrated as a link in a chain of causation ... Where the historian shows his scientific preoccupations, and qualifies for membership of the social sciences, is in concentrating the study of his individual subject on its relevance to general and theoretical problems. He studies rural society in the Middle Ages, which is a unique and unrepeatable phenomenon, because the study is relevant to such sociological problems as the correlation of population, social structure, social class and tenure, economic technique and legal concepts. But unlike a sociologist he refuses to ask universal questions or try to formulate general laws (*ibid.*: 30–32).

The economic historian should make 'microscopic problems of historical research' into the 'microcosmic—capable of reflecting worlds larger than themselves. It is in this reflected flicker of truth, the revelations of the general in the particular, that the contribution of the historical method to social sciences will be found' (*ibid.*: 32, 34; see also Postan 1936: 20–21). He held the same view in 1968, when he said that universal generalisations were not possible, for history was too complex:

Their preference must be for generalizations which are not formulated but merely implied. They would imply the existence of underlying social laws by showing how a seemingly concrete phenomenon is shaped by the actions of general forces; they could make a unique phenomenon reflect the laws of society by presenting it as a microcosm – a particle of a universe and of a flow of events much wider than itself (Postan 1968a: 63).

Postan's credo is still one that most Cambridge economic historians could accept.

He was highly critical of the harm caused by abstract economics. In 1968, he lay some of the blame for Britain's economic problems on a 'plague of economists' whose sophisticated theories were out of touch with real economic situations. The macroeconomic Keynesian approach to demand, savings, investment, labour, and prices was detached from local and specific circumstances, lacking a microeconomic concern with individual firms and people.

The result was policy driven by aggregate concerns, whereas the real problems were ‘not general, not organic, but local, and not always purely economic’ (Postan 1968b: 83). He looked to specific failings in individual sectors, in management, design, or sales, which were not helped by general solutions: his model was the French planning system. In particular, he criticised the misleading approach of dividing the economy into primary, secondary, and tertiary sectors, with the assumption that growth came with the transfer of labour from the primary to secondary sector, and that the growth of services was harmful. In his view, the argument rested on a poor understanding of the Industrial Revolution which led to the ‘Economist-Communist’ route (ibid.: 88) of industrialisation in the Soviet Union and India, with a waste of capital on over-ambitious, capital-intensive projects that did nothing to relieve poverty. He was scathing about the ideas of Nicholas Kaldor that led to the Selective Employment Tax designed to move labour from services into manufacturing. Postan pointed out that grouping all services together was ‘to be guilty of the worse crimes of irrelevant classification’ (ibid.: 89). Some services had low productivity; others made a greater contribution to trade balances and productivity than manufacturing. What was needed from economists was ‘a larger dose of micro-economic understanding, i.e. with ideas about the influences affecting the individual factors of production, labour capital and management, or the factors shaping the structure and behaviour of individual industries and firms’ (Postan 1968c: 93).

Amongst the first pupils to fall under the spell of Postan was Hrothgar John Habakkuk who read history at Cambridge between 1933 and 1936. Clapham suggested that he should carry out research on the industrial history of his native South Wales, but Habakkuk was more enthusiastic about Postan’s suggestion that he turn to estate records in order to understand the gentry in the eighteenth century—a chronological continuation of the study of agrarian society that Postan was carrying out for the Middle Ages. He started from the question of why England developed a distinctive tenurial system of aristocratic landowners, tenant farmers, and landless labourers at the expense of the gentry and freehold peasants. Like Postan, he was concerned with the impact of economic change on different levels of agrarian society. He found the answer in the legal system of marriage settlements which allowed aristocratic families in the eighteenth century to concentrate wealth in favour of the male heir at the expense of the younger sons and daughters. Marriages were dynastic and patriarchal rather than affectionate, and were the most important factor in the rise of great estates (Habakkuk 1940, 1950). Habakkuk’s own views changed and many subsequent works modified this picture.

In 1938, Habakkuk was elected to a Fellowship at Pembroke College. He left Cambridge for war service at Bletchley Park (1940–1942) and then to assist in drafting papers on international financial negotiations leading to Bretton Woods. He returned to Pembroke in 1946, and ran a course with Postan on the development of the British economy between 1886 and 1938. In 1950, he moved to the Professorship of Economic History at Oxford and soon thought that the subject had moved in directions he could not understand as a result of the rise of ‘cliometrics’ (Thompson 2006). The application of econometric analysis to the past was a challenge to economic historians in Britain. In many universities, separate departments were closed and posts lost. Cambridge was different, for economic history was taught within both the Faculty of History and the Faculty of Economics, and it continued to flourish in part through the continuation of Postan’s focus on the relationship between population and resources rather than the use of econometrics.

Here was the central concern of E.A. Wrigley who was, like Postan, a Fellow of Peterhouse, and like him a proponent of a Malthusian approach to economic history. Wrigley brought another intellectual strand into economic history, for he studied both history and geography as an undergraduate at Cambridge, and then wrote a doctoral thesis on the coal belt of north-west Europe during the Industrial Revolution. Wrigley was a Lecturer in geography at Cambridge between 1958 and 1974, and that department had a considerable role in teaching economic history. A major figure was Henry Clifford Darby, whose books on the medieval fens and on the drainage of the fens showed the links between geography and history. Clapham was a strong influence on Darby, encouraging him to produce *An Historical Geography of England before AD 1800* (1936). After he moved to Liverpool in 1945, historical geography continued to flourish at Cambridge with scholars such as Alan Baker. Graduate students in the department made a major contribution to economic history—such as the cohort in the early 1970s of Bruce Campbell (who argued that the pressures on rural society prior to the Black Death arose from the generosity of landlords allowing tenants to have access to land through subdivision), Richard Smith (who analysed medieval English peasant inheritance practices and their impact on social and kinship networks), and Mark Overton on the agricultural revolution (Williams 2004). The approach was quantitative but not econometric, based on a firm grasp of regional characteristics of landscape, farming systems, and demographic regimes, and the connection between people and resources.

The debate over the growth of population had long been a subject of contention. Population collapsed in the fourteenth century. At what point did it regain and surpass earlier levels? Also, why did population recover? Conventional wisdom pointed to a drop in the death rate as a result of better nutrition and living conditions, or a decline in diseases such as smallpox. Here was the central concern of Wrigley. Careful analysis of demographic systems in relation to changing social structures was one of the highlights of Cambridge economic history, following in the steps of Postan's analysis of rural demographic structures, tenurial systems, and use of resources. Of course, an estimate of the total population of the country was essential for a calculation of long-run trends in British economic growth—a task that started with the compilation of national income statistics in the Department of Applied Economics after the Second World War (as explained in other chapters). The data produced by Charles Feinstein for the period after 1855 became a benchmark for modern economic historians (Feinstein 1972), but there were greater difficulties for the earlier period. In 1962, Phyllis Deane and Max Cole produced the first estimates of British economic growth going back to 1688 (Deane and Cole 1962), a pioneering enterprise that was obviously tentative, not least because of uncertainties about the level of population before the first census of 1801. Here was the task for Wrigley and his colleagues.

3 The Cambridge Group for the History of Population and Social Structure and E.A. Wrigley

The Cambridge Group for the History of Population and Social Structure (CAMPOP) was established in 1964 by E.A. Wrigley and Peter Laslett. Laslett was a history undergraduate at St John's from 1935 to 1938; after the war, he returned as a Research Fellow, working on the political thought of Robert Filmer. In 1953, Laslett was appointed a University Lecturer in history and specialised in political thought which led to an interest in family and social structures. Filmer's writings were based on a normative belief in patriarchal households containing a large number of kin. Laslett's discovery of population listings for two parishes provided a different picture: the average household contained just over four members, and was usually a nuclear family of a couple and their children. Marriage was usually after the age of 20, with much movement between parishes, and a large number of unmarried servants. *The World We Have Lost: English Society before the Coming of*

Industry (1965) developed an approach to social history based on the relationship between the household and the wider social structure (Smith 2009).

Analysing social structure and demographic trends required a team effort, and in 1964, Laslett joined Wrigley in establishing CAMPOP which became a unit of the SSRC/ESRC between 1974 and 2000 under the Directorship of Laslett, Wrigley, and Roger Schofield. Although Wrigley moved to the LSE in 1979, and later to Oxford, he remained Co-Director and returned to the Professorship of Economic History from 1994 to 1997. The Group moved to the Department of Geography, along with its then Director, Richard Smith—a Lecturer in medieval economic history—until its return to History in 2013. The major output was Wrigley and Schofield's path-breaking *The Population History of England, 1541–1871: A Reconstruction* (1981). *The Population History* rested on data from 404 parish registers out of a total of about 10,000 in England to give a total number of christenings, burials, and marriages. In order to convert these data into rates of births, deaths, and marriages, it was necessary to estimate the total population which was done by means of 'back projection' from the national census of 1871. It transpired that the population grew rapidly in the later sixteenth and first half of the seventeenth centuries, followed by a period of decline or stagnation in the second half of the seventeenth century, modest growth in the first half of the eighteenth century, and much higher growth from the middle of the century and especially after 1786. Their data strongly suggested that the pattern of English population growth was determined by the 'preventive check' of 'wide, quiet fluctuations in fertility' rather than the 'positive check' of mortality (ibid.: 450–453, 457–480).

The English demographic regime was 'a fertility dominated low-pressure system' (ibid.: 451) in which population was relatively low in comparison with resources, and both mortality and fertility were relatively modest, in contrast to a high-pressure system in which population pressed against the limits of available food and resources, and both fertility and mortality were high. They used 'family reconstitution' to recreate the history of individual families by linking the information on christenings, marriages, and burials in 12 parish registers, to show that fertility was initially controlled by the proportion of the population that married and subsequently a drop in the age of marriage of women. England had a 'natural' fertility regime, with women bearing children from the age of marriage to menopause; a drop in the age of marriage therefore added an additional period of childbearing that could explain much of the increase in births. This work indicated connections between demographic change and economic conditions, for the opportunity to marry depended on

economic circumstances—above all a lagged response to real wages (Wrigley and Schofield (1981); see also Wrigley et al. (1997) which used family reconstructions from 26 parishes to confirm the main findings).

These findings on population trends were linked to three other features of English society to create a mutually reinforcing structural system: household formation; the Poor Law; and energy flows. The age and rate of marriage were linked to the ways in which families adjusted their demographic behaviour and household formation to the resources of the parish, joining the specifics of localities to a grasp of the aggregate outcomes at the national level. The early release of labour from agriculture and the ability to support more people off the land were defining features of economic development in England. Wrigley pointed out that agrarian families in England shed members when marginal productivity fell to the conventionally accepted minimum standard of living, in comparison with Continental Europe where holdings were used to support the maximum number of family members as a form of welfare system, and labour was retained until the average productivity reached the conventional minimum standard of living accepted in the community (Wrigley 1988: 44–45). Here was one explanation for the ability to support more people off the land—a pattern that CAMPOP and above all Smith argue was linked with England's tax-funded Poor Law which offered security to agrarian families to release labour to the towns.

CAMPOP argues that the old Poor Law was closely connected with the nuclear household structure. Late marriage meant that families experienced their greatest hardship in caring for dependent children precisely at the point when parents were entering old age—as a result, married couples supported their children and passed care of the elderly to the parish and relief was relatively generous in the knowledge that current contributors would themselves become beneficiaries. In this account, the old Poor Law was an inclusive institution that balanced resources between generations to overcome 'nuclear hardship', and to allow the release of labour to the towns in the knowledge that relief was available. On this account, the old Poor Law was an integral feature of the interlocking institutional pattern that led to economic growth (Smith 1984, 1986, 1996; Laslett 1988).

At the heart of Wrigley's interpretation was a third factor: a shift from an advanced organic economy dependent on flows of energy from wood, wind, water, and animal and human muscles to stocks of energy from coal. An advanced organic country could have a sophisticated commercial culture and division of labour, as did Holland—but there were negative feedbacks with growth leading to declining marginal returns that made future growth more difficult: 'A technically perfect capitalism appeared to be as easily consonant

with the stationary state as with continuous, rapid growth'. England escaped the fate of Holland, for coal from the north-east of England was easily shipped to London, so allowing it to emerge as the largest city in Europe by 1800. The growth of London forced specialisation in agriculture which raised productivity, and coal allowed land to be released from growing timber for fuel, so that there was more pasture and livestock than other regions, providing manure for the soil and more animal power per worker. England escaped from the cycle of readjustments between land and population, and the onset of declining marginal returns, described by Postan. The balance remained precarious until the second half of the nineteenth century, when the shift from lower productivity artisan trades into steam-powered factories finally allowed a breakthrough in standards of living (Wrigley 1987, 1988: 28, 73, 95, 104, 2004, 2010).

Members and associates of CAMPOP continue to work on demography. Simon Szreter has been more concerned with the fall in fertility in the later nineteenth century, initially by continuing a regime of natural fertility and spacing births more generously between marriage and menopause through self-imposed male sexual abstinence, before the onset of a new regime concentrating births in the earlier years of marriage. He has also stressed the role of investment in public health, with a serious fall in the second quarter of the nineteenth century leading to high mortality in industrial towns, and then renewed investment in the later nineteenth century leading to a marked drop in mortality (Garrett et al. 2001; Szreter and Fisher 2010; Szreter and Mooney 1998; Szreter 1997). But the major project since 2003, under the direction of Wrigley and Leigh Shaw-Taylor, is the occupational structure of Britain between 1379 and 1911. Wrigley's work stressed early occupational change, and the main conclusion of this project is that the growth in employment in the secondary sector (manufacturing and construction) took place earlier than previously assumed, with about 40% of the male population in this sector by the 1710s. Structural change predated technological transformation of major industries such as cotton and higher rates of economic growth in the nineteenth century. The findings are changing the chronology of the Industrial Revolution whose origins are now placed further back in time. The occupational project continues the reliance of CAMPOP on the collection of large data sets and nominal record linkage, funded by large external grants, which are then analysed using relatively sophisticated statistical and geographic information system (GIS) techniques to map change.⁴

⁴ For working papers, publications and main findings, see <http://www.campop.geog.cam.ac.uk/research/occupations/>.

4 History and Economic History

The work of CAMPOP has been of great importance in the interpretation of English economic history, but is not without its critics, even in Cambridge. John Hatcher, a medieval economic historian, criticises CAMPOP's approach as 'robotic', operating in a manner that is 'simple, predictable and linear, rather than complex, volatile and non-linear'. In his view, the Group pared down the 'elemental simplicity' of Malthus's 'sparse propositions'. Hatcher proposed a more complicated picture in which forces were 'manifold, complex and variable rather than singular, simple and mechanistic'. He suggests that the low-pressure regime did not apply in the mid- and late sixteenth century, and that mortality was dominant between 1691 and 1751. He criticised CAMPOP's model as closed: the rate of population growth influenced real wages, and real wages then determined nuptiality, fertility, and the rate of population growth. He also pointed out that their analysis covered less than two cycles of population growth, so the extent to which there was a low-pressure regime over a longer period is not clear (Hatcher 2003: 85, 87–89). Similar complaints have been made about CAMPOP's view of the old Poor Law as an integral element of the nuclear household system that facilitated economic growth. Was it really so inclusive, with a general acceptance that supporting the elderly of other families was a sensible strategy to ensure one's own care in old age?

Many economic historians in Cambridge accept the limitations of the Group's approach but build on it to produce a more nuanced picture. One shortcoming was the lack of sufficient attention to the internal dynamics of households and how decisions were reached. Wrigley and Schofield assumed that the interests of men, women, and children were symmetrical and the outcomes were in the best interests of the household as a whole. Economic historians in the Faculty of Economics have been more sensitive to these issues, above all Sara Horrell and Jane Humphries (a member of the Faculty prior to her move to Oxford). They point to the gendered changes in households as families ceased to be the most significant unit of production and became more dependent on male breadwinners. The number of dependants changed, as did non-wage inputs such as common rights exploited by women. There were major shifts in the ability of women and children to earn. For example, in factory districts, more children went to work at a younger age, their relative wages rose, and they could leave home, so that younger children made a more modest contribution to the household. Further, the allocation of resources within households dependent on a male breadwinner varied according to the availability of female waged labour (Horrell and Oxley 1999, 2012; Humphries

1990; Horrell and Humphries 1995a, b). This interest in the employment of women and children has also been pursued by Craig Muldrew for an earlier period. He argues that employment of women and children increased from the late seventeenth century, providing sufficient income for a woman to support herself and not marry—a possible explanation for the low rate of nuptiality. The changing level of female employment was possibly more important than changes in male earnings, and a bottleneck in labour supply could then lead to mechanisation and hardship (Muldrew 2011a).

Other work refers back to the concerns of Postan on credit. Chris Briggs suggests that debt in fourteenth-century villages was not, as in Postan's account, a sign of a Malthusian crisis of over-population and harvest failure leading to borrowing to buy food and pay rent. Rather, it was a sign of commercialisation, with 'horizontal' loans between middling and upper peasants (Briggs 2009: 8, 10, 146, 216–218). Another project at CAMPOP, led by Smith, considered the role of private law in medieval village society, arguing that even unfree serfs could use the civil courts to defend private property rights, predating the emergence of modern democracies. Such a view has important implications for the claim of new institutional economists that economic growth was the result of secure property rights after 1688 (CAMPOP undated).

A concern for credit was extended to the early modern period, above all in the work of Keith Wrightson who initially wrote on household formation. Subsequently, he and his students stressed the emergence of a new form of market society that changed the nature of human relationships. Economic practices were embedded in social relationships and cultural meanings. Initially, households were connected with the local community through neighbourliness and personal, mutual obligation. This system was tested in the sixteenth century by rising prices and population, and in the seventeenth by falling prices and population. Landed society became more differentiated; the development of rural industry and urban trade created a larger 'middling sort' and more people became landless labourers. Traditional social morality was reworked, with the state encouraging a commercial economy and 'improvement' while also preventing the pursuit of self-interest at the expense of the public good by providing poor relief and maintaining employment (Wrightson 2000).

Wrightson's approach has been pursued by Muldrew's analysis of how expanding market relations affected households in early modern England. As chains of credit became longer, personal reputation for honesty and reliability became less secure, and there were greater risks of default with a surge in litigation to secure payment. This shift from personal reputation was central to

contemporary writings on social relations, and one reaction was their definition in terms of contractual equality, as by John Locke. The market was not just an abstraction, a means of exchanging goods based on individual desire or self-interest: it was about the 'complex motivations and practices of agents acting out relationships of economic exchange—together with an understanding of how they themselves interpreted such actions'. Such an approach 'can produce a much more subtle and complex economic history' (Muldrew 1998: 8, 10). The rapid decline in litigation in England from the early eighteenth century rested on a shift from personal trust to a more impersonal trust in systems—in monetary systems, banks, government, or in accounting standards. Muldrew's ambition was to link the approach of CAMPOP, with its concern for households, with the Cambridge school of contextual intellectual history, which considered one text in the light of its interaction with other texts in the past or at the time. Muldrew went a stage further by considering the context of the market and social relations with which writers were interacting (Muldrew 1993, 1998).

Muldrew has also extended Wrigley's interest in energy inputs into the economy by considering the diet of agricultural labourers during the organic economy from the middle of the sixteenth century to 1780. He argues that English agricultural labourers could work harder because they were better fed than previously assumed, with a higher consumption of bread, beer and meat than in the rest of Europe, in particular after 1650. Further, these were years of low population growth which meant that it was possible to use income to build up capital rather than spending it on food. Once again, Muldrew shows how these concerns informed contemporary debates and provided a central theme of Adam Smith. Traditionally, low wages were assumed to be needed to drive idle labourers to work longer hours. Smith disagreed, arguing that prosperity arose from workers' productivity and consumption. Smith believed that labourers were active savers of capital and consumers of the products they produced, so that a virtuous circle of rising production and 'public opulence' was created by the labouring poor rather than colonies. In Smith's analysis, free markets were designed to benefit small producers and labourers who were disadvantaged by regulations. Muldrew argues that Smith's analysis reflected economic reality, for labourers accumulated capital for investment and savings for consumption during the period of stable population from 1650 to 1760. Although rising population created problems later in the eighteenth century, sufficient capital had been accumulated and financial institutions developed in order to maintain investment, consumption and growth (Muldrew 2011b, c, 2013). Muldrew made a compelling move, bringing together two highly fertile and distinctive areas of work in the Faculty of History—the structural analysis of CAMPOP and the history of thought—in order to create a distinctive form of economic history.

The intellectual and ideological context of economic relations was a major theme of the Centre for History and Economics set up in 1991 and now working jointly with Harvard University. The Centre has been concerned with the large-scale political, economic, and cultural systems of the Atlantic, south-east Asia, and Indian Ocean, and with the history of political and economic thought in the context of economic, religious, and legal history. Such an approach brought together historians of thought such as Gareth Stedman Jones and Emma Rothschild with historians of empire such as Chris Bayly.⁵ One outstanding example of the approach is Rothschild's book on Smith, Condorcet and the Enlightenment in which she showed how 'Economic thought was intertwined with political, philosophical, and religious reflection. The life of cold and rational calculation was intertwined with the life of sentiment and imagination' (Rothschild 2001: 1). The Centre also continues Wrigley's interest in energy. The project on ecology, economy, and society between 1500 and 2000 considers flows in natural and processed resources, and their place in the economic and political thought of Europe. Much of this initiative is based at Harvard, with Paul Warde taking a lead in Cambridge with his interest in long-term trends in energy consumption in England and Europe. His initial work considered the growth of government in early modern Germany through the uses of wood, a fundamental material resource in pre-industrial society. A dilemma emerged in the responses to environmental and ecological challenges: institutions designed to prevent local conflict over scarce resources were not easily adapted to optimise the management of those resources (Warde 2007, 2010; Kander et al. 2015).⁶

This concern for the nature of institutions and how they handled exploitation of resources or the risks of trade has become a major theme in both economics and economic history, above all as a result of the work of new institutional economists. Many economic historians see in this work the dangers of generalisation that Postan warned against and prefer to consider the concrete without losing sight of general processes. One of the leading critics of the new institutionalists is Shelagh Ogilvie, an economic historian in the Faculty of Economics at Cambridge whose approach rests on close archival work on particular episodes allied to a deep appreciation of wider processes. She has analysed the way in which guilds shaped economic development. Some scholars see them as beneficial social capital: by generating sociability and trust, they mitigated the risk of trade by establishing norms of behaviour.

⁵ See <http://www.histecon.magd.cam.ac.uk/research/hex/objective.htm>.

⁶ See also <http://www.histecon.magd.cam.ac.uk/eec/index.html> and <http://www.fas.harvard.edu/~histecon/energyhistory/index.html>.

They are also seen as sources of efficiency that overcame various economic problems of state extortion, commercial insecurity, contract enforcement, principal–agent relations, imperfect information, and economic volatility, so fostering the medieval commercial revolution. On this view, ‘whatever is, is right’ (Ogilvie 2007). Ogilvie took a more sceptical view: social capital could exclude outsiders, and institutions are about distribution as well as efficiency.

Guilds might in some places and times assist economic growth, but most secured rents for their members, part of which they transferred to political elites in return for the legal privileges that enabled guilds to extract rents. Hence, guilds did not arise to deal with market failures but to ‘serve the distributional interests of powerful groups’ (Ogilvie 2014: 187). They survived because the considerable costs of lower growth were spread over large numbers of potential entrants, consumers, and workers; the benefits were minimal, but concentrated in a small group of members and political elites. In Ogilvie’s view, ‘the Commercial Revolutions of medieval and early modern Europe took place despite the social capital generated by merchant guilds’ (Ogilvie 2011: 17). Growth needed generalised institutions that facilitated transactions and secured property rights impartially between all members of society. Ogilvie points out that there were always some areas, such as the Champagne fairs, with generalised rather than particularised institutions. However, the process could go into reverse, for the generalised public institutions of Champagne shifted to a more particularised system that constrained particular classes of merchants, so leading trade to shift to other centres with generalised institutional services. The shift in some countries to a more generalised pattern arose from representative institutions constraining how rulers granted privileges. Nevertheless, strong parliaments did not necessarily lead to growth for the whole economy, for large landowners and merchants might impose policies for their own interests. More significant was the growth of a more diversified urban system in which competing towns limited the ability to secure privileges, and the way in which people came to be wealth holders and hence the policies they wished to adopt (Edwards and Ogilvie 2012; Ogilvie and Carus 2014). Ogilvie is pursuing Postan’s approach of microcosmic history—a concern for the concrete and real without losing sight of the general.

CAMPOP’s approach to the development of the English economy was largely internal, with little concern for trade, imperialism, and international migration, all of which were significant in reducing pressure on available resources. Migration to the colonies held down population growth at home; and the import of colonial commodities such as cotton, sugar, tea, and coffee from North America and Asia added more ‘shadow’ acres. Of course, many of these crops entailed the use of slavery. Economic interests in these commodities

were also connected with the nature of economic policy on the lines discussed by Ogilvie: Were the chartered companies such as the East India Company rent-seeking, particularised institutions, and, if so, how did generalised institutions emerge? These questions have been considered by imperial historians who are interested in the interconnection between British traders and settlers and indigenous economies, and in the ways in which domestic economic interests shape policy.

Imperial historians in Cambridge developed two concepts that have been central to debates in economic history. One was the notion of informal or free trade imperialism developed by John Gallagher and Ronald Robinson which argued that the expansion of formal empire from the 1880s rested on the mid-Victorian informal empire based on the principles of free trade and capital exports (Gallagher and Robinson 1953). The second was the notion of 'gentlemanly capitalism' proposed by A.G. Hopkins, an economic historian of Africa who was appointed to the Smuts Professorship of Commonwealth History at Cambridge, in conjunction with Peter Cain. In Hopkins' view, landed aristocrats and financiers dominated economic policy at the expense of industrial capital—a view that is not universally accepted (Cain and Hopkins 1986, 1987; for a critique, see Daunton 1989). Nevertheless, he did point to a major issue: How did economic interests at home form alliances to shape policy and restructure institutions? These connections between imperial and economic history, between domestic and external economics and politics, are of growing interest.

The interest in free trade imperialism and gentlemanly capitalism has links with another approach which is again not well represented in CAMPOP's concern with the response of households to economic signals. How do businesses function and make decisions? A number of economic historians in Cambridge have written business history which shows how entrepreneurs respond to market opportunities, how they manage their firms and respond to or shape economic policy. The pioneer was Charles Wilson whose initial graduate work was on Anglo-Dutch finance and commerce in the eighteenth century. This research led in two directions. One was to a major reinterpretation of mercantilism, not as some abstract entity with a fetish for bullion, but as a response to actual problems facing politicians (Wilson 1949, 1957a). Wilson understood how merchants and politicians thought—but he was less interested in economic and social structures, and he was hostile to 'sociologised history... which is less concerned with individuals and more with men as members of social groups' (Wilson 1959: 100). In Wilson's view, personal ambitions and character were more important than economics: '[A]s so often in history what looks like economic interests powerful enough to swing politics in their wake

turn out in reality to be a puny thing, politically vulnerable to the slings and arrows of outrageous politics' (Wilson 1970: 17). Not surprisingly, Wilson was appointed to the Professorship of Modern History at Cambridge in 1964 rather than waiting for the Chair in Economic History on Postan's retirement (Mathias 2000: 561–565, 567–569). His attitude was apparent in the second major strand of work. His expertise in Anglo-Dutch history led to an invitation to write the history of Unilever, the first professional economic history of a major multinational firm, and one that had extensive imperial connections such as palm oil plantations in West Africa. The book had an underlying philosophy with political resonance: the need to celebrate 'great *entrepreneurs* whose genius helped to provide...abundance'. Growth was not a natural process but rested on 'human intelligence, human character, ingenuity and enterprise' (Wilson 1954, volume 1: ix–x; italics in original). Wilson went on to write other business histories that celebrated businessmen but paid little attention to productivity growth, profitability, investment and return on capital, management, and organisation. His main interest was the importance of 'a sense of market opportunity combined with the capacity needed to exploit it' (Wilson 1957b: 103)—a theme that he brought out in his study of the so-called great depression of the later nineteenth century where he saw opportunities for businessmen to exploit the market for consumer goods for an expanding domestic market (Wilson 1954, 1957b, 1965, 1968; see also Mathias 2000: 556–560).

This theme was pursued by Neil McKendrick in his study of the marketing strategies of Josiah Wedgwood (McKendrick 1960). Clive Trebilcock considered Vickers Brothers who were major producers of steel for the merchant marine as well as armaments that provided the basis for British naval and military supremacy; he went on to write a history of an insurance company, an important way of mitigating risk through impersonal trust, and an example of the type of investment that worried Postan (Trebilcock 1977, 1985, 1999). The Professorship of Economic History went to people who worked more in this vein—to David Joslin (1966–1970) whose major book was a history of the Bank of London and South America, to Donald Coleman (1971–1981) who wrote a business history of Courtaulds and Barry Supple (1981–1994) who wrote about business history but was deeply interested in the relationship between economic thinking and events, both in relation to trade in the seventeenth century, the insurance industry and the regulation of thrift in the nineteenth century, and the coal industry in the twentieth century (Mathias 2004; Coleman 1969; Supple 1959, 1970, 1987; Furner and Supple 1990).⁷ None of these holders were close to CAMPOP or to the approach of Postan—a pattern broken by the appointment of Wrigley.

⁷ For Barry Supple, see also the interview at <https://www.repository.cam.ac.uk/handle/1810/229662>.

Between 1997 and 2014, I held the Chair of Economic History and pursued a somewhat eclectic approach which brought together several of the strands mentioned above. One concern has been the changing boundaries of the market in modern Britain. A major focus has been on taxation and collective spending, understanding how levels of public spending and debt changed over time, and how a sense of legitimacy or trust was created to secure consent from taxpayers. Why has insurance against social risks shifted over time between market, charitable and public spending, and between local and central government? Why did Britain so rapidly adopt public housing after the First World War, and then move so swiftly to sell social housing in the 1980s? Why were utilities regulated, then handed to municipal or state ownership and then privatised? How were the externalities of pollution treated? An answer to these questions requires an appreciation of changing economic structures and of politics—and an appreciation that ‘interests’ are not preordained but shaped by political rhetoric and culture. Politicians and officials faced difficult choices in how to respond to economic problems, and might reach a politically contingent solution—but that solution created an institutional pattern that shaped subsequent options. Hence, the welfare reforms of Edwardian Britain utilised the administrative capacity and knowledge of trade unions and friendly societies created by working people to mitigate the risks of industrial society. These structures could influence the adoption of different policies and the reception of economic ideas, whether it be Keynes and public works in the 1930s or monetarism in the 1970s and 1980s. Further, there was an interplay between domestic and international economic policies, whether to adopt fixed exchange rates under the gold standard or allow the currency to float, to pursue free trade or protectionism, or allow free capital movements. What shaped the outcomes in Britain and in other countries, and how credible were commitments to international policies designed to mitigate the risks of globalisation? (Daunton 1983, 1985, 2001, 2008).⁸

5 Conclusion

The strength of economic history in Cambridge arises from the fact that there was never a separate department. It has been able to connect with many different types of history and economics, as well as with colleagues in geography and law. It escaped from the sense of failure in other British universities where separate departments were closed in the 1980s as a result of cuts to university

⁸ See also the interviews at http://www.history.ac.uk/makinghistory/resources/interviews/Daunton_Martin.html and <http://www.sms.cam.ac.uk/media/1587173?format=mpeg4&quality=360p>.

budgets and intellectual crisis with the rise of cliometrics, the marginalisation of Marxist approaches, and the widening of the agenda of many traditional history degrees. In most cases, economic historians moved into departments of history, with the potential danger that posts would be transferred into the more flourishing fields of social and cultural history.

In the USA, economic history survived within departments of economics, largely by adopting the techniques of the economics profession and applying theory to the past, sometimes producing real insight and sometimes provoking mutterings about empty economic boxes. In Cambridge, UK, the position was different, for economic history was a requirement both in the History and Economics Triposes, and academic posts were accordingly protected. The subject could look both ways, towards history and towards economics, with the prospect of continued collaboration from somewhat different perspectives. Furthermore, economic history in Cambridge developed its own particular characteristics as a result of five intellectual and institutional factors: a concern for the relationship between population and resources that goes back to Malthus; the work of the Department of Applied Economics of the Faculty of Economics in the creation of long-run national income data; the influence of the Cambridge school of the history of thought; a concern for political and institutional influences on markets and economic life; and connections with the history of imperialism. As a result, disciplinary boundaries were blurred and economic history was more widely diffused, with a greater range of influences and questions.

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9

Theories Came and Went, Good Data Endured: Accounting at Cambridge

Geoff Meeks

1 Introduction

To most readers it may seem surprising that accounting should be included in this volume as a significant theme of the Cambridge Faculty's first century: compared with some of the Faculty's activities it never created much heat or noise. But I want to suggest that Cambridge work at the intersection of accounting and economics has coolly and quietly shed valuable light on fields ranging from macro- and public economics to industrial organisation and regulation, financial statement analysis to economic history;¹ and that academic accounting and accounting practice are broader and more thoughtful for the Faculty's² involvement.

Its impact has been felt in many areas of policy, from paying for the Second World War, to managing the globalisation of financial markets, to mitigating the damage from climate change. Building on a discipline which traditionally used accounts to monitor the performance and solvency of just a

¹As the contributions in accounting were generally 'quiet', and were dispersed across many areas of the Faculty's work, I guess that I will have failed to include some important work. I apologise for this; and would welcome advice of such omissions.

²I am taking an inclusive view of the 'Faculty'—DAE (Department of Applied Economics) research officers and college economics Fellows, as well as University teaching officers.

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single agent—individual or business—Faculty members have pushed back the boundaries and extended the scope of accounting in many directions. They have devised ways (adopted throughout the world) of aggregating from the individual agent to the national economy, and of accounting for differences between national economies. They have extended national accounting back in time, to inform economic history. They have developed ways of linking macro with micro to integrate and forecast national, sectoral, and business accounts. They have pioneered the standardisation and electronic analysis of financial accounts for large populations of businesses. They have used management accounts to measure economies of scale. They have developed ways of measuring externalities which result in climate change and the depreciation of natural capital. In the meantime, they have also influenced the accounting profession directly—through analysis of what should be measured, and indirectly—through educating a vast number of distinguished accountants.

The chapter adopts a broader remit than would be recognised by most academic specialists in accounting. It traces two-way traffic: accounting informing economics and economics enlightening accounting. This chapter is divided into four sections. The first is national income accounting, the second business accounting. Before a final section summarising the impact of Faculty accounting outside academia, the third section is devoted to a diverse collection of ‘occasional accountants’—Faculty economists who at certain points in their careers drew on accounting to advance economics, or used insights from economics to contribute to accounting. I suspect that some of those included would be, or would have been, insulted at being tarred with the brush of accounting, which is widely viewed—I would hold mistakenly—as the dismal science’s most boring and unglamorous sister discipline. But whether or not it might be seen that way, inclusion is intended as a compliment.

2 National Income Accounting

Creating National Accounts

The range and diversity of these efforts have combined with the increasing specialisation of academics to help cloud the accounting contributions of the Faculty. This was brought home to me recently when we invited a distinguished overseas professor of accounting to give the Richard Stone Accounting Lecture in Cambridge. Of course, it was for accounting that Stone—P.D. Leake Professor of Accounting in the Cambridge Faculty—won his Nobel Prize: the 1984 Nobel Prize in Economics recognised his ‘fundamental contributions to the development of national accounts’ that ‘greatly improved

the basis for empirical economic analysis' (Nobel citation). Yet the distinguished overseas accounting professor admitted that when invited to give the Stone Lecture he had had to enquire: 'Who on earth was Richard Stone?'

Deaton (1993) explains Stone's accounting contribution:

What Stone (along with Meade, whose original vision Stone developed and made his own) should be credited with is the construction of an interlocking *system* of balanced national accounts, and the implementation of that system on a worldwide basis. Stone's system of national accounts, the SNA, published by the United Nations Statistical Office in 1953 with subsequent revisions, is not simply a set of tables containing the national income magnitudes, but a set of interlocking accounts in which the principles of double-entry bookkeeping are scrupulously maintained ... The credibility and usefulness of each of the numbers hinges on the systematic framework in which they are set. It was Richard Stone, first with James Meade in the Cabinet Office in London, and later on the world stage at the UN and the OECD, who was largely responsible for the way in which national accounts are today collected and presented throughout the world (Stone 1947; OEEC 1952).

In his Nobel Lecture (Stone 1985), Stone explains the links between his contribution and that of others in the Cambridge Faculty, and especially his debt to Colin Clark, who lectured on statistics to economists at Cambridge from 1931 to 1938, and was described by Keynes—with Keynes's characteristic diffidence—as 'a bit of a genius: almost the only economic statistician I have ever met who seems to me quite first-class' (Keynes quoted in Patinkin 1976: 1,098, fn. 9).³ Marshall, then Pigou (1920), had explored many of the conceptual issues in measuring the 'National Dividend'; Clark tackled actual measurement. Stone writes:

Colin Clark...in 1937, in his *National Income and Outlay* brought together estimates of income, output, consumers' expenditure, government revenue and expenditure, capital formation, saving, foreign trade and the balance of payments. Although he did not set his figures in an accounting framework it is clear that they came fairly close to consistency. Clark was my teacher at Cambridge and his work was the main source of inspiration for mine. Early in the second world war his estimates were updated by Erwin Rothbarth for Keynes, who used them as the statistical framework of *How to Pay for the War*.

In 1941, estimates of British national income and expenditure, which James Meade and I had worked on as civil servants in the War Cabinet Office, were published at the instigation of Keynes in a White Paper entitled *An Analysis of the Sources of War Finance and an Estimate of the National Income and*

³For an account of Clark, see Peters (2001).

Expenditure in 1938 and 1940 which was issued at the time of the budget (Stone 1985: 121; UK Treasury 1941).

The structure of the national accounts reflected the preoccupations of the new macroeconomic theory famously developed at Cambridge, from Kahn's multiplier onwards, and the consequent policy prescriptions: the accounts 'provided the means to design interventions and to monitor them in pursuit of prosperity and full employment' (Offer 2008).

Forecasting and Disaggregating National Accounts

In later years, Stone and his Cambridge colleagues built on estimates of the national accounts in two ways. They built a macroeconometric model to project future national accounts. They also differentiated this model from others at the time by pursuing 'disaggregation, which is carried much further in this than in other British macro-economic models' (Stone quoted in Barker and Peterson 1987: xvii). The resulting (1986 version of the) model had 'about 8000 variables compared with about 1000 variables in the next largest, the Treasury model' (Barker and Peterson 1987: 23). Barker and Weale (1987) emphasise the crucial role in such a large model played by a 'well-ordered system of accounting as the basis on which to link individual econometric relationships and identities', (ibid.: 46) and give priority to a detailed explanation of the accounting framework in the 'book of the model' (Barker and Peterson 1987); and Weale and colleagues made a series of general contributions to statistics within the accounting framework to reconcile statistical discrepancies.⁴ This innovative disaggregation of the economy, to the industry level, lent itself to two unusual accounting extensions: accounting for environmental externalities associated with climate change; and a joint venture with financial accounting to integrate individual company accounts into the macro framework. Both developments are discussed later in this chapter.

Retrospective National Accounts

At the same time as the Cambridge Growth Project (CGP), with Stone as 'chairman' and Alan Brown, then Barker, as 'chief executives was modelling, disaggregating, and projecting national accounts into the future, another Faculty colleague was tracing the national income accounts back into

⁴For example, Weale (1985), Barker et al. (1984), and Sefton and Weale (1995).

history. The historical dimension was published in a 1972 book by Feinstein (1972), one of three (extraordinarily energetic) qualified accountants on the Cambridge Faculty in the 1960s (the others were Cliff Pratten and Geoffrey Whittington—see Section 3). Stone had already in the 1940s initiated a project to compile retrospective national income accounts along Stone–Meade lines. In his chapter on Feinstein in this volume and in Offer (2008), Avner Offer describes the challenges this project faced, its long gestation, and Feinstein’s achievement in bringing to completion a national income series stretching back from 1965 to 1855: it reveals ‘the structure and size of the British economy from 1965 and back to Victorian times. This makes it possible to evaluate how well the economy has performed at any point in the past hundred and fifty years, and to compare it with other periods and other countries’ (ibid.: 3).⁵ Feinstein then joined forces with Robin Matthews and John Odling-Smee to deploy his data in analyses of the sources of British economic growth (Matthews et al. 1982).

3 Business Accounting

IT, Accounting and Economics

While Stone and colleagues were collating national accounts, extending them across the world, and projecting them into the future, and Feinstein was extending them back into history, another Faculty project was innovating at the micro level. Its members were collating, standardising, and analysing statistically a unique archive of company financial accounts which was eventually to stretch from 1948 to 1990.⁶ This began in the early 1960s and built on a National Institute of Economic and Social Research (NIESR) project led by Brian Tew and Cambridge’s Ronald Henderson, which was reported in Tew and Henderson (1959). The NIESR and then the Board of Trade had standardised the accounts and committed them to punched cards. The Cambridge project aimed to take advantage of new developments in computing to extend and assemble these accounts in a form which could be analysed electronically to answer questions in economics. Cambridge was peculiarly well equipped to develop such work because of its lead in computing at the time. The hardware

⁵ Offer makes clear Feinstein’s debt to other Cambridge researchers with an accounting bent: ‘It was a golden age for the study of trends in the Victorian economy, a good deal of it at Cambridge’ (Offer 2008: 5), with major Faculty publications including Deane and Cole (1962) and Mitchell (1988).

⁶ By which time commercial providers offered databases suitable for such work. The 1948–1990 data are available from the ESRC Data Archive at Essex University.

and expertise newly available in Cambridge made possible a step change in the volume of data which could be processed: a whole population of UK quoted companies' financial accounts was for the first time assembled in digital form and analysed econometrically. Geoffrey Whittington, a key figure in this development, recalls joining the lines of famous mathematicians, astronomers, and geologists queuing to use the pioneering Cambridge computers, the series of false starts as different approaches and machines were tried, and the many night shifts required to gain time on the machine. In addition to access to such path-breaking hardware, the Faculty researchers benefited also from outstanding software specialists, especially Harry Burley and Joyce Wheeler.⁷ The resulting statistical analyses drew on much more substantial bodies of evidence than could previously have been managed: landmark publications from this work include Singh and Whittington (1968, 1975), Singh (1971), and Whittington (1971, 1972).

The Impact in Economics of the Faculty's Analyses of Company Accounts

The nature of such work and its prominence in British empirical research at the time are well illustrated by the UK Government's 1978 Green Paper on Monopolies and Mergers Policy (Hattersley 1978), which led to changes in UK policy and law on industrial organisation. A remarkably large proportion of the evidence base for this Paper came from accounting studies in the Cambridge Faculty. The Hattersley Review drew upon Whittington's (1971) analyses—using the Cambridge databank of companies' financial accounts—of the relationship between company size and profitability: above a modest threshold level, there was little evidence of the positive relationship which might have been expected from increased market power and economies of scale. The same data archive was the basis for evidence cited from Meeks and Whittington (1975) on executive pay—the financial incentives for executives to merge even where this did not enhance performance. Again that same Cambridge data source played a central role in the findings reported in the Review from Singh (1971) on disciplinary takeover. He addressed the proposition in the theoretical work of Marris that the

⁷Their contributions were recognised on the title pages of Singh and Whittington (in collaboration with H.T. Burley) (1968) and Meeks et al. (1998).

prospect of hostile takeover would deter executives from pursuing managerial objectives, such as growth maximisation or satisficing, at the expense of profitability and share price. Singh's evidence did not provide much support for that proposition. Still more analysis based on the Cambridge accounting databank was cited in the form of Meeks's (1977) study of changes in the profitability of merging firms. This failed to find compelling evidence that financial performance improved after the typical merger.⁸ The Review also drew on Pratten's monumental (1971) study of economies of scale, which analysed evidence he had painstakingly elicited from cost accountants to quantify for each industry minimum efficient scale and the extent of increasing returns to scale.

This stream of research, analysing financial statements to answer questions in industrial organisation and corporate governance, was subsequently developed at Cambridge chiefly by Alan Hughes, Andy Cosh, and their collaborators. Examples from a large body of publications include Dunne and Hughes (1994), which confounded a widespread view that industrial concentration was rising inexorably with results that smaller companies were actually growing faster than larger ones; Cosh et al. (1980) and Kumar (1984) which extended the scope of merger studies using accounts, with international comparisons and innovative approaches to measurement; and influential papers on the relation between owners and managers, including Cosh (1975), investigating the impact of growth and profitability on directors' pay, and Cosh et al. (1989), analysing the role of institutional investors in the market for corporate control. These themes developed into a division of the new Centre for Business Research (CBR) at Cambridge, directed by Hughes, which, although housed in the Business School, welcomed disciplines from across the University, including economics, engineering, and law. This diversification into cognate disciplines coincided with a period in which the Faculty was withdrawing from sister fields such as accounting, development studies, finance, politics, and sociology. It was warmly welcomed and funded by the ESRC, which praised the CBR's 'first class programme of interdisciplinary research', especially 'first class contributions to the analysis of corporate governance and the growth and performance of small and medium sized enterprises'.⁹

⁸ Although based on UK data, such studies, and subsequent ones by Cosh, Hughes, and their colleagues (discussed below), influenced the US literature. For example, there were many citations of these authors in the standard industrial organisation text of Scherer and Ross (1990), as well as references in the standard US text on financial statement analysis (Foster 1986).

⁹ From the (unpublished) report of the ESRC Evaluation Panel.

Other Developments in Financial Accounting for Business

Database development continued for many years. In collaboration with the UK Government's Board of Trade (and subsequent reincarnations, for example, the Department of Trade and Industry (DTI)), which carried out the primary standardisation of accounts in order to produce industrial analyses in *Business Monitor* publications, the Cambridge Faculty went on to extend the database of accounts until 1990 (Goudie and Meeks 1986; Meeks et al. 1998). Whittington had moved on in 1972, to chairs in Edinburgh and Bristol, but advised from a distance, and spent a spell back in Cambridge working on his influential conceptual book on accounting for price changes (Whittington 1983).¹⁰

By the time of his return to the Economics Faculty as Price Waterhouse Professor of Financial Accounting in 1988, Whittington had developed a range of interests in accounting theory and accounting regulation (Whittington 2007). His 1990 paper with David Tweedie set much of the agenda for accounting regulation for the next decade, and he was asked to join the new UK Accounting Standards Board and then, in 2001, the International Accounting Standards Board (IASB)—both of them chaired (to acclaim) by Whittington's co-author, Tweedie. In this role, Whittington can partly be seen as resuming his efforts to standardise company accounts: in 1960s Cambridge this was to produce data amenable to comparative statistical analysis by economists; around the millennium it was to allow investors to conduct reliable comparative financial statement analysis for companies across the UK Stock Exchange and then (with the IASB) across the world's equity markets.¹¹ Philip Brown, one of the fathers of the dominant modern accounting stream in the USA, wrote (2008): '[A]ny careful reader must be impressed by the sheer quality and practical import of Whittington's work over more than 40 years ... Whittington has rendered great service to academia, to British commerce and industry, and to the accounting world at large' (ibid.: 559).

¹⁰ There was a lively interest throughout this period among Faculty members in how to measure profit. Publications include King (1975), Meeks (1974), and Meeks and Meeks (1981). There were vigorous debates, discussion papers, and newspaper articles involving, among others, Wynne Godley, Bob Rowthorn, and Adrian Wood (the latter working on his 1975, *A Theory of Profits*).

¹¹ The Economics Faculty provided a very active environment for analysing regulation in this period. Whittington participated in the work of David Newbery's team on the regulation of utilities and helped inform Meeks's work on estimating the costs and benefits of accounting regulation (Meeks and Meeks 2002; Gwilliam et al. 2005; Meeks and Swann 2009).

In his 1971 book, *The Prediction of Profitability*, and various papers, such as Whittington (1972, 1978), Whittington had exploited the Cambridge databank to identify information in companies' past accounts which might predict their future profitability—for example, their size, their past rate of growth, and their sources of finance. The ambitious development of Stone's national income accounting by Barker, Peterson, and their colleagues made possible another approach to predicting companies' future accounts. The huge CGP multisectoral macroeconomic model of the UK was linked by Andrew Goudie and Geoff Meeks to companies in the Cambridge databank. The CGP model had achieved sectoral/industrial disaggregation of past and projected national accounts. Marrying industries with individual large companies enabled Goudie and Meeks to model the impact of macroeconomic developments—for example, a change in the exchange rate—on the future accounts of individual companies, and to aggregate and feed back the resulting company sector financial accounts into the macroeconomic model (Goudie and Meeks 1981, 1982; Goudie et al. 1987).

Building on the sector-level relationships developed by King (1976), each of the hundred largest companies was separately modelled within the macro model. This allowed for the elimination of aggregation bias in econometric estimates—for instance in the dividend function (Goudie et al. 1987). The extra information on the distribution of values across members of the company sector made it possible to answer questions not amenable to aggregate analysis, for example, how great was the impact of tax exhaustion on tax receipts from companies (Goudie 1984). Also, the availability at the company level of balance sheet data as well as flow accounts permitted new approaches to modelling company failure, estimating the impact of macroeconomic developments on the solvency, and probability of failure, of individual businesses (Goudie 1987; Goudie and Meeks 1991, 1998).

4 Occasional Accountants

Several important Faculty contributions at the intersection of accounting and economics extend beyond the themes outlined so far. These include work in microeconomic theory, macroeconomic theory, and environmental economics.

Microeconomic Theory of Accounting for Business

Although he is primarily known for his contributions to economic theory and the history of economic thought, Geoff Harcourt's PhD in the Cambridge Faculty was actually an empirical piece on replacement cost accounting.

This experience combined with his theoretical interests led to his famous 1965 paper, 'The Accountant in a Golden Age'. He showed that, even under helpful 'Golden Age' assumptions, comparisons of the accountant's rate of profit for different industries, or the same industry in different countries, could not be reliably used for inferences about relative economic profit rates—internal rates of return. He showed the distortions which are introduced by different lengths of lives for machines, patterns of quasi-rents, and depreciation methods.¹²

These issues were revisited two decades later in the Faculty by Jeremy Edwards and his co-authors (Edwards et al. 1987 (EKM)).¹³ This followed a renewal of interest in profit measurement in the aftermath of aborted attempts to introduce inflation-adjusted accounts in the USA and UK. EKM addressed the problem posed by Harcourt, and provided a crisp argument that amendments are possible to conventional accounting profitability measures which would permit legitimate comparisons across activities. In particular they propose the use of deprival value (value to the owner) conventions for valuing capital employed, and full articulation of the income statement and the balance sheet (all changes in book value flowing through the income statement). Since that work the accounting mainstream has flowed in a different direction, preoccupied with current market valuations. But now that the limitations of marking-to-market have become more obvious (Whittington 2008; Amel-Zadeh and Meeks 2013) the approach of Harcourt and EKM—motivated by the managerial decisions that accounting data have to inform—is overdue for a revival.

Accounting Identities in Macroeconomic Theory

Wolf (2012) explains how Wynne Godley used the accountant's articulation of flow accounts—income and cash flow statements—on the one hand, with the stock account—the balance sheet—on the other. Godley focused on the

¹²Harcourt is also well known for his work on the capital controversies involving the 'two Cambridges'. Since much of accounting is devoted to measuring capital, it is perhaps surprising that those controversies were not more evident in the accounting studies in the Faculty at the time. In relation to one of the qualified accountants, Feinstein, Offer comments in his chapter in this book: 'Charles took no part in the theoretical debates convulsing Cambridge, on whether capital was a coherent and tractable category. His job, he said, was merely to measure its historical cost. But the acrimony of the debate unsettled him'. Although the debate and some of the participants were indeed acrimonious, this is not a description that could ever be applied to the generous Harcourt. Those controversies will be presented elsewhere in this book—by experts.

¹³Their theoretical discussion was buttressed with data from the Cambridge/DTI databank of company accounts discussed earlier.

financial balances in the accounts of the various sectors of the economy (personal, corporate, government, and overseas) governed by the principles of double-entry bookkeeping. In Wolf's words:

The essential idea is that since income has to equal expenditure for the economy, as a whole (which is the same as saying that savings equal investment), so the sums of the differences between incomes and expenditures of each of these sectors of the economy must also be zero. These differences can also be described as 'financial balances'. Thus, if a sector is spending less than its income it must be accumulating (net) claims on other sectors. The crucial point is that, since sectoral balances must sum to zero, a rise in the deficit of one sector must be matched by an offsetting change in the others. It follows that if the fiscal deficit is increasing, the sum of the surpluses of the other sectors of the economy must be increasing in a precisely offsetting manner

This framework of accounting identities, developed, for example, in Godley and Lavoie (2007), has been deployed to analyse the recent recession. In the wake of the banking crisis, the private sector in leading Western economies has been paying down debt rather than spending; the increase in net private sector saving has not been matched by the increase in government borrowing, and this, it is argued (e.g. by Koo 2011) contributes to 'balance sheet recession'.

The accountant in Godley is not just evident in his models being disciplined within an accounting framework. Some of his colleagues told me at the time that what gave Godley his 'edge' in forecasting, leading to some of his famously prescient predictions, was his intimate understanding and knowledge of the national accounts (the data he had pored over in his work at the Treasury).

Environmental Accounting

One strand of Cambridge work in accounting work harks back to preoccupations of Faculty members in the 1920s and 1930s—in, for example, Pigou's *Economics of Welfare* (1920).¹⁴ Accounting for externalities, a key concept in Pigou, received scant attention in the Faculty in the third quarter of the last century, but was taking centre stage towards the millennium, with concerns

¹⁴Writing on the 'National Dividend', and building on the work of Marshall, Pigou observes, for example, 'It is a paradox...that the frequent desecration of beauty through the hunt for coal or gold...must, on our definition, leave the national dividend intact, though, if it had been practicable...to make a charge for viewing scenery, it would not have done so' (Pigou 1920: 32). Another distinguished contributor to this long-running discussion, who spent two spells working at Cambridge is Nobel Laureate Amartya Sen (see, for example, Stiglitz et al. 2009).

over the global warming and environmental degradation which were accompanying (conventionally measured) GDP growth.

Barker developed a descendant of the CGP's model, with its disaggregated industry accounts, to analyse policies for the mitigation of greenhouse gases, an output, or by-product—an economic 'bad' rather than an economic 'good'—conventionally left out of business and national accounts (e.g. Barker and Scricciu 2009). He drew on this work as one of the coordinating lead authors for the *Fourth Assessment Report of the Intergovernmental Panel on Climate Change* (IPCC 2007 and for the *Third Report*). He was recognised as one of those who had 'contributed substantially to the preparation of IPCC reports...contributing to the award of the Nobel Peace Prize for 2007 to the IPCC' (IPCC 2012).

Partha Dasgupta has also tackled problems of environmental externalities unaccounted for in conventional income measures. He builds on Hicks's definition of income: the maximum amount that could be consumed while maintaining capital intact. The accountant assesses whether capital has been maintained intact with the balance sheet, and in one of his contributions Dasgupta proposes innovative ways of incorporating the natural environment as natural capital—including, for example, soil, fisheries, fresh water, and the atmosphere—in the balance sheet. He explores the depreciation of natural capital, which should be represented in the balance sheet and income statement but is routinely ignored in accounts; yet it can be sufficiently important that, if it were recognised, it could entirely offset the incomes conventionally recorded (Dasgupta 2008).¹⁵ Among Dasgupta's many honours is the 2002 Volvo Environment Prize, which recognises among other achievements his part in founding ecological economics, 'his decisive work on the economics of exhaustible resources, his insights on their role in providing essential ecological services' (Volvo Environment Prize 2002).¹⁶

Behind the Scenes Contributors

Externalities of a different sort are behind the inclusion of my last two occasional accountants. Although both produced distinguished publications, neither of them would be recognised as primarily in accounting. Yet both were responsible for positive externalities which did much to raise the Faculty's game in accounting. Both were/are Faculty 'lifers', whose service has spanned seven decades and counting.

¹⁵ See Dasgupta (2001) for a more extended discussion.

¹⁶ Dasgupta has also been appointed by the Indian government to lead the effort to create Green National Accounts for India.

Brian Reddaway had a deep understanding of accounting. He practised ‘financial statement analysis’ before the term was invented, advising his College—Clare—on its investments in companies, as well as the wider world, through his ‘Academic Investor’ column for the *Investors Chronicle*. He directed the Singh/Whittington project on company accounts, and his role as constructive critic is very warmly acknowledged in the publications emerging from that project, as in so much work published by the DAE while he was Director. Offer (2008) describes him as ‘the greatest Cambridge influence on Charles [Feinstein]’. I was a very grateful beneficiary of his grasp of accounting and his skill in inference in my work on takeover and bankruptcy. The inductive approach he deployed in scrutinising and improving our work was for long embedded in Tripos training in applied economics—often based on national accounts data.

William Peterson actually taught national income accounting in the Tripos from the 1980s until after the millennium, so that generations of Tripos students had a formal framework for the accounts-based exercises which formed a distinctive part of their training in applied economics—one much appreciated by many employers. He was a linchpin of the Faculty’s teaching effort, contributing disproportionately to the management of an effective Tripos when many colleagues were busy feuding or promoting their own careers. On the research side, his contribution to the CGP founded by Stone has been mentioned earlier. There his extraordinary grip on the range of accounting *and* programming *and* econometrics *and* economic theory meant that he naturally fell, alongside Barker, into the central role of building the infrastructure of the massive model. Then the rest of us¹⁷ benefited greatly from his ‘upstream’ work, using the model in our various specialist publications.

5 The Impact of Faculty Accounting Outside Academe

Current assessments of research in UK universities are widening the criteria from publication directed at other academics to include also the impact of the research on the world outside academe.¹⁸ The accounting-related activi-

¹⁷Who in my limited time on the Project included a future President of the American Economic Association, a Governor of the Bank of England, a Director of the National Institute of Economic and Social Research, a Chief Economist for Scotland, and holders of many prestigious professorships.

¹⁸The bibliography to this chapter (far from comprehensive) gives a clue as to the extent of accounting-related publications by Faculty members. Much of this is in sister disciplines of accounting. Within the more conventional boundaries of academic accounting observed in most universities, an indicator of publication rates by Faculty members (and faculty elsewhere in the University who had been trained in the Economics Faculty) is given by Chan et al. (2006): they report that Cambridge accounting was fourth in Europe in its per capita rate of publications in the main accounting journals.

ties of the Faculty offer a lengthy scorecard in this respect. Thus, Stone was a central figure in the construction of the UK's, and then the world's, national accounts. Whittington was a central figure in the overhaul and reconstruction of first the UK's, and then the world's, business accounts. Barker, with Peterson, led the way in engaging big business—companies such as IBM and ICI—in modelling and forecasting disaggregated national accounts; and Barker played a lead role at the IPCC. Dasgupta has been a leader in developing Green National Accounts for India.

Then there was an important indirect channel of influence on practice. At times in the twentieth century the accounting profession was (by some margin) the leading recruiter of graduates from the Economics Tripos. For example, in the four years (1979–1982), 150—more than a quarter—of those whose destination was known by the University Careers Service joined the accounting profession, almost twice as many as went into banking, broking, and so on, the next most popular destination.^{19,20} Despite not having an accounting Tripos or an accounting department,²¹ Cambridge was evidently well thought of for accounting: the 2012²² QS World University Rankings placed Cambridge accounting tenth in the world on the basis of citations and evaluations by employers and academic peers. The recognition also took a form which both economists and accountants find particularly persuasive: Stone's chair was funded by the Institute of Chartered Accountants in England and Wales (ICAEW) and Whittington's by Price Waterhouse.²³

¹⁹ I am extremely grateful to officers in the University Careers Service, in particular Mary Blackman, Gordon Chesterman, and Alan Fawcett, for access to their excellent long-term records of destinations of graduates.

²⁰ More recently the primary destination has become the banking industry: in the decade up to the 2007–2008 financial crisis value added in the financial sector of the UK economy grew at around 6% per annum, twice the rate of GDP (Burgess 2011), and this meant that the sector absorbed many Cambridge graduates. Even after the crisis, 143 of those graduating from the Economics Faculty went into banking in the four years after 2008. Only 29 joined accounting firms (University Careers Service data).

²¹ Except in the MPhil in Finance in the 1990s the Faculty did not have an exam paper devoted just to accounting. Peterson gave a course on national income accounting within the Part 1 Statistics paper from the 1980s, and Whittington, then Meeks, gave one on traditional business accounting in the 1990s. Nevertheless, national income accounts were for decades an important part of the regular diet in applied economics for Tripos students: the Blue Book was a required purchase when I joined the Tripos in 1969.

²² Although this post-dates the Faculty's withdrawal from accounting, this reputation was built up by members of the Economics Faculty or Cambridge academics in other departments who had been trained in the Faculty.

²³ Declaration of interest: I have benefited from an ICAEW Fellowship, research funding and their platform to engage with the profession, for example, the 2008 P.D. Leake Lecture.

6 Conclusion

There are few traces of accounting now left in the Faculty. In the process of departmental fission by which Cambridge tends to evolve (economics out of moral sciences, sociology out of economics, etc.) much of accounting has moved into the new (in Cambridge time) Business School. But it survived longer than in most economics departments elsewhere, and this chapter suggests some reasons for this. First, in contrast with some universities, there was a very indistinct frontier between accounting and economics for much of the Faculty's first century. People were comfortable straying over the frontier in both directions as their current research problem demanded. Second, as I have tried to illustrate, the activity delivered results, recognised by academic peers, and the outside world. Third, the participants were not identified with a particular single faction in the Faculty's feuds, and the activity was often seen as worthy, but boring, and no threat to the glamorous high theorists or policy maestros. So the participants in accounting were allowed to continue quietly with their measurement activities, confident that ultimately, in the words of an American CEO,²⁴ 'what gets measured is what gets done'.

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²⁴ Carly Fiorina, of Hewlett-Packard.

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Part II

Some Cambridge Economists

10

William Paley (1743–1805)

A.M.C. Waterman

1 Introduction

Though traces of ‘economic thought’ in Cambridge may be detected as early as the 1770s, the earliest example to appear in print is contained in William Paley’s *The Principles of Moral and Political Philosophy* (1785), which brought its author instant fame and fortune. On the strength of its Book VI, Chapter XI, Maynard Keynes conjectured that ‘Perhaps, in a sense, *he* [rather than Malthus] was the first of the Cambridge economists’ (Keynes 1933 [1972]: 79, fn. 2; italics in original).

William Paley (1743–1805) was a Yorkshire man and was mildly derided at Cambridge for speaking Latin with a Yorkshire accent. He was the son of a clergyman and Cambridge graduate who became Master of Giggleswick School. The biographical information that follows is adapted from Waterman (2011).

I acknowledge with gratitude the generous assistance of Mr Nicholas Rogers, archivist of Sydney Sussex College, Cambridge University, for giving me access to the Hey Archive and for transcribing material from volume III of Hey’s ‘Lectures in Morality’. I also wish to acknowledge permission from Oxford University Press to make unrestricted use of Waterman (1996), much of which is incorporated in sections 3 and 4 of this chapter. Also, I am very grateful to Dr Neil Hitchin for showing me his as yet unpublished report on Paley’s lecture notes, which include detailed notes of the lectures on ‘Moral Philosophy’ made by W. Thomas, son of Hugh Thomas, Master of Christ’s during Paley’s tenure as a Fellow and an undergraduate student of the College from 1774 to 1779.

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Paley was born in Peterborough in July 1743 where his father, the Reverend William Paley, was a minor canon of Peterborough before his appointment to Giggleswick in 1745, upon which the family returned to Yorkshire. Like his father, the younger William went up to Christ's College, Cambridge, where he matriculated in 1759, graduating BA in 1763 as Senior Wrangler, meaning that his was the highest performance of all Mathematical Tripos candidates in that year. After three years of school teaching in Greenwich, he was elected Fellow of Christ's in 1766 and ordered deacon. In 1767, he was ordained priest and graduated MA. He was awarded the Doctor of Divinity degree in 1795 for his *Evidences of Christianity* (1794), which remained on the reading list for 'Little Go' (Cambridge Previous Examination) until the 1920s.

For 10 years after his election as Fellow, Paley occupied various college offices and played a large part in teaching undergraduates. At that time, this was almost entirely conducted in the colleges by college lecturers and tutors. The few university lectures delivered by a small handful of professors were optional and scantily attended. But college classes were compulsory. All undergraduates faced a common curriculum designed to prepare the next generation of clergymen, magistrates, and legislators for their public duties in a Christian society: the Latin and Greek classics supplemented in some colleges by biblical languages and literature, and some reading in 'moral and political philosophy'. Many undergraduates went down after one or two years of this, and most of those who remained in residence received the ordinary BA without examination, after keeping the requisite number of terms.

A small minority of ambitious students who needed to make their way in the world, and who were usually of relatively humble origins, sought an honours degree and competed in the Tripos examinations. These consisted of five public disputations in Latin, using Aristotelian logic, on theses acceptable to the Moderators. However, by the middle of the eighteenth century, the oral tests were supplemented by a written 'Senate House Examination' in mathematics—the first written examinations in any university of the Western world. Candidates for honours therefore supplemented their other studies with Newtonian 'natural philosophy' and mathematics.

Paley was soon known throughout Cambridge as a superb teacher, and many students came from other colleges to attend his lectures. A much later commentator wrote of Paley's 'utter inability to be obscure' (Annan 1984: 244). In all probability, Paley taught the entire curriculum with the exception of the classics, in addition to mathematics and natural philosophy for Tripos candidates in Christ's College.

In 1776, 10 years of hard work—with the assistance of patronage—brought its reward. Paley was preferred to the rectory of Great Musgrave, Westmorland. Thus, at last he was able to marry, resigning his Fellowship as was then required. His wife, Jane Hewitt of Carlisle, bore him ten children, two of whom died in infancy. He remained in the diocese of Carlisle for the rest of his career, on terms of cordial friendship with his mentor, patron, and ecclesiastical superior, Edmund Law, Bishop of Carlisle (1768–1787), who had been a Fellow of Christ's in the 1730s, and Master of Peterhouse and Knightsbridge Professor of Philosophy when Paley was a young man. Paley's energy and efficiency soon led to his promotion as Archdeacon (1782) and Chancellor (1785). But he also held benefices later in the dioceses of Lincoln and Durham, and in 1796, he moved to Bishop Wearmouth in Durham whilst remaining Archdeacon of Carlisle. He was exemplary in parochial and diocesan duties, active in promoting education of the poor, and a leader in the campaign to abolish the slave trade. He also advocated tithe reform and supported independence for the American colonies. His first wife having died in 1791, he married Catherine Dobson of Carlisle in 1795. Paley died on 15 May 1805 after a lingering and painful illness, during which he completed his last book, *Natural Theology* (1802). He is buried in Carlisle Cathedral.

Paley's daughter, Mary, was grandmother to Mary Paley (1850–1944), who was among the first five women to enter Newnham College when it opened its doors in 1875. She read for the Moral Sciences Tripos and married her economics instructor, Alfred Marshall.

Soon after leaving Cambridge, Paley was urged by his bishop and other friends to write up his college lectures for publication. He began with *Moral and Political Philosophy* (1785), a spectacular success for which his publishers paid £1,000—more than Malthus was to earn from all his books in a lifetime. It was almost at once adopted as a required text for all undergraduates at Cambridge, went through 20 English editions by 1814 (15 in Paley's own lifetime) and 10 American editions by 1821. In the USA, it remained 'the most popular text on moral philosophy from the 1790s to the Civil War' (Haddow 1939: 67). Though long superseded as a textbook, its sparkling lucidity still had appeal for the discriminating in the twentieth century: 'If anyone will take up again Paley's *Principles*', Keynes declared in 1933, 'he will find, contrary perhaps to his expectation, an immortal book' (Keynes 1933 [1972]: 79, fn. 2).

The first five books of *Principles* are concerned with moral philosophy: obligation and duty—to God, to our neighbour, and to ourselves. Book VI contains 'Elements of Political Knowledge', with chapters on government, obedience and civil liberty, the British constitution, the administration of justice, and religious and military establishments.

Since there can be no obligation to do that which is unfeasible, moral and political philosophy must entail some positive investigation of the economic and social circumstances to which normative principles apply. Therefore an element of what we now call ‘economic analysis’ is always to be found, implicit or explicit, in almost all expositions of political philosophy at least since Plato’s *Republic*. What we might think of now as Paley’s ‘economic thought’ was neither ‘political œconomy’ in the sense of either Sir James Steuart or Adam Smith nor ‘economics’ as later conceived by his great-grandson-in-law. It is entirely contained in the penultimate chapter of *Principles*, Book VI, Chapter XI: ‘Of Population and Provision’, which, though only one out of 88 chapters, is long and complex and comprises 8% of the entire text.

What follows consists of three parts. Much of the material, especially in the second and third parts, is adapted from Waterman (1996), some of it verbatim:

First, an examination of Paley’s method of thought: the intellectual context of mid-eighteenth-century Cambridge in which it emerged including such evidence as there is of economic thinking by his Cambridge contemporaries; his individualist, proto-utilitarian view of society; and the possible influence of other eighteenth-century authors such as Bernard Mandeville, David Hume, George Berkeley, Steuart, and Smith.

Second, a formalisation of Paley’s implicit demand-led, two-sector macro model: the stability of its equilibrium and the possibility of comparative statics, its generalisation of Mandeville’s *Fable of the Bees*, its explicit recognition of what is perhaps the first example of optimisation in economic thought; and other economic topics in ‘Population and Provision’.

Third, a consideration of what may have led Keynes, writing in the early 1930s when his own economic thinking was in flux, to be so impressed by Paley’s analysis, and to think of him as ‘the first of the Cambridge economists’.

2 Paley’s Method of Thought

The Cambridge Context of Paley’s Economic Thought

Since Paley included some analytical treatment of economic matters in Book VI of *Principles*, and since this is presumably based, like Books I–V, on his college lectures, it seems highly probable that at least some tutors and lecturers in other colleges did the same. A great deal of what we now recognise as ‘economic literature’, both in French and in English, was becoming available to the learned from about

the middle of the eighteenth century, and it is unlikely that Cambridge dons—who were an important part of a very small intellectual elite in England at that time—would not have been aware of it. But evidence of this is hard to come by. Hints may be found here and there in correspondence between undergraduates and their parents, for example, such as that between ‘Bob’ Malthus and his father, or Philip Yorke and Lord Hardwicke (Searby 1997: 545–561). However, no one has yet attempted the immense and possibly fruitless task of collecting all surviving correspondence and extracting such information from it.

One straw in the wind has recently come to light, however. A contemporary of Paley’s, John Hey (1734–1815), like him a Yorkshire man and almost certainly a friend and fellow member of the Hyson Club (founded in 1758 by wranglers), was a Fellow and Tutor at Sidney Sussex College from 1758 to 1779. In 1780, he became the first Norrisian Professor of Divinity, and there is evidence that Malthus attended his university lectures in that chair. During the 1770s, Hey gave a series of college lectures on ‘morality’ for his Sidney Sussex pupils which attracted the voluntary attendance of undergraduates from other colleges including William Pitt the Younger, then (1773–1776) at Pembroke College.

In 1997, whilst conducting research for an article on Hey in the *New Dictionary of National Biography*, I discovered in the archives of Sidney Sussex nine bound volumes of his unpublished ‘Lectures on Morality’ which had been deposited by his brothers after his death in 1815 and which, so far as I have been able to discover, have never been looked at by anyone since. The manuscript had been intended for publication, and volume I (p. 7) contains a ‘Preface’, written in 1814, which includes the following passage:

I do not recollect at what time D^r Paley began to read in morality at X’s College; but as we were contemporary readers, it has always seemed best to me to let our disquisitions be independent of each other. For this reason only I have abstained from reading D^r Paley’s Moral Philosophy, tho’ I have perused all his other works with attention and admiration. I have not in the least thought of setting my lucubrations in any competition with his Moral Philosophy.

Hey’s Norrisian *Lectures in Divinity* had been highly successful and continued in use throughout the Anglophone world—including both Canada and Australia—into the 1840s. But despite repeated attempts, he could find no publisher for his Lectures on Morality: for alas, Hey’s ‘lucubrations’ were indeed in competition with those of his colleague. Moreover, after 1785 there was room only for one textbook in that field: ‘D^r Paley’s Moral Philosophy’.

It is instructive to compare Hey's treatment of economic matters with Paley's. Though there is an answer to Mandeville in volume I, there is no attempt to formulate a macroeconomic model of the interdependence of 'luxuries' and necessities. But in volume III, there are seven lectures on 'permutatory contracts' relating to the exchange of goods and services; to the nature and use of money; to buying and selling; and to the letting and hiring of persons, things, and money. Although the intention is normative and the treatment at times quasi-legal, analysis is never far below the surface:

I confess I have never in any author seen the idea of value or price made clear and satisfactory, tho' all authors who have written on natural law, have offered explanations of it. This makes me desirous to convey my idea, by way of trying whether it will be more satisfactory (Hey (deposited 1815): volume III, 1,325; underlining in original).

Hey suggests that the best way 'to conceive the value of anything according to men's general wants' would be 'to suppose all men bidding for it at an universal auction'. Hence 'the value of any one thing, when x of them are saleable, is the x th part of the least sum which only x persons could be prevailed upon to give up for them all' (ibid.) In developing this idea Hey's exposition seems to be a possible source of the first-ever formal demand function, which was specified by Malthus (1800) 24 years later. As with Malthus, Hey seems to have had in mind a demand function of price interacting with a price-inelastic supply curve that may shift from time to time. If there was a characteristically 'Cambridge' way of conceiving value theory in the eighteenth century, Hey's lectures are important evidence of it. He may even perhaps be enrolled, along with Smith and Malthus, among the pioneers of the supply-and-demand approach to value theory that 'won out ultimately' (Schumpeter 1954: 482) over the labour theory of value maintained by Ricardo and Marx.

The lectures in volume III on 'economic' topics are variously dated from 12th to 21st November 1776, by which time Paley had quit Cambridge for Cumberland. Neil Hitchin's recent discovery of a student's notes of Paley's lectures suggests that they were delivered, possibly on several occasions between 1775 and 1776, probably in 1775 itself, and that manuscript copies of his lecture notes circulated in Cambridge after his departure (Hitchin n.d.). This evidence is consistent with Hey's recollection that he and Paley were 'contemporary readers'. Yet it is remarkable that there could have been so little intellectual contact (at least about their teaching) between two friends working in almost neighbouring colleges, and also that there should have been such dissimilarity in their college lectures on the same topic. Like Hey's, Paley's

lectures (or at any rate his book which is based on them) contain treatments of contracts: of sale, of hazard, of lending, and of labour (Paley 1785: Book III, Part I, Chapters VI–XIV). But there is no trace of the economic analysis with which Hey informed his exposition of these topics. Paley may have been ‘the first of the Cambridge economists’, but value theory played no part whatsoever in his thinking. Hey, on the other hand, had nothing to say about anything we would now call macroeconomics.

3 Utilitarian Ethics and Methodological Individualism

In one very important respect, however, Paley and Hey were at one. The normative social theory of each was utilitarian in the original, Cambridge, sense. Utilitarian ethics seem to have originated with John Gay’s ‘Dissertation concerning the Fundamental Principle of Virtue or Morality’ published as a Preface to the English translation by Paley’s patron Edmund Law (1731) of William King’s *De Origine Mali* (1702). Gay, who was a Fellow of Christ’s from 1724 to 1732, argued that the happiness of mankind is willed by God, and therefore that humans are obliged to act so as to maximise the happiness of their fellow creatures. This position seems to have been taken for granted in Cambridge from the mid-eighteenth century and would undoubtedly have been taught at Christ’s when Paley was an undergraduate (1759–1763)—as it had been when his father was an undergraduate during Gay’s tenure as a Fellow. Paley develops the theme in *Principles*, which was almost contemporaneous with Jeremy Bentham’s *An Introduction to the Principles of Morals and Legislation* (1789). Bentham’s utilitarianism is sometimes characterised as ‘Paley with God left out’. Paley begins his ‘economics’ chapter in *Principles* by declaring that ‘The final view of all rational politics is to produce the greatest quantity of happiness in a given tract of land’ (Paley 1785: 587). In Hey’s lectures on contracts, ‘the good of mankind’ is similarly normative (e.g. Hey (deposited 1815): volume III, 1,280–1,281).

Utilitarian thinking is congruent with both political and methodological individualism. The latter was implicit in much eighteenth-century economic thought, as in *The Wealth of Nations* in which the individual decision-maker, each independent master acting entirely for his own benefit, is the prime mover (see Arrow 1994). But Paley seems to have been the first economic thinker to make explicit the assumptions of both kinds of individualism. At the outset of ‘Population and Provision’, he states his position in a passage the first sentence of which foreshadows a somewhat similar declaration by former British Prime Minister Margaret Thatcher:

[A]ltho' we speak of communities as sentient beings; altho' we ascribe to them happiness and misery, desires, interests and passions, nothing really exists or feels but *individuals*. The happiness of a people is made up of the happiness of single persons; and the quantity of it can only be augmented by encreasing the number of the percipients, or the pleasures of their perceptions (Paley *ibid.*: 587–588; italics in original).

In one interesting respect, the penultimate clause of this affirmation is eccentric. How can 'the happiness of a people' possibly be 'augmented by encreasing the number of the percipients'?

Paley assumed—subject to important qualifications—that the 'happiness' of each individual is rather like his weight: it could be represented as a scalar magnitude and thereby aggregated with those of others in order to get a total happiness (weight, etc.) of the relevant social unit: '[T]he collective happiness will be nearly in the exact proportion of the numbers, that is, twice the number of inhabitants will produce double the quantity of happiness' (*ibid.*: 588). Moreover, although this only holds true in 'adjoining periods, in the same country', in general 'it may, and ought to be assumed in all political deliberations, that a larger portion of happiness is enjoyed among *ten* persons, possessing the means of healthy subsistence, than can be produced by *five* persons, under every advantage of power, affluence and luxury' (*ibid.*: 588–589; italics in original).

In effect, Paley has implicitly formulated the first social welfare function: 'collective happiness' $U = U(N)$, where N is population and $U' > 0$. In the macroeconomic analysis which occupies much of the rest of his 'economics' chapter, maximisation of population is the policy goal. It is essential to distinguish Paley's reasons for this from those of the 'political oeconomists' of the seventeenth and eighteenth centuries for whom the maximisation of population was likewise a policy goal. Political oeconomists from Antoine de Montchrétien to Steuart sought to increase the wealth and military power of the nation state. Population was merely instrumental: more bodies meant lower wages and larger armies. Their normative criterion was the welfare of *le roi soleil* and other heads of state. Paley was radically 'modern' in identifying the welfare of individuals, rather than that of the sovereign, as the proper object of public policy: 'The riches, strength, and glory of nations...have no value farther than as they contribute to...the happiness of a people' (*ibid.*: 587).

4 Other Possible Influences on Paley's Economic Thought

By the third quarter of the eighteenth century, a great deal of what become 'political economy' in the nineteenth century and 'economics' in the twentieth had begun to circulate in informed circles in France, Italy, Scotland, and England, and the outlines of a common body of knowledge can be identified:

- (a) Agriculture normally affords more food than is necessary to feed those who produce it.
- (b) The cost of production—of food as of all other commodities—will not normally be incurred unless there is an expectation of an adequate return: 'effectual demand' is thus a necessary condition of production.
- (c) Since manufactured goods need inputs from agriculture (food to sustain manufacturers), an urban manufacturing sector can provide a demand for the agricultural surplus.
- (d) In the same way, a rural agricultural sector can provide demand for a manufacturing surplus, hence the two sectors are mutually sustaining.
- (e) Labour needed in production is produced by human beings supplied with food (and manufactured necessities).
- (f) A certain per capita average of food and other necessities will keep the population and workforce stationary. At higher incomes than this, these will grow and vice versa.

Proposition (f), sometimes thought of as 'Malthusian' but actually commonplace among all eighteenth-century authors, is at the centre of Smith's theory of wages (Smith 1776 [1976]: Book I, Chapter viii, 39). Propositions (c), (d), and (f) are classically illustrated in Smith's (*ibid.*: Book III, Chapter i) 'Of the Natural Progress of Opulence', which describes and discusses the standard eighteenth-century, two-sector general equilibrium model of the interdependence of 'town' and 'country' (Waterman 2001). Proposition (b) was noted by the Physiocrats among others, and proposition (a) seems to have been taken for granted by all. Though it is evident that Paley was familiar with these ideas and indeed made them the focus of his own analysis, he gave us no help in discovering his sources.

In addition to this common core associated in particular with Mandeville, Richard Cantillon, François Quesnay, and Smith, Paley is also seemingly aware of many other elements of eighteenth-century economic thought to be found in John Locke, Hume, Berkeley, Steuart, and Josiah Tucker. Paley's treatment of money, for example, like Hey's (*ibid.*: volume III: 1,324–1,325),

seems obviously to depend on Hume, but whereas Hey acknowledged this source, Paley did not. Paley's understanding of the effects of technical progress (Paley 1785: 629–631) as also his ranking of export industries by labour intensity (ibid.: 612–614) may have come from Steuart. Yet only Berkeley's 'walls of brass, fifty cubits high' appear with attribution (as they do in Malthus). What Paley seems *not* to know about, or at any rate not to think important enough to teach his undergraduates, are: (a) price theory as found incipiently in Smith, (b) general equilibrium in competitive markets as pioneered by Pierre Le Pesant, sieur de Boisguilbert, and (c) the virtues of laissez-faire as taught by the Physiocrats. The last is in marked contrast to his somewhat older (Oxonian) contemporary, the Reverend Josiah Tucker (1713–1799), whose praise of the self-regulating market economy was later echoed by Smith.

Although Paley's lectures were delivered at the latest one year before the appearance of *The Wealth of Nations* in March 1776 (the year he left Cambridge), the occurrence of certain passages in *Principles* which read like summaries of Smith's work suggests the possibility that Paley did read it sometime between 1776 and 1785, and incorporated some of its ideas in his revision. For example, Paley's remark that population may double in 20 years (Paley ibid.: 590) resembles that in Smith (1776 [1976]: Book I, Chapter viii, 23) where the period is 25 years. His account of money, property, and power (Paley ibid.: 604) could be a digest of a similar argument in Smith. Smith's famous trio, 'the butcher, baker, brewer' crops up in Paley, as does the assumption that 'the only spring which keeps human labour in motion' is 'the exclusive right to the produce' (ibid.: 606, 602). However, any conclusion on the basis of such evidence can only be conjectural, for Paley explicitly declined to acknowledge his sources:

I have scarcely ever referred to any other book, or mentioned the name of the author whose thoughts, and sometimes, possibly, whose very expressions, I have adopted. My method of writing has constantly been this; to extract what I could from my own stores and my own reflections in the first place; to put down that; and afterwards to consult upon each subject such readings as fell in my way: which order, I am convinced, is the only one whereby any person can keep his thought from sliding into other men's trains (ibid.: xi).

As Paul Samuelson (1946: 197) said of Lord Keynes: '[H]is was one of those original minds which never accepts a thing as true and important unless he has already thought it through for himself'.

5 Economic Analysis in ‘Of Population and Provision’

The Interdependence of ‘Provisions’ and ‘Luxuries’

At the centre of Paley’s argument in ‘Of Population and Provision’ is a clearly thought-out account of the interdependence of production, employment, and population between a rural sector supplying ‘provisions’ and an urban sector (‘flourishing cities...populous towns’ (Paley 1785: 609) supplying ‘luxuries’:

It appears...that the business of one half of mankind is, to set the other half at work; that is to provide articles, which, by tempting the desires, may stimulate the industry, and call forth the activity of those, upon the exertion of whose industry, and the application of whose faculties, the production of human provision depends (ibid.).

If a stable equilibrium exists in this system of mutual causation, total population and therefore ‘collective happiness’ may be determined. We can examine this possibility by means of a formal reconstruction of Paley’s text.

Suppose an annual amount of ‘provisions’, P , is produced by the employment of A units of *agricultural* labour only. Suppose the annual food requirement of each worker is p , a constant of nature. ‘Since the soil will maintain many more than it can employ’ (ibid.: 608), A units of agricultural labour can only be employed if there is an effectual demand for provisions, P^D , which exceeds food producers’ own consumption by the amount $(P - pA)$. In a closed economy without government this can come from only two sources: the expenditure on food by workers in other sectors, and the expenditure upon food by landlords for their own and their dependents’ (servants’, retainers’, etc.) consumption.

It is therefore evident that annual production and employment in agriculture can only be sustained if non-agricultural workers and landlords receive and spend the appropriate amounts: ‘The plenty of provisions produced...affords subsistence to individuals only in proportion as it is *distributed*. Now there is but one principle of distribution that can ever become universal, namely the principle of “exchange”’ (ibid.: 604; italics in original). Hence ‘the sale of provisions depends upon the number...of those who have the fruits of some other kind of industry to tender in exchange’ (ibid.: 605–607). Following Paley, who clearly had Mandeville in mind at this point (ibid.: 596; cf. Mandeville (1732 [1988]: Book I, 107–123), we may group all non-provisions as ‘luxury’ goods.

Suppose an annual quantity of ‘luxury’ goods, Q , is produced by the employment of L units of *manufacturing* labour only. Suppose the annual

food requirement of workers in the manufacturing sector, as in agriculture, is p , and the per capita demand for 'luxury' goods in each sector is q . Quantities of P goods are measured in bushels of 'corn' per annum, and of Q goods in yards of 'cloth' per annum. Paley had no explicit theory of production and treated output as demand-determined: '*the quantity of provision... will evidently be regulated by the demand*' (Paley *ibid.*: 605; italics added). He recognised that both 'the husbandman' and 'the landowner' are 'entitled to [some share of] the produce of the soil' (*ibid.*: 609, 610), but he had no theory of rent. We must therefore suppose that rent, R (measured in 'corn'), is exogenously determined and may treat it as a parameter. Landlords might well spend some of this on the output of the 'luxury' goods sector. But to keep the model as simple as possible it will be assumed that all rents are spent on personal services, that all menial servants spend their wages on provisions only, and that Q goods are bought by workers only. The essential ingredients of Paley's two-sector model of reciprocal demand may therefore be stated as

$$P^D = pA + pL + R \quad (10.1)$$

$$Q^D = qA + qL \quad (10.2)$$

where Q^D is the quantity of 'luxury' goods produced.

It is evident that the amounts of employment in the two sectors, A and L , are the key variables. Also, it is equally evident that Q goods will only be produced if the agricultural sector provides the provisions which L workers require. Paley called agricultural labour *productive*, manufacturing labour *instrumental*, but judged 'both equally necessary, though the one have no other object than to excite the other' (*ibid.*: 609).

The question is, what determines employment in each sector? This is not a question Paley was, or could have been, equipped to answer. What follows next therefore is a 'rational reconstruction' of his text: not what Paley actually said but what he might have said had he enjoyed the advantages of reading 'economics' in present-day Cambridge. Rational reconstruction is not history: but in my opinion, which I have defended elsewhere (Waterman 2003), it can be a useful tool of history.

It is evident that for Paley, employment is an increasing function of the demand for labour, which in turn depends upon the demand for goods. However, it must also depend upon the supply of labour. Paley argued that the supply of labour is a decreasing function of the propensity of workers in each sector to desire 'luxury'. In a remarkable passage, which is almost certainly the chief source of Malthus's concept of the 'preventive check', Paley noted that 'men will not marry, to *sink* their place or condition in society'

(ibid.: 596; italics in original; cf. Malthus (1798: 64ff.). Hence an increase in ‘luxury’ which renders ‘the usual accommodations of life more expensive’ and raises the cost of ‘the established mode of living’, deters marriage and family formation so tending to reduce population and workforce (ibid.: 596).

We may therefore take q as an index of the prevailing degree of ‘luxury’ and suppose that an increase in q (other things being equal) would reduce the *supply* of labour. Since an increase in P^D and Q^D (other things being equal) would increase *demand* for labour, we may write sectoral employment as

$$A = A(P^D, q), A_p > 0, A_q < 0, \tag{10.3}$$

$$L = L(Q^D, q), L_Q > 0, L_q < 0. \tag{10.4}$$

When product demand is satisfied in each sector, we may equate P^D with P and Q^D with Q and represent Paley’s story of the interdependence of provisions and ‘luxury’ goods as

$$P = p.A(P, q) + p.L(Q, q) + R \tag{10.5}$$

$$Q = q.A(P, q) + q.L(Q, q), \tag{10.6}$$

two simultaneous equations in P and Q for given values of the constant of nature, p , the behavioural parameter, q , and the exogenous variable, R .

Let the A and L functions be represented in linear form as $A = uP + vq$ and $L = wQ + xq$, respectively, where $u > 0, v < 0, w > 0$ and $x < 0$. Let $v + x = y$, which of course is negative.

Then

$$P = \alpha + \beta Q \tag{10.7}$$

$$Q = \gamma + \delta P \tag{10.8}$$

where

$$\alpha = (pyq + R) / (1 - pu), \beta = pw / (1 - pu), \\ \gamma = yq^2 / (1 - qw), \text{ and } \delta = qu / (1 - qw)$$

The simultaneous determination at equilibrium of *provisions* and ‘luxury’ goods is shown in Figure 10.1, in which the curve labelled $P(Q)$ plots Equation (10.7) and $Q(P)$ plots Equation (10.8). The requirement that $Q(P)$ be steeper than $P(Q)$ is satisfied by the stability condition of the model (see Appendix A).

Given Paley’s social welfare function, ‘rational politics’ must seek to maximise population N , where $N = P/p$ at equilibrium. Therefore, P must be maximised given the biological food requirement, p . In terms of Figure 10.1, this amounts to action which may increase the intercept α , and/or increase the slope β , and/or increase the (negative) intercept γ , and/or increase the cotangent δ . Since p is biologically determined, we are left with R , with the two technical parameters u and w (which may be regarded as reciprocals of the marginal product of labour in each sector), and the two behavioural parameters y and q , the first of which measures the (long-run) response of population and labour supply to a rise in customary living standards, the second of which is the degree of expected and desired ‘luxury’.

It is clear from Figure 10.1 that an increase in R increases α and hence both P and Q at equilibrium. This is because the greater are rents, other things being equal, the greater the demand for provisions and hence the greater the employment of agricultural workers. Paley argued strongly for private property in land, and for social arrangements which gave incentives to landlords to farm, or at least oversee the farming of their own land, so as to maximise rent (ibid.: 601–603, 633–636).

Paley considered the effects of ‘abridgement of labour’ by ‘mechanical contrivances’ upon employment and population (ibid.: 629–631). Though the immediate effect may be technological unemployment, ‘some more general and remoter consequences’ may ‘increase the demand for work’, hence ‘the quantity of employment, upon the whole, will gain an addition’ (ibid.: 630). Technical progress in agriculture reduces u which lowers α , β , and δ , and in ‘luxury’ goods

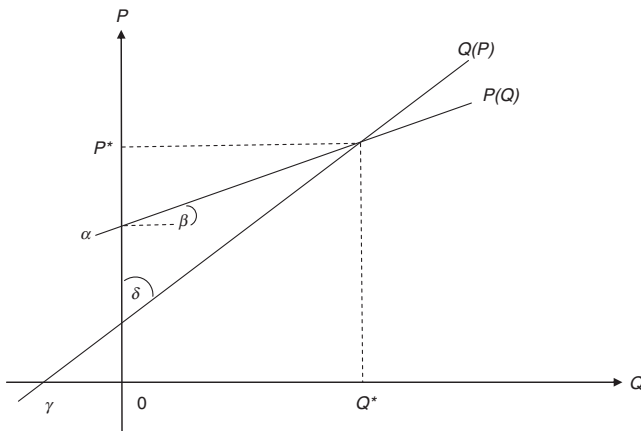


Figure 10.1 The mutual determination of provisions and ‘luxury’ goods

reduces w which lowers β , γ , and δ . Figure 10.1 therefore captures the initial, technological unemployment, but not the subsequent expansion. This is because the latter depends upon an increase in workers' incomes, so raising α , the effect of which I have not allowed for in Equations (10.7) and (10.8). For similar reasons the diagram is not able to illustrate Paley's analysis—obviously derived from Hume (1752 [1994]: 118–120)—of the effect of a 'continual increase' in the money supply, which keeps wages high while it lasts (Paley *ibid.*: 619–621).

The Generalisation of Mandeville and Optimisation

Figure 10.1 is of the most use in illustrating the effect of changes in the two behavioural parameters, y and q . When the supply of labour is independent of the degree of 'luxury', y is zero and so therefore is γ , and α is simply $R/(1 - pu)$. For given values of u , w , and α , the outputs P and Q are as large as possible. The effect of an increase in 'luxury' in these circumstances is unambiguous: δ increases and $Q(P)$ rotates clockwise about the origin, determining a higher equilibrium of P and Q . This case is pure Mandeville (1732 [1988]: Book I, 34, 35). The greater the degree of 'luxury', the greater total production, income, and population. But should Jove convert the bees from 'vice' and turn them to a virtuous frugality, their hive collapses:

As Pride and Luxury decrease...
 All Arts and Crafts neglected lie;
 Content, the Bane of Industry...
 So few in the vast Hive remain,
 The hundredth Part they can't maintain...

For as q (and δ) decline, $Q(P)$ rotates anti-clockwise until it lies along the vertical axis. National product falls to the bare minimum of a subsistence, agricultural economy: $P = \alpha = R/(1 - pu)$, and $Q = 0$.

Paley was deeply aware of the importance of 'luxury' in stimulating employment and industry, and thereby farm production and population: 'The watchmaker, while he polishes the case, or files the wheels of his machine, is contributing to the production of corn as effectually, though not so directly, as if he handled the spade or held the plough' (Paley *ibid.*: 610). He discussed the stimulating effect upon agriculture of trade with large urban centres in a passage which resembles the treatment by Smith (*ibid.*: 610–612; cf. Smith 1776 [1976]: Book III, Chapter i) and may owe something to Hume (1752 [1994]: 98–99). But as I have noted above, Paley was also aware—as Mandeville most certainly was not—that the effect of 'luxury' upon production and population

cuts two ways. On the one hand, indeed it stimulates demand, production, and population, but on the other, by tending to reduce the supply of labour, it has the opposite effect. This is apparent from Figure 10.1. An increase in q causes $Q(P)$ to rotate clockwise which by itself would increase output. However, it also causes $P(Q)$ to shift downwards and $Q(P)$ to shift to the left, which by themselves would reduce output:

It appears, then, that *luxury*, considered with a view to population, acts by two opposite effects and it seems probable that there exists a point in the scale to which luxury may ascend...beyond which the prejudicial consequences begin to preponderate. The determination of this point, though it assume the form of an arithmetical problem, depends upon circumstances too numerous, intricate and undefined, to admit of a precise solution (Paley *ibid.*: 597–598; italics in original).

Though no ‘arithmetical’ determination of optimum $q = q^*$ is available, a merely algebraic one may be had. For as Paley’s intellectual grandson Robert Malthus noted 20 years later, ‘many of the questions, both in morals and in politics, seem to be of the nature of the problems *de maximis* and *minimis* in fluxions; in which there is always a point where a certain effect is the greatest, while on either side of this point it gradually diminishes’ (Malthus 1814 [1986]: 102). By means of elementary ‘fluxions’, we may differentiate P with respect to q , and so solve for q^* from the first-order conditions for a maximum of P (Appendix B).

6 In What ‘Sense’ Was Paley ‘The First of the Cambridge Economists’?

Paley’s Putative Influence on Malthus

Keynes set forth his conjecture in his biographical essay on Malthus, written at a time when his own thinking seems to have been excited and perhaps drastically changed by his Malthusian studies (Kates 1994). Moreover, it is evident that some of Malthus’s most ‘Malthusian’ ideas are to be found in Paley’s *Principles*—although to be sure some are also to be discovered in *The Wealth of Nations*, which Malthus (alone among Cambridge men of his generation) probably knew at least as well as *Principles*.

Paley’s brief exposition (1785: 589–596) contains virtually the whole of Malthus’s *population* theory, narrowly considered, including strong hints of Malthus’s own phraseology: Nature has provided for ‘an indefinite multiplication’ of the human, as of all other species. Under favourable conditions,

human populations double in 20 (not 25) years. There is a ‘tendency’ to continual increase, but this is countered by ‘checks’ to population, provided males do not indulge in ‘irregular gratifications’. Marriage is the chief cause of population, but ‘men will not marry’ unless they can expect ‘that mode of subsistence to which each class...is accustomed’. Finally, when living standards rise and remain high for long enough, there is a ratchet effect upon the socially determined ‘subsistence’ requirement, for ‘habitual superfluities become actual wants’. Other Malthusian ideas, such as the quasi-Physiocratic emphasis on the strategic importance of agriculture (ibid.: 611–612, 633–636) are also to be found. We need not infer from any of this that Malthus was a ‘master in plagiarism’ (Marx 1954: Book I, 475, fn. 1). As we have seen from Paley’s own example, the unacknowledged use of other, well-known authors was quite acceptable in eighteenth-century Cambridge.

What is lacking in Paley, however, is Malthus’s *production* theory, which, as many have noted, is implicit in, perhaps derived from, the famous ‘ratios’ of food and population increase: specifically the much-derided ‘arithmetical’ ratio (see Waterman 1992). From that seed grew diminishing returns to labour (and capital) in agricultural production, the ‘Ricardian’ doctrine of rent, and the ‘canonical classical model’ (Samuelson 1978). Now, in all versions of ‘classical’ and ‘pre-classical’ political economy, population is constrained by the available food supply. But in the ‘canonical’ tradition, the production of food is governed by diminishing marginal product of the variable ‘labor-cum-capital’ factor applied to a given vector of lands of differing fertility. With competitive factor and product markets, profit-maximising ‘cultivators’ employ capital and labour up to the point at which (composite) marginal product equals (joint) factor payment. In stationary equilibrium, the variable factor return is that at which the growth rates of capital and labour are zero. Employment, production, and population are arrested well short of the ecological maximum, and rent is maximised for given ‘subsistence’ rates of factor payment and the state of technique. Implicit in this account is the assumption that demand for food will always be sufficient to justify cultivators in employing and producing at profit-maximising equilibrium. What this means is that *an increase of ‘luxury’*, interpreted as that component of the socially determined ‘subsistence’ wage in excess of biological requirements, *has only one effect*, which is to reduce the equilibrium levels of production and employment.

Malthus and Archbishop of Canterbury John Bird Sumner, a keen supporter of Malthus’s population theory, based their programme for the ‘improvement’ of the lower orders upon this understanding. Only the prudential check, theologically sanctioned as ‘moral restraint’, could permanently raise both real incomes and relative share of the poor. Thomas Chalmers (1808, 1832), who articulated the sectoral structure of the ‘canonical’ model

more fully than any other (see Waterman 1991), was obsessed by this point. Also, on the social importance of population control, Ricardo, James Mill, and the other ‘Philosophical Radicals’ were wholly at one with Malthus and ‘Christian Political Economy’.

For Malthus and classical economics generally, an increase in ‘luxury’ tends to *decrease* population. For Mandeville, as we have seen, it tends to *increase* population. Paley is unique in his attempt to recognise both these effects, and perhaps the first-ever economic analyst to consider the problem of optimisation.

A Proto-Keynesian Paley?

Paley could get the best of both worlds, however, because he ignored scarcity. Given q , the degree of luxury, the outputs of provisions and ‘luxury’ goods, and the levels of employment and population are all determined by the exogenous components of demand in either sector. (In my version, there is only R , but the point is general.) There is no land scarcity in Paley, no diminishing returns, no opportunity costs, no relative prices, and no problem of resource allocation. Though a market-clearing exchange rate ($r = qA/pL$) must exist between provisions and ‘luxury’ goods, it is never mentioned. There is no consideration of economic growth. None of the analytical concerns of classical political economy appear: not even those which are adumbrated in Hey’s lectures. Strictly speaking, Paley’s economic thought is not really ‘economics’ at all: not at least in any way that could interest Smith and his successors. I wish to suggest that it is in this second, *methodological* sense, as much as the first, *historical* sense, that Keynes may have regarded Paley as ‘the first of the Cambridge [or at any rate, “Keynesian”] economists’.

This is illustrated—not to say caricatured—by the model set out in Figure 10.1, which is, of course, isomorphic with Romney Robinson’s (1952) analysis of the interdependence of national incomes in a two-country, ‘Keynesian’ (i.e. underemployed, fixed price) world. Robinson’s model may well be the high-water mark of ‘crude Keynesian’ macroeconomics. All supply curves are horizontal, interest and prices are irrelevant, and the only thing that matters is aggregate demand.

Obviously, there are some fundamental differences between Paley’s and Robinson’s conceptions of the economy, leaving aside (as we may) the fact that the former deals with one country, the latter with the world. In Robinson’s model, demand determines supply in the (Marshallian) *short period* because a given population, workforce, and capital stock are massively underemployed

and all prices are stationary or sluggish. If prices adjusted rapidly, or if full employment were approached, his story would change drastically. In Paley's model, demand determines supply in the (Malthusian) *long period* because population, and fully employed workforce, is perfectly elastic at the socially determined subsistence wage.

However, what they have in common is precisely what distinguished Keynes's vision of the economy (during a period from the Michaelmas Term of 1932 to sometime after 1936) from that of all his other predecessors, including Malthus. For although Malthus insisted on the importance of 'effective demand' in his controversy with Ricardo, and attempted to make analytical use of the concept in his *Principles* (1820, 1836) [1989], and though it may well be true, as Steven Kates (1994) has persuasively argued, that Keynes actually got his central idea as a consequence of reading the Malthus–Ricardo correspondence in late 1932, the fact remains that Malthus's model of the economy—inasmuch as there is any such thing—is more than merely demand driven. From one standpoint no doubt, the whole of Chapter VII of *Principles* (Malthus *ibid.*: Book I, 345–373) may be regarded as a vast elaboration of Figure 10.1. But its exposition is fatally flawed. Land scarcity and diminishing returns are recognised, but they are not integrated with 'effective demand' into a coherent model, and at one point, Malthus actually enounces a theorem which is seriously at variance with the predictions of Paley's purely demand driven model (see Waterman 1996: 685–686). Indeed, so 'classical' does Malthus frequently appear in *Principles* that one eminent, present-day economist—presumably unaware of Paley's demand theory which lies behind portions of this work—has declared that 'Malthus is not so much an underconsumptionist as a supply-sider' (Negishi 1989: 152).

Of course, there is more to 'the economics of John Maynard Keynes' than mere 'Keynesian economics' in Samuelson's elementary textbook sense. But in so far as the latter may genuinely be discovered in some parts of *The General Theory*, its relentless concentration on the causal nature of aggregate demand has more in common with Paley's bold reductionism than with Malthus's conscientious but flawed attempt to do justice to the whole of economic reality. This may possibly have occurred to Keynes when he added the footnote on Paley to his rewritten Malthus essay in the autumn of 1932.

Appendices

Appendix A: Stability of Equilibrium

Let out-of-equilibrium quantity adjustment be specified as

$$dP/dt = h(P^D - P); \quad h > 0 \quad (10.9)$$

$$dQ/dt = j(Q^D - Q); \quad j > 0 \quad (10.10)$$

Then by substitution of the linear versions of Equations (10.1) and (10.2) for P^D and Q^D and rearrangement we obtain

$$\begin{bmatrix} dP/dt \\ dQ/dt \end{bmatrix} = \begin{bmatrix} -h(1-pu) & hpw \\ jqv & -j(1-qw) \end{bmatrix} \cdot \begin{bmatrix} P \\ Q \end{bmatrix} - \begin{bmatrix} -h(pyq + R) \\ -jyq^2 \end{bmatrix} \quad (10.11)$$

Or simply

$$d\mathbf{V}/dt = \mathbf{J} \cdot \mathbf{V} - \mathbf{C}. \quad (10.12)$$

$$\text{Now } \text{Det } \mathbf{J} = hj(1-pu-qw), \text{ and } \text{Tr } \mathbf{J} = -h(1-pu) - j(1-qw). \quad (10.13)$$

$$\text{Thus } (\text{Tr } \mathbf{J})^2 - 4(\text{Det } \mathbf{J}) = [h(1-pu) - j(1-qw)]^2 > 0.$$

Therefore the roots are real and distinct, hence the time paths of $P(t)$ and $Q(t)$ out of equilibrium will be non-oscillatory. Also, if $\text{Det } \mathbf{J} > 0$ and $\text{Tr } \mathbf{J} < 0$, the system will be stable. It can be seen from these inequalities that the necessary and sufficient condition for stability is simply that

$$(1-pu-qw) > 0. \quad (10.14)$$

The graphical requirement, in Figure 10.1, that the slope of $Q(P)$ should be steeper than that of $P(Q)$, is evidently equivalent to satisfaction of the inequality

$$(1-qw)/qv > pw/(1-pu), \quad (10.15)$$

which reduces to inequality (10.13).

Appendix B: Optimisation

At equilibrium $d\mathbf{V}/dt = 0$, hence we have the matrix equation $\mathbf{J} \cdot \mathbf{V} = \mathbf{C}$, from which we may obtain P_q by partial differentiation with respect to q and the use of Cramer's rule. The first-order condition for a maximum of P is that $P_q = 0$, from which we may solve for q^* , 'that point in the scale to which luxury may ascend...with advantage to the community, and beyond which the prejudicial consequences begin to preponderate' (Paley 1785: 597).

Since

$$P_q = (\text{Det } \mathbf{J})^{-1} \cdot \text{hj} [py + pywq + pw(uP + wQ)] \quad (10.16)$$

and since $\text{Det } \mathbf{J} > 0$ for stability, the condition $P_q = 0$ permits the solution

$$q^* = (yw)^{-1} \cdot [-y - w(uP + wQ)]. \quad (10.17)$$

Since y is negative and all other variables positive, $q^* > 0$ as $(uP + wQ)/(-y) > w^{-1}$, the latter is interpreted as the marginal and average productivity of labour in the 'luxury' goods sector. It may be seen that if the marginal responsiveness of employment to 'luxury' were very small and approached zero, q^* would approach infinity, signifying that there is no limit to the degree to which 'luxury may ascend...with advantage to the community': which is Mandeville's special case.

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11

Thomas Robert Malthus (1766–1834)

Samuel Hollander

1 Introduction

Thomas Robert Malthus was born on 14 February 1766, near Dorking, Surrey. He entered Jesus College, Cambridge, in 1784, graduating with high distinction in mathematics in 1788. William Paley, whose *The Principles of Moral and Political Philosophy* figured large in the curriculum, had been Tutor at Christ's College of William Frend (mathematician, controversial reformer, and harsh critic of the Pitt ministry), Malthus's own Tutor. William Otter, Malthus's biographer, was a fellow undergraduate. Malthus was ordained minister of the Church of England after graduation, and in 1793 was appointed curate of Okewood, Surrey, officiating there for several years; he was instituted rector of Walesby, Lincolnshire, in 1803, but paid a curate to officiate. A Fellowship at Jesus College in 1793, which provided a small income without obligation, was conditional upon his remaining single. He forfeited the Fellowship in 1804 upon marriage to Harriet Eckersall. The following year, Malthus was appointed Professor of Modern History and Political Economy in the newly founded East India College, the earliest chair of Political Economy in England. He was a founding member of the Political Economy Club and of the Royal Statistical Society. He died on 29 December 1834.

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In his chapter on Malthus in *Essays in Biography*, John Maynard Keynes focused famously on a perceived sharp divide between Malthus and Ricardo, as revealed by their correspondence: ‘In economic discussions Ricardo was the abstract and a priori [sic] theorist, Malthus the inductive and intuitive investigator who hated to stray too far from what he could test by reference to the facts and his own intuitions’ (Keynes 1933 [1972]: 95). Again, Ricardo

is investigating the theory of the *distribution* of the product in conditions of equilibrium and Malthus is concerned with what determines the *volume* of output day by day in the real world. Malthus is dealing with the monetary economy in which we happen to live; Ricardo with the abstraction of a neutral money economy (ibid.: 97; italics in original).

Indeed, ‘[w]hen one has painfully escaped from the intellectual domination of these pseudo-arithmetical doctrines [the Quantity and Purchasing Power Parity Theories], one is able, perhaps for the first time for a hundred years, to comprehend the real significance of the vaguer intuitions of Malthus’ (ibid.: 88). Keynes gave particularly high marks to Malthus’s early pamphlet *An Investigation of the Cause of the Present High Price of Provisions* (1800) as indicative of ‘a certain line of approach in handling practical economic problems...more likely to lead to right conclusions than the alternative approach of Ricardo’ (ibid.: 87). As for the *Essay on Population*, purely a priori procedure had governed the first edition, whereas in later versions ‘political philosophy gives way to political economy, general principles are overlaid by the inductive verifications of a pioneer in sociological history’ (ibid.: 84–85). Marshall had similarly contrasted the first edition, where Malthus ‘gave his argument without any detailed statement of facts’ with the second, where ‘he based himself on so wide and careful a statement of facts as to claim a place among the founders of historical economics’ (Marshall 1890 [1920]: 179, fn. 1).

Keynes’s first section in 1933 designates Malthus as ‘the first of the Cambridge economists’. It is unlikely that by this remark he intended a belief that before Malthus there had been at Cambridge no one who could fairly be designated an ‘economist’, since he surmised regarding Paley: ‘Perhaps, in a sense, *he* was the first of the Cambridge economists’ (Keynes 1933 [1972]: 79, fn. 2; italics in original. On this matter, see Waterman 1996 and this volume). But there is a more specific signification of the term sometimes encountered—that referring to the anti-Ricardian ‘Cambridge inductivists’ represented in economics by Richard Jones (Gonville and

Caius College). For Jones excluded Malthus from his indictment (Jones 1831: vi), and William Whewell (Trinity College), who took the campaign into the enemy's own territory (Whewell 1829), focused entirely on Ricardo. Keynes may have had this connection in mind when he talks of Malthus's 'vaguer intuitions', 'inductivist verifications', and 'a certain line of approach in handling practical economic problems' preferable to Ricardo's 'alternative approach'. But, as will be made clear in our discussion, to place Malthus at the head of an anti-Ricardo 'inductivist' line would be wholly unjustified. For he refused to join Jones in his campaign (see Malthus to Whewell, 28 February 1831, Additional Manuscripts c, Trinity College Library, Cambridge University: Add. Ms. c. 53²). Moreover, as early as 1803, he protested against those who declaimed against 'theory and theorists...prid[ing] themselves upon the distinction of being practical', but failing to distinguish 'partial experience which...is no foundation whatever for a just theory', from 'general experience, on which alone a just theory can be founded' (Malthus 1803: 582–583).

That the 1803 essay engages in 'inductivist verification'—illustrating from the historical and contemporary record in all parts of the world the different forms taken by 'checks' to population growth—may justify Malthus's high status as a pioneer historical economist, but in no way does this detract from his contribution to deductive theory, the dimension that will primarily engage us in this chapter. Now Keynes, in fact, himself observed in his 1933 biography that Malthus learned as an undergraduate 'the a priori methods of Cambridge—whether Paley, the Mathematical Tripos, or the Unitarians' (Keynes 1933 [1972]: 86; see also Keynes 1935 [1972]: 107 on Paley and a priori method). Moreover, a Cambridge Centenary allocution in 1935 dilutes the notion of a sharp Ricardo–Malthus methodological divide by representing Malthus as 'a great pioneer of the application of a frame of *formal thinking* to the complex confusion of the world of daily events', who sought 'to penetrate these events...by a mixture of intuitive selection and *formal principle* and thus to interpret the problem and propose the remedy' (ibid.; italics added). We have been recently reminded of 'Malthus's quest for a middle way between opposed doctrines, his search for the golden mean, his desire for a balance between abstract theory and experience [which] derived from the intellectual legacy of the eighteenth-century Anglican *via media* whose stronghold was Cambridge' (Cremaschi 2014: 11)—although it is unlikely that this is what Keynes intended by his designation of Malthus as 'the first of the Cambridge economists'.

Whatever methodological debt Malthus may have owed to Cambridge—a priori procedure or a ‘balanced’ perspective or both—my main concern will be to establish his bona fides as a leading theorist. I proceed by reference first to his land-based growth model—I use the term ‘model’ advisedly—yielding the canonical classical inverse wage–profit relation and downward paths of the wage and profit rates until the stationary state; and to his representation of the profit rate as a physical ratio of corn surplus to corn input, transposed to the industrial sector via variation in the terms of trade between food and manufactures, a representation famously ascribed by Piero Sraffa to the early Ricardo. The use to which the primary growth model is put in application will be a major concern, with particular reference to the charge that Malthus maintained the inevitability of low living standards. The historiographical implications of our reading will also engage us.

Secondly, I shall take up Malthus’s perception of an aggregate-demand problem, independent of the scarce land constraint, both in the secular and short-term contexts—strikingly unorthodox in the former case. Here arises the question of a Malthus–Keynes nexus. Malthus was certainly preoccupied by ‘the facts’ of the real world, as Keynes insisted. He did not, however, abandon theoretical endeavour because of his dissatisfaction on factual grounds with the orthodox model, but sought rather to devise an alternative better able to account for the facts, believing—prematurely it turns out—that his theory of sustainable growth satisfied the requirement. The same may be said of his analysis of the post-1814 depression. The precise character of Malthus’s ‘inductive’ procedures in support of the theoretical models in both these cases will be briefly addressed.

A third general theoretical issue is Malthus’s ‘Physiocratic’ perspective on the source of surplus in agriculture—a central feature of his work, albeit inconsistent with canonical growth doctrine. I shall demonstrate the late abandonment of this and related theoretical elements, in response to major empirical transformations of the day, and also of the case for agricultural protectionism based upon them.

Drawing upon the foregoing discussions, I attend finally to Malthus’s methodology of applied economics, characterized by great caution in addressing policy issues. Here, factual circumstance relating to time and place is of the essence. So too is the problem of ‘proportions’ or defining optima in addressing policy, an issue raising a further possible dimension to the Cambridge connection considering its centrality in Paley’s *Moral and Political Philosophy* (see Cremaschi 2014: 29). Our coverage seeks to capture the main characteristics of Malthus’s contribution to economics, both theoretical and applied.

2 Growth Theory

The ‘principle of population’ of the first *Essay* entailing ‘arithmetical’ and ‘geometrical ratios’ attributed respectively to food and population increase, and necessitating ‘checks’ to the latter, derived—as, Malthus himself later informs us—from Hume, Wallace, Smith, and Price (Malthus 1803: iii). In the light of our concern with possible Cambridge connections, it is significant that Malthus referred *critically* in 1803 to Paley’s chapter on population in *Moral and Political Philosophy* (1788), and found the matter of ‘checks’ treated (albeit inconsistently) only in the later *Natural Theology* (1802). These facts should be borne in mind when evaluating Keynes’s contention that Paley’s *Principles* ‘must be placed high...amongst the intellectual influences on the author of the *Essay on Population*’ (Keynes 1933 [1972]: 79).

The ratios themselves in Malthus’s exposition lack an analytical foundation, entailing the superposition of purported data derived from completely different environments. Nonetheless, the principle of diminishing returns does make an appearance even in the first edition, providing the foundation of a growth model involving a declining path of real average wages in consequence of a decelerating rate of growth of labour demand—what has come to be labelled the ‘canonical’ classical growth model. According to the argument, as population density rises, *per capita* output and the real wage fall, ultimately leading to a stationary population (Malthus 1798: Chapters IV, VI, VII, XVII). This analysis neglects the profit rate, particularly its downward path along with the real wage, a prime feature of the full model. The fifth (1817) edition of the *Essay* repairs this deficiency by an impressive formulation of the process of expansion towards the ‘practical limit’ (the theoretical stationary state) entailing deceleration of both capital and population growth rates, in consequence of the increasing paucity of ‘cultivable land’, and declining returns to labour and capital until their respective minima (*ibid.*: volume II: 433–435). Malthus also elaborated the conditions, in addition to unchanged technology, that must be satisfied for a ‘regular’ deceleration of population growth and of the wage and profit rates, for a steady pattern was not to be expected in practice.

Malthus’s *Principles of Political Economy* provides an especially elegant account of the fully fledged canonical growth model, which I believe is the best by far in the entire classical literature. Here, Malthus demonstrates that to assume a constant real wage in the course of growth not only is ‘contrary to the actual state of things’ (Malthus 1820: 297, 1836: 272) but also entails a logical ‘contradiction’:

[I]f from the first [the labourer] had no more than sufficient to keep up the actual population, the labouring classes could not increase; nor would there be any occasion for the progressive cultivation of poorer land. On the other hand, if the real [1836: corn] wages of labour were such as to admit of and encourage an [1836: a considerable] increase of population, and yet were always to remain the same, it would involve the contradiction of a continued increase of population [1836: at the same rate] after the accumulation of capital, and the means of supporting such an increase had entirely ceased (*ibid.*).

A further passage explains in crystal-clear terms that the incidence of diminishing returns cannot fall solely on labour since the decline in the real (corn) wage, because of its effect on the population growth rate, is constrained relative to the fall in the marginal product. The outcome of increasing land scarcity is accordingly to raise the share in the produce paid to labour at both the extensive and intensive (zero-rent) margins, thereby depressing the profit rate. Thus, the returns to labour and to capital decline simultaneously to their respective minima at which stage further expansion of population and capital ceases (*ibid.*: 298–299, 273–274). Every feature of the classical growth model is substantiated, including the most basic of all—the functional relation between the profit rate and the *proportionate* wage such that a fall in the profit rate is consistent with a fall in the real wage during the course of growth, subject to declining agricultural productivity.

I note here that Malthus responds in the typical terms of a theorist to Richard Jones's so-called inductivist objections to the land-based growth model. 'I am not sure', he wrote to Whewell in his letter of 28 February 1831 cited above,

whether [Jones] has not gone beyond the truth in his unwillingness to admit the *tendency* of continued accumulation, and of the progress of population and cultivation to lower the rates of profits and corn wages on the land ... If the progress of cultivation and population has no *tendency* to diminish corn wages, I do not see what cause should ever retard the rate at which population is known to increase in the new colonies (*italics in original*).

The explanation for the falling profit rate outlined above runs in aggregative terms. Malthus should also be recognized as probably the first serious proponent of a dual-sector model wherein the profit rate is determined within agriculture and then applied to industry by way of variation in the terms of trade between food and manufactures (Malthus 1820: 300). Values in the sense of *exchange rates* only enter the picture in transferring from the agricultural rate

to the general rate, and not in the determination of the agricultural rate itself. The 1836 version of the *Principles* confirms that there are two forces at play in profit-rate determination: ‘In the cultivation of land, the cause of the *necessary* diminution of profits is the diminution in the quantity of produce obtained by the same quantity of labour’—a physical quantum; in ‘manufactures and commerce it is the fall in the exchangeable value of the same amount of [manufactured] produce’ (Malthus 1836: 275; italics in original). For,

corn, on account of its being the main support of the labourer, is the only object in the production of which a comparison may be instituted between the quantity advanced and the quantity produced ... In manufacturing and mercantile employments, there is no approach towards a possibility of comparing the advances with the products in regard to quantity (ibid.: 265–266).

It is the homogeneity of input and output in agriculture that holds the key. To justify the expression of the agricultural profit rate in physical terms, a particular assumption is made to preclude short-term or seasonal price fluctuations or the exceptional closing of markets that create situations where ‘a part of the crop might be of no value [at all]’ (Malthus 1820: 296, fn., 1836: 272, fn.). The disturbance to a physical or corn-profit calculation by sudden corn-price movements is however irrelevant in the growth context. Also in that context, Malthus, in the *Inquiry into the Nature and Progress of Rent* (Malthus 1815a), applied a sort of Law of Markets to corn whereby the physical surplus—‘the excess of their quantity above the quantity necessary to maintain the labour required to produce them’—guarantees a value surplus by generating, via population growth, the requisite demand for food; in other cases, value reflects scarcity or ‘the smallness of the supply compared with the demand’ (ibid.: 12). The *Principles* too reiterates that only specialty products—where demand is ‘exterior to, and independent of, the production itself’—are treatable in *scarcity* rather than *surplus* terms (Malthus 1820: 146–147, 1836: 145–146).

Malthus laboured under the misconception that Ricardo failed to apply demand–supply analysis to long-run cost pricing, allowing solely for supply conditions. Certainly, Malthus insisted that ‘cost of production itself only influences the prices of commodities as the payment of this cost is the necessary condition of their continued supply [1836: in proportion to the extent of the effectual demand for them]’ (ibid.: 1820: 74–75, 1836: 71), a much admired formulation often said to presage Jevonian doctrine. I perceive here more of a Marshallian flavour, but in either event there is no justification for regarding Malthus as a ‘subjective value’ theorist in any sense that would not apply

to Ricardo, who argued only for the primacy of supply and protested against Malthus's refusal to treat the long-run pricing of corn in demand–supply terms (Ricardo 1951, volume I: 405–406).

In so far as science is a social enterprise, it is sometimes said that should contemporaries be unfamiliar with a contribution it is as if that contribution had never been made as far as concerns the scientific endeavour. To take this line, I suggest, would be to ignore information pertinent to the *origins* of scientific ideas and for the *progress* of science. For even when a contribution goes unnoticed, we learn from it—when it finally sees the light of day—what technically could be achieved within the early intellectual and empirical environment, and also which causal conditions may be excluded as required for similar notions emerging independently at a later stage. Much the same may be said of misinterpretation by contemporaries.

The Malthus case illustrates these propositions. Most contemporaries—Ricardo is an exception—seem to have been unaware of Malthus's outstanding contribution to growth theory. Judging from the textbooks the neglect continues to this day. Had Malthus's account been properly addressed, we might have been spared decades of fixed-wage theorising taught to thousands as a logical structure.

Taking proper account of Malthus's contribution also undermines much of the Sraffian version of the historiography of economics. In his early Cambridge lectures on advanced value theory (1928–1931), Sraffa attributes embryonic features of a corn model entailing calculation of surplus in physical terms to William Petty and François Quesnay. Subsequently, he famously attributed the construct—though as a 'rational reconstruction'—to a brief statement in 1814 by Ricardo that 'it is the profits of the farmer that regulate the profits of all other trades' (Sraffa 1951, volume I: xxxi). Malthus is mentioned but only to emphasize his opposition to the formulation of 1814. His elaboration of just such a model in his later texts goes wholly unnoticed.

I turn to the common charge against Malthus of trumpeting the inevitability of low living standards. This misreading characterized Marx and all manner of utopian reformers. In fact, Malthus, in common with other classical thinkers including Ricardo, used the growth model not as a predictive device but as an engine of analysis to demonstrate how, by the practice of population control, a low-wage outcome *might be avoided*. For while increasing land scarcity dictated a deceleration in the rate of population growth, 'a gradual change in the habits of the labouring classes would...effect the necessary retardation in the rate of increase', so that 'in the progress of cultivation and wealth the condition of the lower classes of society would be in a state of constant improvement' (Malthus 1817, volume III: 420–421).

Prudential population control is treated by Malthus not merely as an exogenous matter, the outcome in particular of appropriate education, but as endogenous to the growth process itself. Already in the 1803 *Essay* there is mention of the possibility that rising wages further stimulate such control by generating more sophisticated tastes (Malthus 1803: 246). The 1817 version adds the analytical insight that the rising corn-manufactures price ratio during the growth process alters taste patterns in favour of manufactured wage goods, thereby contributing to a deceleration of the population growth rate (Malthus 1817, volume III: 23–24).

Mill therefore rightly posited Malthus as one of the great reformers (Mill 1848 [1965]: 753). Bonar took this same line in his delightful allocution at the Cambridge Commemoration in 1935 (Bonar 1935: 221–225). All the more heroic is Malthus when we recall his status as a Church of England clergyman and the risk to his reputation he was prepared to take by championing ‘prudential’ restraint should ‘moral’ restraint prove too much to ask. Charles Fay in his allocution touches effectively and amusingly on this feature (Fay 1935: 228–229). Malthus’s courage is no less apparent when in 1815 he opted for agricultural protectionism, notwithstanding the damage to his standing with the progressive journals. His abandonment of this position, it must be allowed, was made with little fanfare.

A word more on Malthus as reformer in the light of his harshly expressed objections to in-house poor relief that have stamped him with gross illiberality. His position, I suggest, can be more accurately portrayed by supposing the general practice of prudential restraint, such that ‘low’ family size is the rule. With the removal of population pressure, the practical approach to policy is transformed. A case is even made out as early as the second *Essay* for family allowances to help those who had more children than they could be expected to foresee (Malthus 1803: 595).

Marx’s strictures apply to Paley rather than to Malthus. In his *Moral Philosophy*, Paley had argued ‘that the condition most favourable to the population of a country, and, at the same time, to its general happiness, is, “that of a laborious frugal people ministering to the demands of an opulent luxurious nation”’ (Paley quoted in *ibid.*: 592). Moreover, in *Natural Theology* Paley deduced from the principle of population—citing Malthus on the arithmetic and geometric ratios—the impossibility ‘to people a country with inhabitants who shall all be in easy circumstances’ leading him to justify ‘the distinctions of civil life’ (Paley 1802: 340–341). This sort of deduction Malthus could not fathom, considering Paley’s concession in 1802 that ‘mankind will in every country breed up to a certain point of distress’ (*ibid.*: 539), for in that case ‘that country will evidently be the happiest, where the degree of distress at this

point is the least, and consequently, if the spread of luxury, by producing the check sooner, tend to diminish this degree of distress, it is certainly desirable' (Paley quoted in Malthus 1803: 592, fn.).

It may be noted that as early as 1796—in a non-extant manuscript (*The Crisis*)—Malthus had apparently objected on theoretical grounds, with evident Smithian pedigree, to Paley's proposition whereby 'the quantity of happiness in any country is best measured by the number of people'; for Malthus, '[i]ncreasing population is the most certain possible sign of the happiness and prosperity of a state; but the actual population may be only a sign of the happiness that is past' (Malthus quoted by Empson 1837: 482). Malthus is also reported as later regarding Paley and Pitt as 'the two converts of whom he was most proud' (ibid.: 483).

3 Aggregate Demand and the Malthus–Keynes Relation

The determination of the profit rate in terms of the land-based growth model, or its two-sector refinement, relates to the *maximum possible return* (Malthus 1820: 300, 1836: 275). The return on capital might, however, be lower. The relevant chapter, 'On the Progress of Wealth', analyses the problem of secular stagnation notwithstanding high growth potential, or the absence of a land-scarcity constraint (ibid.: 1820: 345, 1836: 309). I set aside issues peculiar to economic development strictly defined and focus on a perceived problem of aggregate-demand deficiency disallowing sustainable output expansion, and depressing the rate of return, arising from a process of 'saving'—translated into real capital accumulation and resulting in expanded capacity—financed by reduced consumption. Here lies the prime theoretical difference with Ricardo, who relied on the Law of Markets to assure expansion of markets to absorb increased output emanating from a larger capacity.

Malthus, however, characterized the war period 1793–1814 as one of unparalleled *sustainable* growth because the creation of new capacity was in response to and financed out of initially increased profits reflecting expanded markets rather than out of reduced consumption (ibid.: 1820: 459, 1836: 395). The prosperity is also attributed to war finance via government borrowing and 'unproductive' expenditure which raised the profit rate and stimulated private accumulation (ibid.: 1820: 373–374, 1836: 329–330).

Malthus's technical argument relating to unsustainable growth is problematic. Consider the proposition that a successful saving (investment) process requires initially raising the exchangeable value of aggregate output by way of demand emanating from *outside* the productive sector, or by 'the maintenance of an adequate proportion of unproductive consumers [1836: consumers not directly productive of material objects]' (ibid.: 1820: 463, 1836: 398). The problem is that a source of demand external to the productive sector was not in fact posited, as is implied here, as a *necessary* condition for sustainable growth:

[W]ithout supposing the productive classes to consume much more than they are found to do by experience, particularly when they are rapidly saving from revenue to add to their capitals it is absolutely necessary [1836: it is necessary] that a country with great powers of production should possess a body of unproductive consumers [1836: consumers who are not themselves engaged in production].

Indeed, Malthus is explicit in both editions that 'there can be no occasion for unproductive consumers, if a consumption sufficient to keep up the value of the produce takes place among those who are engaged in production', and that 'there might be little occasion for unproductive consumers' were capitalists to restrict their savings to 'what could be beneficially added to their capitals' (ibid.: 1820: 464–465, 1836: 399–400). The empirical norm, however, was an *excessive* response to profit opportunities, reflecting a Marx-like passion for saving: 'Almost all merchants and manufacturers save, in prosperous times, much more rapidly than it would be possible for the national capital to increase so as to keep up the value of the produce' (ibid.: 1820: 465, 1836: 400). At the close of his book, Malthus again insists on 'a greater tendency to parsimony than is consistent with the most effective encouragement to the growth of public wealth' (ibid.: 1820: 517, 1836: 434). Malthus, we may conclude, does not propose a 'general theory' of unsustainable growth reflecting aggregate-demand deficiency.

Notwithstanding his reputation as a pioneer of historical economics, Malthus offered only two instances of the alleged excess, taking for granted that if land scarcity can be ruled out there remained only his own explanatory candidate for any observed downward trend of interest and profit rates. The first is the British case after the mid-eighteenth century:

[T]hat this fall in the rate of interest and profits arose from a redundancy of capital and a want of demand for produce, rather than from the difficulty of production on the land, is fully evinced by the low price of corn at the time, and the very different state of interest and profits which has occurred since (*ibid.*: 1820: 475–456, 1836: 405–456).

Late seventeenth-century Italy provides a second instance of the purported phenomenon. These casual appeals to the historical record to support a theoretical argument scarcely justify Keynes's attribution to Malthus of a sustained concern with 'inductivist verification'. We recall also the purely 'theoretical' response to Jones in justifying the land-based growth model. Malthus does not, at least in these key instances, live up to his own insistence that 'general experience' alone can provide the foundation of 'a just theory'.

The perceived problem of an excessive rate of saving (investment) arises only when profits are initially high; there would be no motive to expand capacity during periods of low profits. Accordingly, Malthus did not analyse the post-war years of depression in terms of his notion of unsustainable growth; rather, those years are characterized in his account by saving unaccompanied by corresponding investment. The initiation of the depression is found in a sudden inflow of foreign corn generating falling farm incomes and consequently reduced expenditures on manufactured products; monetary contraction was a contributing feature (Malthus 1820: 493–494, 1836: 416–417). With Ricardo in mind, Malthus asked: '[W]here are the understocked employments...fully capable of absorbing all the redundant capital which is confessedly glutting the markets of Europe in so many branches of trade? It is well known by the owners of floating capital that none such are now to be found' (*ibid.*: 1820: 499, 1836: 420). Malthus also refers to capital flight abroad,

when profits are low and uncertain, when capitalists are quite at a loss where they can safely employ their capital, and when on these accounts capital is flowing out of the country...when all the evidence which the nature of the subject admits...distinctly proves that there is no effective demand for capital at home (*ibid.*: 1820: 495, 1836: 418).

The weak incentive to invest is also attributed to a lag of wages behind the fall in prices (*ibid.*: 1820: 446–447, 1836: 387). But wage adjustment did not provide the solution, for the stagnation continued even after labour had 'adjusted itself to the new level of prices' (*ibid.*: 1820: 446, 1836: 387). For

though labour is cheap, there is neither the power nor the will to employ it all; because not only is the capital of the country diminished, compared with the number of the labourers, but, owing to the diminished revenues of the country, the commodities which those labourers would produce are not in such request as to ensure tolerable profits to the reduced capital (*ibid.*: 1820: 495, 1836: 417).

One passage concerns the sudden reduction at war's end of government spending accompanied by lower taxation and 'saving' from increased disposable incomes:

The returned taxes, and the excess of individual gains above expenditure, which were so largely used as revenue during the war, are now in part, and probably in no inconsiderable part, saved. I cannot doubt, for instance, that in our own country very many persons have taken the opportunity of saving a part of their returned property tax (*ibid.*: 1820: 499, 1836: 420, Hollander 1997: 609).

Since, in the prevailing circumstances, attempts to add to capacity were repeatedly ruled out, it would seem fair to understand 'saving' in this context as hoarding or a demand for money to hold, or other forms of leakages. Somewhat more tricky is a reference to 'that disturbance in the balance of production and consumption, which has been occasioned by the sudden conversion of soldiers, sailors, and various other classes which the war employed, into productive labourers' (Malthus 1820: 512, 1836: 430) which might be said to imply the employment of the demobilized servicemen in actual investment programmes that turned out to be unsustainable. But again, Malthus's account of post-1814 Britain points away from this reading.

Can we then conclude—as did Jacob Viner—by saying that 'Malthus attributed the depression, apparently, to an increase in saving unaccompanied by a corresponding increase in investment, and thought that, given the absence of sufficient incentive for investment under prevailing conditions, the remedy was to be found in increased private expenditure on consumption' (Viner 1937: 194, fn. 27)? The answer would be clearly affirmative were it not for one severe complexity. Although Malthus recognized leakages from the expenditure stream in the post-war years, when refuting those who appealed for increased 'saving' as the solution to the problem of unemployment he has in mind not hoarding but real investment or additions to capacity. We must carefully note that nothing is said of investment as implying a net increase in expenditure; the objection to the proposal turns rather on the increased flow of output emerging from the expanded capacity corresponding to increased 'saving':

[S]aving is the means of furnishing an increased supply for the increasing national wants. If however commodities are already so plentiful that an adequate portion of them is not consumed [1836: profitably consumed], the capital so saved, the office of which is still further to increase the plenty of commodities, and still further to lower already low profits, can be comparatively of little use [1836: to save capital can only be still further to increase the plenty of commodities, and still further to lower already low profits, which can be comparatively of little use] (Malthus 1820: 467, 1836: 401).

We thus find intruding the concept of unsustainable expansion which is strictly pertinent to growth economics, or the ‘long run’. Furthermore, it is evidently assumed that the finance of capital expansion derives from a reduction in consumption spending rather than from hoarded funds, since otherwise increased current expenditure could not possibly be neglected; indeed, the point in question is precisely that a reduction in consumption outlays to finance increased capacity as a solution to depression must be damaging.

The foregoing orientation is amply confirmed by Malthus’s counter proposal for tax-financed public works as the most immediate step available (ibid.: 1820: 511, 1836: 429). For while increased government outlays would admittedly be balanced by reduced private outlays precluding any net increase in expenditure—the ‘Treasury View’ as it later came to be termed in Britain—a stimulus is yet perceived to be generated by the transfer from ‘productive’ to ‘unproductive’ expenditure:

[I]n our endeavours to assist the working classes in a period like the present, it is desirable to employ them in unproductive labour, or at least in labour [1836: those kinds of labour] the results of which do not come for sale into the market, such as roads and public works. The objection to employing a large sum in this way, raised by taxes, would not be its tendency to diminish the capital employed in productive labour; because this, to a certain extent, is exactly what is wanted (ibid.: 1820: 511, 1836: 429).

The significance of all this becomes apparent when we revert to the Malthus–Keynes relation. That Keynes held Malthus in the highest regard is not in any doubt. But admiration falls short of affirming positive ‘influence’. For he was not uncritical in the ‘Robert Malthus’ essay of the analysis of the ‘balance of Saving and Investment’ (Keynes 1933 [1972]: 102). Moreover, in *The General Theory*, while commending Malthus for assuring ‘the notion of the insufficiency of effective demand...a definite place as a scientific explanation of unemployment’ (Keynes 1936 [1973]: 362), he expressed regret that Malthus had failed to provide an adequate ‘alternative construction’ to

Ricardo's since he was 'unable to explain clearly (apart from an appeal to the facts of common observation) how and why effective demand could be deficient or excessive' (ibid.: 32). For this there is considerable justification since the technical case for 'unproductive consumption' in the growth context is not adequately supported—recall that the requirement turns on an *empirical* estimate of excessive responses to high profits.

Recognition by Malthus of excess demand for money to hold in his portrayal of the post-Napoleonic depression might certainly be said to have a 'Keynesian' flavour. But there are two important riders. First, allowance for excess demand for money is scarcely unique to Malthus, as Mill's famous 'Of the Influence of Consumption on Production' testifies. Malthus himself pointed out to Ricardo, correctly, that Say, no less, acknowledged the phenomenon (in Ricardo 1951-1973, volume VIII: 260). But secondly, and more important, we recall that when objecting to proposals for increased 'saving' to ameliorate the depression, Malthus—identifying 'saving' with a real 'investment' programme financed by reduced consumption expenditure—focused entirely on the damaging impact on activity of an expanded flow of output emanating from a larger capacity in the face of reduced consumption. This is far from the approach in *The General Theory*, where output flows deriving from investment expenditure are, of course, irrelevant in the 'short run'.

The difference between the Malthus's and Keynes's worlds is particularly striking when it comes to public works proposals. Malthus's argument for mitigating a depression by tax-financed public works expressly adopts the Treasury View, relying on a stimulus provided by the switch from 'productive' to 'unproductive' (in this case, government) spending. By contrast, Keynes's analysis of the multiplier effects of public works assumes there to be no contemporaneous reduction in either consumption or investment (Keynes 1936 [1973]: 116–117, 119).

Conspicuous in Malthus's account of the depression is the absence of automatic adjustment mechanisms assuring recovery; the alleviation of the depression is attributed to 'the happy opening of new and large channels of exportation' reflecting, in part, trade liberalisation (Malthus 1824: 324). Reliance on external stimuli suggests a Keynes-like 'unemployment equilibrium' in their absence. Furthermore, as with Keynes, downward wage flexibility was perceived as a necessary but insufficient condition for recovery. However, on only one occasion, to my knowledge, did Malthus caution that reduced wages would have a tendency to diminish aggregate demand (see Malthus 1827). Moreover, even here there is no suggestion that a nominal wage reduction cannot assure a real-wage reduction, a conspicuous feature of Keynes's analysis.

Taking a broader perspective, there is surprisingly extensive common ground between Malthus and orthodox economists regarding stabilisation policy. Thus, Malthus rejected monetary injections as a solution to the post-1814 depression (Malthus 1820: 514–515, 1836; 431–432). He adhered to standard opinion regarding the Gold Standard, and in one respect he was more conservative than Ricardo, who justified permanent devaluation of the pound should that be called for to avoid severe deflation. Malthus's fiscal conservatism also emerges in the rejection of debt financing in the post-war period on the supply-side grounds that the future burden of taxation required to pay off the National Debt would hamper production. Particularly striking is his objection to interference with the rate of saving, notwithstanding his preoccupation with its 'caprices', particularly a tendency to 'excessive' saving in response to a high return on capital mitigating against sustainable growth. That there was no Malthusian 'revolution' is understandable not only because of the deficiencies in the argument regarding the necessity for 'unproductive consumption' but also because novel policy implications were absent to resolve the perceived problem.

However, this conclusion must be qualified. It would be easy to get the picture out of focus if we dismiss Malthus's practice of putting to the test policy proposals according to their impact on macro-distribution and consequently on the volume of final demand. The establishment of a research programme of this order—to be illustrated presently by Malthus's caution regarding National Debt repayment and, more generally, preoccupation with the difficulties of correcting faulty policy—goes far to justify Keynes's qualified enthusiasm.

There remains a complexity in the record that we can only mention briefly here. Keynes was much impressed by Malthus's account of a protracted post-war depression that, Malthus charged, Ricardo failed even to acknowledge. But a recent study (Davis 2005) demonstrates that Ricardo's intimate knowledge of contemporary monetary and financial circumstances convinced him that there was no unmitigated depression requiring explanation but rather a series of cyclical fluctuations. If this is so, Ricardo's macromodel incorporating the Law of Markets by no means precluded a proper diagnosis of the post-Napoleonic years.

4 Malthusian 'Physiocracy' and Its Abandonment

Pervading much of Malthus's discourse is an agricultural bias reflected in the notion of a 'surplus' unique to agriculture, a primary feature of canonical Physiocracy. (Paley too expressed 'physiocratic' bias.) In the second *Essay on*

Population, for example, manufacturing income is treated simply as a transfer payment made from income generated on the land:

But manufactures, strictly speaking, are no new production, no new creation, but merely a modification of an old one, and when sold must be paid for out of a revenue already in existence, and consequently the gain of the seller is the loss of the buyer. A revenue is transferred, but not created (Malthus 1803: 433).

Even so, Malthus allows a qualification when confuting the Single Tax doctrine: ‘Admitting, as I shall be disposed to do, that the surplus produce of the land is the fund which pays everything besides the food of the cultivators; yet it seems to be a mistake to suppose that the owners of land are the sole proprietors of this surplus produce’ (ibid.: 440, fn.). In 1803, and in later editions of the *Essay*, England is represented as the ‘richest’ European county given the magnitude of its surplus agricultural produce; the notion of *net* national revenue generated solely in agriculture remains implicit in a caution against industry-based development dependent on food imports; and the value of corn exports is said to constitute in its entirety a net addition to national income, whereas from the value of manufacturing exports must be deducted subsistence costs to obtain ‘a clear national profit’ (ibid.: 449).

Regarding Malthus’s famous agricultural protectionism in *The Grounds of an Opinion* (1815b), Ricardo complained primarily of its rationalisation in terms of ‘the benefits of agriculture over commerce in increasing production’, on the grounds that it clashed with both the scarcity and related diminishing-returns perspectives adopted by Malthus elsewhere (Ricardo to Malthus, 13 February 1815, in Ricardo 1951-1973, volume VI: 178). There is considerable justification for Ricardo’s charge. For the loss of agricultural capital and output is said in the *Grounds* to be, pound for pound, more damaging than a loss in other sectors, a reflection of the ‘additional value’ peculiar to the agricultural sector (Malthus 1815b: 30–36). This additional value would be erased by a free corn trade—Malthus here recommends his *Rent* pamphlet—to the national detriment from the perspective of both consumption power and tax revenue: ‘And this additional value is not a mere benefit to a particular individual, or set of individuals, but affords the most steady home demand for the manufactures of the country, the most effective fund for its financial support, and the largest disposable force for its army and navy’ (ibid.: 35). Malthus in this context allowed that ‘the last additions to the agricultural produce in an improving country are not attended with a large proportion of rent’, yet concluded that ‘in all cases the importation of foreign corn must fail to answer nationally, if it is not so much cheaper than the corn that can be

grown at home, as to equal both the profits and the rent of the grain which it displaces' (ibid.: 35–36). In order to justify free trade in corn, the savings in resources must be sufficient to compensate for the loss of rent.

Malthus conceded in the 1817 *Essay on Population* that with a free corn trade the absolute size and rate of growth of population would be greater than with a primary reliance on domestic agriculture, but maintained that the cost would be too high, namely

a greater degree of uncertainty in its supplies of corn, greater fluctuations in the wages of labour, greater unhealthiness and immorality owing to a larger proportion of the population being employed in manufactories, and a greater chance of long and depressing retrograde movements occasioned by the natural progress of those countries from which corn had been imported (Malthus 1817, volume II: 495–496).

For all that, the physiocratic dimension was by 1817 already under threat and with it the protectionist implication. Thus, we find a new emphasis on industry-based growth as characteristic of the later stages of economic development, a tendency reflecting growing land scarcity and an 'increasing taste for conveniences and luxuries' which tended to 'direct the greatest part of new capital to commerce and manufactures' (Malthus 1817, volume III: 6). Also, the *Principles* allows that with prudential population control a *major* part of the national surplus (whatever its ultimate source) takes the form of wages, even the wages of industrial and service labour:

Those who live upon the wages of labour, unproductive as well as productive [1836: including of course those engaged in personal services,] receive and expend much the greater part of the annual produce, pay a very considerable sum in taxes for the maintenance of the government, and form by far the largest portion of its physical force (1820: 423, 1836: 368).

This allowance for improved working-class living conditions (notwithstanding a natural tendency to industry-based growth) and expenditure out of wages undermines the case for agricultural protection to assure balanced growth on the grounds that landlord's rent constituted a prime source of demand.

All this implies a case for a free corn trade. Indeed, in the mid-1820s Malthus gave his support to the government's new free-trade policy as consistent with a 'naturally' growing sectoral imbalance favouring manufactures. Thus, a note in the 1826 edition of the *Essay* declares that, notwithstanding the greater stability of prices under protection, Malthus was on balance opposed to the

policy because of its ‘unsocial tendency’—the damage caused to the ‘interests of the commercial world in general’ (Malthus 1826, volume II: 209, fn.). The government was ‘most laudably setting an example of a more liberal system of commercial policy’, and foreign nations should not be allowed ‘so marked an exception as our present corn-laws to cast in our teeth’ (ibid.). Malthus does not here simply affirm that, from a cosmopolitan viewpoint, free trade in corn was desirable, but proposes that, given the new British commercial policy, global free trade involving the full range of products was no longer a pipe dream—indeed, liberalisation of corn imports would provide a potent weapon for its achievement.

In his 1824 *Quarterly Review* article on Ricardian economics, Malthus had already allowed that expansion of foreign markets for British manufactures had mitigated the worst effects of the post-war depression by drawing labour from the agricultural sector, such expansion in part ascribed to the ‘improved views of our government in commercial legislation’ (Malthus 1824: 324). The empirical significance of *manufactures* from the perspective of aggregate activity reinforces the case for free trade since expanded markets could scarcely be relied upon with agriculture ‘so marked an exception’ to a general free-trade programme. The protectionist case of 1815 had turned partly on the unrealistically large expansions of manufacturing required to absorb even the agricultural workers who were unemployed in 1815. By the mid-1820s, the manufacturing sector was proving itself adequate in that respect.

We now recall Malthus’s original affirmation of 1815 in *Grounds of an Opinion* that, pound for pound, the loss of agricultural capital and output is more damaging than a similar loss in other sectors from the perspective of both consumption power and tax revenue. The emphasis in 1824 on expenditure derived from export sales and the rejection of pro-agricultural intervention implies a weakening of the case for ‘unproductive’ consumption by landlords and their dependents.

Malthus’s renunciation of support for balanced growth and self-sufficiency in food in favour of industry-based growth amounts to the Ricardian position precisely. This reconciliation with Ricardo establishes the validity of perceiving a well-defined ‘classical school’, and incidentally puts the corn-model device into the shade. The episode illustrates strikingly, perhaps uniquely, the effects of empirical experience, particularly changing industry structure, on intellectual effort, Malthus personifying the transition between the agricultural and industrial eras. If one feature must be singled out as defining Malthus’s significance for economics, this would be my choice.

5 Applied Economics

Malthus's support for public works in circumstances of depression illustrates well his appreciation of the complexity of applied economics. An aspect of the problem is encountered in the 1817 *Essay on Population* where Malthus points out the dilemma that should a problem of long-term excess labour supply exist, public works might complicate the solution, for in

no conceivable case can the forced employment of the poor, though managed in the most judicious manner, have any direct tendency to proportion more accurately the supply of labour to the natural demand for it. And without great care and caution it is obvious that it may have a pernicious effect of an opposite kind (Malthus 1817, volume III: 273).

The recommendation to finance public works from tax revenues provides a second instance. Malthus excluded borrowing, having in mind the legacy of the war in producing an unprecedented National Debt and concerned that taxation required to finance the debt, 'if pushed to any considerable extent, can hardly fail of interfering with the powers of production' (Malthus 1820: 484, 1836: 411). Furthermore, a large debt aggravated 'the evils arising from changes in the value of money', rising prices unfairly reducing the real purchasing power of annuitants, and falling prices increasing the real burden of taxation to fund the debt on producers (ibid.: 1820: 484–485, 1836: 412). Notwithstanding, on balance the least objectionable alternative in the post-war emergency circumstances was an immediate increase in taxation to finance the proposed public expenditures. But what was allowed in emergencies did not justify a policy 'of raising the supplies of a long and expensive war within the year'—that is, by taxation—and this because (as had occurred in 1815 on Malthus's reading), upon the 'restoration' of taxes, there would result a 'stagnation' accompanied by 'general distress' due to a failure of consumer spending to compensate for reduced government spending (ibid.: 1820: 503, 1836: 423). (Recall the concern with excessive 'saving' in depression, referring in this context to hoarding, not real investment.) In brief: 'The evil occasioned by imposing a tax is very rarely compensated by the taking it off', from which Malthus inferred conservatively

that taxes should never be imposed, nor to a greater extent, than the necessity of the case justifies, and particularly that every effort should be made, consistently with national honour and security, to prevent a scale of expenditure so great that it cannot proceed without ruin, and cannot be stopped without distress (ibid.: 1820: 519, 1836: 435).

The moral arising from all this relates to the problem of correcting faulty policy: ‘To state these facts is not to favour taxes, but to give one of the strongest reasons against them; namely, that they are not only a great evil on their first imposition, but that the attempt to get rid of them afterwards, is often attended by great suffering’, alluding we have argued to hoarding (*ibid.*: 1820: 521, 1836: 437). The same sort of problem applied to proposals to pay off the National Debt, Malthus raising the question

whether the evils attendant on the national debt are not more than counterbalanced by the distribution of property and increase of the middle classes, which it must necessarily create; and whether by saving, in order to pay it off, we are not submitting to a painful sacrifice, which, if it attains its object, whatever other good it may effect, will leave us with a much less favourable distribution of wealth? By greatly reducing the national debt, if we are able to accomplish it, we may place ourselves in a more safe position...; but grievously will those be disappointed who think that, either by greatly reducing or at once destroying it, we can enrich ourselves, and employ all our labouring classes (*ibid.*: 1820: 507, 1836: 426–427).

These complexities regarding policy are closely related to that of defining ‘optima’—the problem of ‘proportions’—well illustrated by the advantage to be derived from saving from the perspective of sustainable growth. For while growth certainly hinged on new investment, ‘the principle of saving, pushed to excess, would destroy the motive to production’ (*ibid.*: 1820: 8, 1836: 6). Unfortunately, the ‘intermediate point...taking into consideration both the power to produce and the will to consume [where] the encouragement to the increase of wealth is the greatest’ (*ibid.*) was not easily definable. In fact, ‘the resources of political economy may not be able to ascertain it’ (*ibid.*: 1820: 9, 1836: 7). Yet more strongly: ‘What the proportion is between the productive and unproductive classes of a society [1836: the productive labourers and those engaged in personal services], which affords the greatest encouragement to the continued increase of wealth...the resources of political economy are unequal to determine’ (*ibid.*: 1820: 464, 1836: 399). The ideal distribution of property was similarly illusive: ‘There is here a point...though we may not know how to place it, where the division of property is best suited to the actual circumstances of the society, and calculated to give the best stimulus to production and to the increase of wealth and population’ (*ibid.*: 1820: 9–10, 1836: 7–8).

Malthus considered applied economics very much as an art—Keynes stressed the ‘intuitive’ dimension—heavily reliant on the appreciation of theoretical principles and on empirical circumstance. His procedure militates against the tendency towards universalist policy proposals based on doctrinal,

even ideological, positions. Malthus's original support of the Corn Laws provides a striking instance, for we have shown it to reflect not a defence of the 'class interests' of landowners, but a perception of the '*national* interest' requiring modification with the disintegration of the old 'Commercial System' and in the light of the actual experience of industrial buoyancy.

6 Conclusion

I have maintained that to place Malthus at the head of an anti-Ricardo 'inductivist' line as does Keynes is unconvincing, Malthus refusing to join Richard Jones in his inductivist campaign and protesting against those who declaimed against 'theory and theorists...prid[ing] themselves upon the distinction of being practical'. I have confirmed Malthus's bona fides as a leading theorist by reference to his impressive land-based growth model; his perception of an aggregate-demand problem, independent of the land constraint; and his Physiocratic analysis of the source of surplus in agriculture. The subsequent abandonment of the latter perspective, and with it the case for agricultural protectionism, illustrates the effects of changing industry structure on intellectual effort, Malthus personifying the transition between the agricultural and industrial eras, and more generally demonstrating supreme caution when addressing policy issues where factual circumstance relating to time and place is of the essence. But in this domain of applied economics too Keynes's methodological dichotomy proves unjustified. It is inconceivable that Keynes would have drawn his sharp contrast to Ricardo's detriment had he recalled the latter's concern with policy problems relating to short-run transitional states in a variety of contexts (see Hollander 1979: 491, 572–573, 591, 658–659), and particularly the countenancing of permanent devaluation of the pound should that be called for to avoid severe deflation.

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12

George Pryme (1781–1868)

Robert A. Cord

1 Introduction

The Professorship of Political Economy at Cambridge has a long and distinguished history dating back nearly two centuries. However, unlike many of those who have held this prestigious position, the first holder, George Pryme, is almost unknown to academic and professional economists, including, dare one say, to historians of economics.¹ There is good reason for this, namely that Pryme was and is not recognised as an economist who made any original or significant contributions to the subject. Nevertheless, it is difficult to deny his importance as an organiser and crusader for the teaching and promotion of political economy as a serious academic discipline at Cambridge at a time when the University was at risk of being left behind by other institutions. Section 2 of this chapter presents a biography of Pryme's interesting and varied life, tracing his early years and then undergraduate education in mathematics at Cambridge, his subsequent work as a barrister, and as a Member of Parliament (MP). Section 3 is an in-depth examination of Pryme's unceasing efforts to promote political economy at Cambridge combined with some of his views on the subject, where they can be ascertained. Section 4 concludes the chapter.

¹For instance, although Pryme must have been known to Schumpeter, he fails to get a mention in the great Austrian's monumental, albeit somewhat idiosyncratic, *History of Economic Analysis*.

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2 Life

George Pryme was born in April 1781 in Cottingham, a village located just outside of Hull in East Yorkshire. He was descended from a Huguenot family who had settled in the north of England after migrating from French Flanders. Pryme was the son of one Christopher Pryme, a merchant of the local area, and Alice Dinsdale.² Financially, the family was comfortable, with Pryme inheriting a considerable amount of property around the Hull area following his father's death during George's childhood. He was sent to private schools at Nottingham and at Bunny, before, in 1796, entering Kingston-upon-Hull Grammar School, perhaps best known as the former school of William Wilberforce. At Hull, Pryme came under the influence of headmaster and divine Joseph Milner and was also privately tutored by John Dawson, a mathematician and surgeon. Dawson had strong connections with Cambridge and could lay claim to having tutored at least eight pupils who would go on to become Senior Wranglers. It is not unreasonable to suggest that Dawson probably played an important part in steering the young Pryme towards an undergraduate education at Cambridge, specifically in the Mathematical Tripos. This he embarked on in October 1799, becoming a student at the staunchly liberal, Jacobin stronghold of Trinity College, which for these same reasons was unique but also relatively isolated and reviled within the University. Pryme was a star pupil. Although he was a student of mathematics, securing a scholarship in April 1800, he was perhaps better known for his efforts in composing Latin epigrams and Greek odes, winning him the Browne Medal in 1801 and 1802. There were other prizes to follow, including another prize for a Greek ode in 1804, the Members' Prize in 1804 for an essay in Latin entitled 'The Causes of the Decline and Fall of States', a further Members' Prize a year later, and, a few years down the line, a Seatonian Prize in 1809. Pryme had, not surprisingly, become 'Prize Pryme'.³

In between this adulation, Pryme was placed Sixth Wrangler in the Mathematical Tripos of 1803, an achievement which, along with the aforementioned prizes, helped him become a Fellow of Trinity a couple of years

²Through his mother, Pryme was distantly related to the Anglican minister, theologian, and co-founder of Methodism, John Wesley.

³Pryme continued to compose poetry well into later life. His *Jephthah, and Other Poems*, which appeared in 1838, was critiqued in *The Monthly Review*, where it was noted that 'from a lecturer on the principles and sources of the wealth of nations, one does not readily expect to hear the chieftain's triumphs or the maiden's tragedy chaunted in loftily constructed rhyme' (Anonymous 1838: 308). Poetic works by Pryme also get their own listing in *The Cambridge Bibliography of English Literature: 1800–1900*, published as recently as 1999 (Shattock 1999).

later in October 1805. His placing in the Tripos was unexpected, as Pryme himself admitted in his *Autobiographic Recollections* published in 1870:

As I had divided my time almost equally between my classical and mathematical studies, I had not expected to do more than just obtain a place at the bottom of the Wranglers' list. When the brackets came out on the morning of the last day of examination, I began to look at the place whereabouts I hoped to be, and not finding my name as I read downwards, felt dismayed, till, casting my eye upwards, I saw my name in the second bracket, as sixth wrangler [there were 13 wranglers in 1803] (Pryme 1870: 55).

The Cambridge examination system has of course changed radically since Pryme's day. To start with, there were only two degrees available at Cambridge in the early nineteenth century, mathematics and classics. Moreover, the amount and type of knowledge then required to obtain a degree was, as Pryme himself admitted, scarcely believable: 'Two books of Euclid's Geometry, Simple and Quadratic Equations, and the early parts of Paley's Moral Philosophy, were deemed amply sufficient. Yet in the year 1800 three students failed to pass even this test' (ibid.: 92).

Despite a good performance in the Mathematical Tripos, Pryme decided to take up the study of law, this taking him to Lincoln's Inn in London in October 1804 where he became one of five pupils of John Atkinson, a well-known special pleader. Pryme's pupillage seems to have gone well as he was called to the Bar in November 1806. However, his time in London turned out to be rather short as he was forced to leave the city in the summer of 1808 on health grounds, being advised by his doctor that his life might be at risk were he not to seek out the cleaner air of Cambridge. Pryme's life in the law world had just begun and he was to carry it on through various roles in and around Cambridge, including practising on the Norfolk Circuit and Wisbech Sessions, ending up securing a considerable practice; he was also appointed to several public offices, including Paving Commissioner. Being forced to leave London was nevertheless a 'severe disappointment' (ibid.: 83) for Pryme who had set his sights on a legal career. Given his undoubted talents, it is an intriguing question how far he might have risen within the judiciary. However, the legal profession's loss turned out to be the economics profession's gain. Marriage to Jane Townley followed in August 1813, with Pryme and his wife living in Barnwell Abbey just outside Cambridge. It was not long after, in March 1816, that he began to lecture on political economy at the University.

Aside from teaching, practising as a barrister, and composing poetry, the other main calling of Pryme's life was his service as an MP for Cambridge. Given that he had been a student at Trinity, it was perhaps no great surprise that Pryme was able to successfully stand for election. As Reid (2013: 630–631) notes, 'Between 1790 and 1820 [albeit slightly before Pryme became an MP] 151 men who had passed through its [Trinity's] gates were elected to the House of Commons. No Cambridge college could rival its record of producing statesmen'. Elected as a reforming Whig, Pryme first entered parliament in 1832, being re-elected in 1835 and 1837, before being forced to stand down as an MP in 1841, again due to health reasons. Pryme was active as a politician and was a supporter of, amongst other things, shorter parliaments and the repeal of the Corn Laws, making a speech on the latter in the House on 2 April 1840, where he employed Ricardo's theory of rent to support his position (although Ricardo himself was eventually forced to make his case for free trade by using the theory of comparative advantage (see Kubo 2015: 7)).⁴ Pryme took part in parliamentary proceedings on a number of other issues, including the Tithe Commutation Act and university reform at Oxford and Cambridge,⁵ and also introduced a bill to the Commons in November 1837 which proposed the abolition of the grand jury system in Britain, although this was resoundingly defeated by 25 votes for to 196 against (see Elliff 1938: 8–9).⁶ Pryme was intimate with a number of political luminaries during this period, including future Prime Ministers Lord Palmerston and Lord John Russell, and Chancellor of the Exchequer Lord Althorp, as well as various noted people outside parliament, such as the poet William Wordsworth, the historian Thomas Babington Macaulay, and the scientist Sir Humphry Davy.⁷

Following his retirement from parliament, Pryme spent his time teaching at Cambridge and conducting his legal practice, followed by a move with his family to Wistow in Huntingdonshire in 1847, where they lived on a 500-acre

⁴Fetter (1975) examines the influence that economists elected as MPs had on British legislation during the period from 1819, Ricardo's election to parliament, to 1868, when John Stuart Mill left parliament. Fetter argues that 'in the halls of Westminster...back-benchers had an impact on legislation, by voting, by speeches, by sponsoring of legislation, and by activities on committees, that is unmatched at any other time or in any other country' (ibid.: 1,060). Fetter's text mentions Pryme twice, albeit only in passing on both occasions.

⁵A motion introduced by Pryme to the Commons in 1837 for reform of the universities was withdrawn after he was assured that an overhaul would follow soon thereafter (see Rashid 1980: 288). However, reform did not take place for at least another 15 years.

⁶The grand jury system eventually ceased to be used, at least in England, in 1933.

⁷Pryme was also a very good friend of anti-slavery campaigner Thomas Perronet Thompson; the two had been at school and Cambridge together. It does not seem unreasonable to argue that Thompson was an influence on the development of Pryme's liberalism. (Copies of the letters exchanged between Pryme and Thompson are housed at the Hull University Archive. See <http://www.hull.ac.uk/arc/downloads/DTHcatalogue.pdf>.)

estate. One of Pryme's hobbies during this period was the cultivation of his land through the basics of chemistry that he had picked up. Continuing to lecture on political economy until late 1863, he resigned from Cambridge, with the University shortly thereafter agreeing to put the Professorship of Political Economy on a permanent basis. Pryme died in December 1868.⁸

3 Establishing Economics at Cambridge and Views on Economics

Pryme's introduction to the discipline of political economy took place when he was 14-years-old. However, it was an inauspicious beginning for somebody who would go on to become the first Professor of Political Economy at Cambridge. Pryme recounts how when he was being schooled at Bunny he came across a copy of Smith's *Wealth of Nations*. He read three or four chapters but found it 'too deep' (Pryme 1870: 23). Despite this, Pryme notes that he was particularly attracted to Smith's ideas on the division of labour: '[I was] interested in the division of labour, and in the fact that with it ten men could make, amongst them, upwards of forty-eight thousand pins in a day, whilst separately they could not have made more than two hundred' (ibid.). Fast-forwarding to 1804 when Pryme arrived in London to begin his legal training, he relates how, as before, he knew very little political economy:

Soon after my arrival in town I became a member of a debating society, called "the Academical," from the rules requiring a candidate to be a member of [a] University or Inn of Court. We met once a week at a room in Bell Yard, between Lincoln's Inn and the Temple, for debate. Recent politics, or allusions to living statesmen, were excluded. Modern History and Political Economy were occasionally touched on, and were subjects wherein I felt my deficiency (ibid.: 65).

His exposure to *The Wealth of Nations* as a boy aside, it seems, at least from Pryme's *Autobiography*, that the most important driving force for the reigniting of his interest in political economy as an adult was this 'deficiency' in knowledge he felt at the meetings of the Academical. The result was the

⁸One of Pryme's legacies was his collection of books on economics. As Fetter notes, 'In 1840 it was probably the greatest collection of English economic literature in the world' (Fetter 1939: 416). It has nevertheless had a somewhat nomadic existence. The collection was given to Cambridge in 1872 but remained uncatalogued until the mid-1930s. The books then arrived at the Marshall Library, where they were restored and catalogued under Sraffa's direction. The Pryme Collection is currently housed in the Rare Books Section of Cambridge University Library. It is made up of around 1,850 volumes, of which over 1,500 are British (see ibid.).

purchase of a copy of *The Wealth of Nations*, to which he devoted extensive study every Thursday. Pryme's zeal would have been fortified by an encounter with Malthus in 1805 where Malthus recalled how the population principle suddenly occurred to him during a discussion with his father (*ibid.*: 66).

Smith and Malthus aside, with his permanent return to Cambridge in 1808, Pryme's main focus for the next few years was the establishment of his legal practice; it was not until 1816 that he was to become the first person to offer a complete set of lectures on political economy at an English university. Although this is Pryme's main claim to fame, it is worth considering the status of political economy at Cambridge and other universities during this period and why there was an explosion of interest in it in the early part of the nineteenth century. In his Inaugural Lecture upon becoming the Drummond Professor of Political Economy at Oxford in 1952, Hicks stated that, 'Oxford could lord it over Cambridge in the days of Senior and Pryme' (Hicks 1953: 120). Although this was to a significant degree true, it perhaps overlooks earlier attempts to kick-start the academic study of the economy. In the 1750s, Lord Townshend had offered prizes in economics at Cambridge only to then object to the questions that were set for such awards. This was followed by lectures on commerce delivered by William Paley in the 1770s, unsuccessful urgings by the Reverend R.A. Ingram of Queens' College for political economy to be part of the Cambridge curriculum, and William Smyth's consideration of some elements of political economy in his Modern History course starting in 1807 (see Rashid 1980: 282). Another Professor of History, John Symonds, had devoted many years to examining the Italian agricultural sector in addition to problems linked to colonisation (see Checkland 1951: 44–45).

Outside of Cambridge, Smith, as the holder of Edinburgh's Chair in Moral Philosophy, delivered his series of lectures on political economy in 1753 to be followed by the philosopher and mathematician Dugald Stewart in 1800 and 1801;⁹ Malthus commenced teaching at the East India College in 1805; and there was activity at Oxford (even though the Drummond Professorship was not created until 1825). However, it would, of course, be misleading to suggest that the study of political economy was monopolised by the British.

⁹Pryme notes how Stewart's lectures were apparently so popular that 'several Members of our own University [Cambridge] went from the South of England to pass the winter at Edinburgh, for the purpose of attending them' (Pryme 1819: vii–viii, fn. †). What Pryme does not mention was the absence of regular lectures on political economy at turn-of-the-century Cambridge as well as the fact that the Grand Tour had become too dangerous due to the French Revolutionary Wars, in particular the War of the Second Coalition from 1798 to 1802 (see Checkland 1951: 43). Both of these factors contributed to English students travelling north in search of new experiences and 'excitement'.

The first Professorship of Political Economy in Europe was in Naples as early as 1754, with Antonio Genovesi as its first occupant. Other chairs followed in France and Russia.

There is a deeper context here which points to why the discipline of political economy itself became more popular during this period.¹⁰ A number of general and specific factors coincided, quickly capturing the public's imagination. General influences included: a fetish for more precise theories which could only be provided by 'science', this at the expense of philosophy and history; a perceived alignment between societal liberalism and the theories being offered up by political economists; the emergence and consolidation of the Industrial Revolution in Britain and the changes this engendered in terms of population and productivity, greater class consciousness, and the reform of governmental institutions; and finally, the growth of the free press. Amongst the more specific factors were Ricardo's election as an MP and the employment of free trade theory against the Corn Laws. It was far from a clean sweep for political economy, however, with opponents in prominent places. For instance, Poet Laureate Robert Southey referred to it as 'diarrhea [sic] of the intellect' (Southey quoted in Langer 1987: 15). Such observation aside, it was clear that political economy had secured a place in the British psyche.

With this momentum, Pryme embarked on his project to increase the profile of political economy at Cambridge. The first step in what turned out to be a somewhat tortuous process for Pryme personally was an approach he made to Vice-Chancellor John Kaye to ask permission to give a set of lectures to Cambridge students. Pryme states in his *Autobiography* that he waited until Kaye had become vice-chancellor as he was known for his liberal views (Pryme 1870: 120). Kaye immediately expressed his support for Pryme's venture. There were serious objections from some heads of the colleges, but these were ignored by Kaye. Nevertheless, the one condition that had to be met was that Pryme was not allowed to deliver his lectures before midday for fear that they might interfere with other teaching in the University. Unperturbed, Pryme set about compiling his 'elementary and eclectic' lectures (*ibid.*: 121), which he delivered for the first time in March 1816 to an audience of 45 people.

We shall return later in this section to the story of Pryme's efforts which culminated in the establishment of a permanent Professorship of Political Economy at Cambridge. However, to get a flavour of Pryme's views on the state of political economy as it existed in the early nineteenth century and his ideological outlook, we can briefly examine some of the remarks he makes in

¹⁰This paragraph draws on Langer (1987: 1–9).

the Prefaces he wrote for the various published syllabi for his course,¹¹ the first edition of which was in 1816 (subsequent editions appeared in 1819, 1852, and 1859),¹² his Introductory Lecture, his other lectures, and snippets from his *Autobiography*.

Pryme begins the Preface to the first edition of his *Syllabus* by asking why political economy was not being more widely taught at British universities, especially when Britain had ‘produced a Philosopher [Adam Smith] who has done little less for [the subject] than Newton did for Natural Philosophy’ (Pryme 1859: iii). The answer for Pryme was ‘*the want of an English work from which the principles of this science may be obtained with any ordinary labour*’ (ibid.: iv; italics in original). Pryme notes that *The Wealth of Nations* was read by few Englishmen and approvingly quotes Say that the book is ‘but a confused assemblage of the most sound principles of Political Economy’ (Say quoted in ibid.: 5). Pryme’s lectures were meant to provide clarity, in what in fact amounted to no more than a popularisation of the discipline as it then stood. Moreover, by the time of the third edition of the syllabus, albeit more than three decades later in 1852, Pryme’s course had become more or less redundant, long overtaken by more comprehensive treatises, including, as Pryme himself noted, those by McCulloch, Mill, and Torrens.

Meanwhile, in what seems to have been a one-off publication issued in 1823, Pryme authored a version of his syllabus which is probably most interesting for its Preface and Introductory Lecture. In the Preface, Pryme acknowledges the influence that Stewart’s lectures had on his syllabus, with Pryme relying on the notes taken by two students who had been on Stewart’s course (Pryme 1823: iv–v). Although Pryme adhered to Ricardo’s deductivist methodology, there were also strains of Stewart, a fellow economic liberal, to be found in his lectures with respect to not only the method but also the scope of political economy and its role in explaining human history. For example, Pryme agreed with Stewart’s view on the development of wealth and population, this founded on the emergence of farmers, manufacturers, and merchants, in that order. This was in contrast to Smith’s four-stage theory of development, starting with the hunting and gathering stage, followed by pastoralism and nomadism, then agriculture, and, finally, commerce.

Pryme supported Stewart in his theory that the main driver of economic growth was population increase (see Kubo 2013: 71, 75, 79). However, in line

¹¹For ease of reference, I use the Prefaces to the first three editions of Pryme’s *Syllabus* as they appear, reprinted from the originals, in the fourth edition of the *Syllabus* (Pryme 1859). Pryme does not appear to have written a Preface to the fourth edition.

¹²A copy of Pryme’s 1816 syllabus can be found in the form of an appendix in Langer (1987: 195–208).

with his liberal economic approach, Pryme was also of the view that less government intervention in the form of lower taxation and free trade played a part in driving prosperity. In his *Autobiography*, he goes out of his way to pay homage to Lord Althorp, Chancellor of the Exchequer between 1830 and 1834, congratulating Althorp's decisions to lower stamp duty on newspapers and on advertisements, to lower and equalise duties on the manufacture of paper, and to abolish the tax on the tanning of skins (Pryme 1870: 211). Finally, later on in his *Autobiography*, Pryme describes a brief conversation he had at Cambridge with the noted German diplomat and scholar, Baron Bunsen. Whilst acknowledging Bunsen's 'fine intellect', Pryme did not consider his views on political economy to be very enlightened as suggested by his advocacy of 'Free Trade in Germany, but only between the different States' (ibid.: 257).

Pryme divided his course into 20 lectures, beginning with an Introductory Lecture, followed by a host of other topics, including the 'Origin of Commerce', 'Money', 'Price of Things', 'Population', 'Banks', 'Finance', and 'Loans and Public Funds'. The aim of the lectures was, according to Pryme, basic but clear:

They attempted to analyse the original and efficient causes of national prosperity—to shew [sic] by what measures of the legislature, and by what conduct of individuals in private life, it is augmented or diminished—and to assist the reader of history in explaining the phenomena of the strength or weakness, the rise or fall, of States. They were of an elementary and popular nature, requiring no previous knowledge of the subject. They were intended to facilitate the study of a science hitherto inaccessible without the most arduous perseverance; to simplify the order, explain the obscurities, and point out the errors of Adam Smith's *Enquiry into the Wealth of Nations*; to combine with his discoveries what the experience of subsequent events, and the researches of subsequent authors have taught, and to place some part of the subject in a point of view different from what any writer had done. Their plan was—first, to trace the history of national wealth from the rudest to the richest state of society, and to examine each change as it naturally arises in the progress of opulence and civilization; secondly, briefly to explain the systems of the ancients, of Dr Paley [Pryme was critical of Paley's utilitarianism], of the French Economists, and what is called the commercial system; and thirdly, to explain the principles of taxation and finance (ibid.: 121–122).

It is not the intention here to examine in detail the content of each of Pryme's lectures but rather to highlight a few salient points.

In his Introductory Lecture, Pryme begins by trying to provide a definition of what is meant by political economy, even though he confesses that he has not completely satisfied himself in this respect. Pryme posits that political

economy is, ‘The science which investigates the origin of national wealth and the causes of its increase or decline’ (Pryme 1823: 9–10), a clear echo of Smith.¹³ Pryme’s respect for Smith’s achievement is unquestionable: ‘In 1776, Adam Smith presented to the public his “Inquiry into the Nature and Causes of the Wealth of Nations”, a work so profound, so original, and for the most part so incontrovertible, that Political Economy may be said before that time to have had no existence’ (ibid.: 23). Nevertheless, it should be remembered that Pryme had agreed with Say’s criticism of *The Wealth of Nations*, and now used his own lectures to point out some of its faults, at least as he saw them, even if none of these criticisms were of a fundamental nature. For instance, in his second lecture, Pryme asks, what constitutes wealth? He posits three sources, namely security of property, capital, and division of labour. But Pryme then argues that with respect to the advantages of the division of labour, Smith’s analysis only identifies dexterity, the saving of time, and the invention of a greater number of machines, Pryme arguing that Smith misses out experience.¹⁴

As noted, although Pryme chose to point out some of the deficiencies of his fellow economists’ theories, such criticisms did not pose a serious threat to these analyses. Indeed, Pryme went to great lengths to avoid serious controversy in his lectures. He liked to boast that it was difficult for attendees to decipher what his political persuasion might be. This was driven by a concern that he could easily be discouraged or even removed from lecturing by the University authorities, especially if he advocated economic ideas which could be perceived as being at odds with Cambridge’s financial interests. The most obvious example of this was Pryme’s treatment of Ricardo, or rather lack thereof. Granted, the first edition of Ricardo’s highly influential *Principles* did not appear until 1817, too late to be included for consideration in Pryme’s 1816 syllabus. However, even in the 1819 syllabus, there is little if any mention of Ricardo. Instead, Pryme limits himself, albeit still rather loftily, to noting in the advertisement for the second edition that Ricardo ‘may seem to have effected a material change in the science; but his fundamental principle (that Dr Smith’s three component parts of price

¹³ A full set of notes (79 pages) taken by the otherwise unidentified ‘W.H. and C.J.’ at the set of lectures given by Pryme in 1818 are held at Lambeth Palace Library, reference MS 1740.

¹⁴ Selected other issues on which Pryme attempts to ‘controvert’ Smith, as he usually puts it, are the origin and cause of barter, Smith’s account of the influence of capital on wages and profits, and the approach that Smith adopts in criticising the proposition that land is the sole source of wealth, which Pryme claims is an attack on ‘the superstructure, not the foundation’ (Pryme 1819: 21). Smith was not Pryme’s only target. He also questioned, amongst others, Paley on the foundation of the right of property; Aristotle, Montesquieu, and Locke on the notion of monied interest; and the French economists’ view that all taxes fall ultimately on land.

are reducible to one, viz. the wages of labor, which alone constitutes the value of every thing) was explained and some of its consequences deduced in these Lectures [Pryme's] a year before' (Pryme 1819: xii).¹⁵ Pryme was not moved to a greater acknowledgement of Ricardo even in later editions of his *Syllabus*, despite hints from his acquaintances as to Ricardo's importance. One example was a letter Pryme received from Say in August 1819 where Say talks about the preparation of the fourth edition of his *A Treatise on Political Economy*, stating:

It will be more comprehensive, and I hope it will be found more complete and better connected than all the previous ones. The criticisms of Mr. Ricardo have been very useful to me. They have obliged me to elaborate on the doctrine of values as the measure of riches, and to solve, apart from many others, this interesting problem: *How is the low price of products at the origin of the riches of nations?* (Say quoted in Pryme 1870: 126; italics in original).¹⁶

Nevertheless, as already noted, it should be remembered that Pryme was a supporter of deduction, in line with Ricardo. In the Introductory Lecture contained in the 1823 version of his syllabus, Pryme confidently states that

[p]olitical economy...is established on firm foundations, since the principles which serve for its basis, are rigorously deduced from incontestable *general* facts, or practical maxims of good sense approved by experience in all ages; and to determine the justice of which we have only to retire into our own bosoms, or to open our eyes to what is passing around us (Pryme 1823: 30; italics in original).

However, in his lectures, Pryme never explicitly makes clear the link between Ricardo and his own support of the deductivist method. Taking such a position would have meant going up against the Cambridge Inductivists, which included the powerful Master of Trinity College, William Whewell, Richard Jones, John Cazenove, Charles Babbage, and John Herschel, a formidable group by any standard. In addition to Ricardo's deductivist credentials, the Cambridge Inductivists were also wary of his theory of rent, which suggested that there was little relationship between labour productivity and wages, a reformulation of the iron law of wages where the amount paid to workers, in this case by landowners, tends to move towards the minimum

¹⁵Kubo (2013: 82–86) shows that, despite his claim, Pryme did not arrive independently at Ricardo's theory of value and, moreover, that he did not understand it.

¹⁶The translation from French (which is how it appears in Pryme's *Autobiography*) to English used here can be found in Gehrke and Kurz (2001: 479, fn. 13).

necessary to sustain those workers, with any excess generated by the land being appropriated by landowners. With specific reference to the finances of Oxford and Cambridge, Checkland (1951: 53) notes, '[It] was not merely the particular vulnerability of the universities to the rent theory that could create a hostile feeling: the Universities and the Church were bound in triple alliance with the landowners'.¹⁷

On a penultimate note, Pryme took the bold step in the third edition of his syllabus (1852) to introduce a new topic which he entitled 'Application of Mathematics to Some Doctrines of Political Economy'.¹⁸ As there are no extant notes on Pryme's lectures from this later period, it is not clear what he presented to students. However, his motivation for including the topic was Whewell's 'Mathematical Exposition of Some Doctrines of Political Economy', which had appeared more than two decades previously in 1829 in *Transactions of the Cambridge Philosophical Society* where Whewell had mostly examined issues pertaining to taxation (see Henderson 1996: 287). By including mathematics in his lectures, Pryme can be considered something of an innovator as it would be another two decades following the publication of the third edition of his syllabus that the more regular use of mathematics in economics started to take place with the marginalist revolution of the 1870s.

Finally, on a more abstract point regarding Pryme's lectures, it can perhaps be argued that he also played a part in creating the so-called Cambridge oral tradition in economics. Johnson and Johnson (1974: 261) describe this tradition as 'a style which relies heavily on allusion to and caricaturization of the work of a few eminent contemporaries and predecessors rather than on meticulous documentation of sources'. Pryme (1823: v) notes how he had been 'pressed by several friends to publish my entire Course of Lectures; but I thought that the cultivation of this strangely neglected science would be better promoted by continuing to give them orally, with such improvements from year to year as further reading or maturer reflection might furnish'. Pryme's undergraduate education at Trinity probably played a part in this appreciation for oral presentation in so far as he would have observed, and probably participated in, the 'declamation' engaged in by his contemporaries who had arrived at Cambridge from schools such as Eton, Harrow, and Westminster (see Reid 2013), this at a time when there was in fact a wider shift within

¹⁷ Pryme's reluctance to overtly support Ricardo was made further apparent on the occasion of Ricardo's death in 1823 when James Mill wrote to McCulloch stating that they were his only two genuine disciples (see Checkland 1949: 40). In the same article, Checkland adopts a sociology of science approach to argue that it was Ricardo's disciples who were the key to propagating Ricardianism in the face of stiff opposition.

¹⁸ It was retained in the fourth edition (1859).

Cambridge away from an oral culture and more towards writing (see Warwick 2003). As such, it may be that when we mostly associate the oral tradition with Marshall, we should also pay homage to Pryme.

Having considered some of Pryme's approach to political economy, we can now return to his efforts to establish the Professorship of Political Economy. Again, some context is instructive here. There were various factors operating at Cambridge and more widely which would have worked in Pryme's favour and some against him in his endeavour. On the negative side, the subject of political economy faced an uphill struggle to gain recognition at Cambridge during Pryme's time as the University was almost wholly geared towards producing either clergymen or natural scientists. With there being little explicit link between political economy and the Classical and Mathematical Triposes, there would have been minimal incentive for students to attend Pryme's lectures. Second, the proposal by Pryme to actually deliver lectures was, as mentioned, met with opposition in some quarters. It was not unknown for the holders of some Cambridge professorships to go for decades without having to stand in front of an audience, the holders of the Lady Margaret Professorship of Divinity being the most notable example (see Henderson 1984: 5). Third, the creation of professorships by Cambridge was hardly a common occurrence, this in part due to a lack of finance. The first was the aforementioned Lady Margaret Professorship of Divinity, established in 1502, followed by five Regius Professorships in 1540. There was then a gap of nearly a century before a Professorship of Arabic received approval in 1632, followed by another 14 professorships in the period up to 1800; the Professorship of Political Economy was next in line but had to wait for nearly three more decades. Finally, decisions by the University as to who should be elected to professorships appeared, at least on some occasions, to be somewhat arbitrary, this resulting in what were, to the outsider at least, somewhat surprising appointments. A good example of this was relayed by Pryme himself in his *Autobiography* when he discusses the vacancy for the Professor of Geology that had arisen in 1818:

The Woodwardian Professorship of Geology became vacant this year by the marriage of Professor Hailstone. There were two candidates for it, Mr Gorham of Queens', and Adam Sedgwick of Trinity. The latter professed to know nothing of the subject, but pledged himself, if elected, to master it, and to resign the assistant tutorship in order that he might give the more complete attention to it. Some of his friends however, myself among the number, voted for Mr Gorham, feeling that it was only just to do so, as he had been studying Geology for a long time. I need hardly say that Sedgwick, who was elected, completely redeemed his promise, and that his eloquent Lectures have been the delight of

all who have heard them. Mr Gorham's fate was different. He took a curacy, and was afterwards famous for his controversy with the bishop of Exeter (Philpotts) (Pryme 1870: 135).

Notwithstanding the fact that Gorham was better qualified than Sedgwick to assume the Professorship, Pryme was right to praise the latter, Sedgwick going on to become one of the most celebrated geologists of all time and holding the Woodwardian Professorship for no less than 55 years.

With these factors against him, it would not have been a surprise had Pryme decided that a permanent Professorship of Political Economy was out of the question. However, as we noted earlier, there was one particularly strong force in play during the early decades of the nineteenth century, namely the gathering pace of the Industrial Revolution. In terms of the courses that it offered, Cambridge was clearly not equipped to provide students with training that would have allowed them to enter and understand the nascent world of business. As such, there had to be a response from the University authorities. This came in the form of Whewell and Sedgwick, who led a wider reform movement at Cambridge, the main focus of which was the freeing of the University from religious control.¹⁹

Pryme's relationship with Whewell was particularly interesting. Whewell had what can only be described as a rather dim view of Pryme, writing in a letter to Richard Jones in May 1828 that Pryme's lectures were 'dull & unpopular, & will never be otherwise' (Whewell quoted in Henderson 1996: 287) and that Pryme was not suited to be a Professor of Political Economy. However, the reformist Whewell was sensible enough to realise that Pryme had already been lecturing for 12 years and that this had helped to strengthen his position in the University. Moreover, deductivism aside, Whewell broadly approved of the material that Pryme was teaching. He therefore did not campaign against the proposal to make Pryme a professor, and it was on this basis that Pryme was elected in May 1828 by a comfortable margin of 18 votes to 9 (see *ibid.*) (although the appointment did not carry with it any pay).²⁰

¹⁹ Pryme himself would become involved in trying to reform the University's religious arrangements. In December 1834, he suggested to the Senate, unsuccessfully as it turned out, that it appoint a committee to consider whether the requirement of religious subscription in order to proceed to a degree should be abolished (see Twaddle 1966: 48–49).

²⁰ The Professorship of Political Economy did eventually attract a salary. However, it was not until 1882 when its then occupant Henry Fawcett—also a Ricardian, whose election as Pryme's successor in 1863 was supported by Pryme but opposed by Whewell—started to receive £700 per annum that the Professor of Political Economy achieved parity, more or less, with other Cambridge professors (see Neild 2012: 16).

Even though Whewell had not campaigned against Pryme, the Master of Trinity had become more anti-Pryme in the years between 1828 and 1830. It was in the latter year that Whewell hatched a plan to try to remove Pryme and replace him with Malthus in a chair that would have been created to honour Tory politician and advocate of free trade, William Huskisson.²¹ However, for whatever reason, Whewell's plot fell through (see Kubo 2015: 5–6). Meanwhile, Pryme continued lecturing whilst also expanding his network. Most importantly, he became an honorary member of the important Political Economy Club in London in June 1833, a body founded mostly to support not only the cause of free trade but also independent discussion on topical issues.

With his tenure as an MP ending in 1841, there would have been some concern on Pryme's part as to the decline in attendance at his Cambridge lectures, this leading to delivery only in alternate years. Although he agreed to them, Pryme would also have been concerned that the four lectures given at Cambridge in 1844 by the economist Thomas Banfield might have threatened his position. However, Pryme was 'rescued' by the introduction in 1851, with the support of Whewell, of the Moral Sciences Tripos; political economy was made one of its compulsory modules. This immediately boosted attendance at Pryme's lectures to between 50 and 60.²² Attending students also had to pay for the privilege. Spurred on by this expansion in numbers and profile, Pryme rededicated himself to persuading the University authorities to make the Professorship of Political Economy a permanent position. This included three pleas in 1857, 1859, and, to the vice-chancellor, in 1861, all of which fell on deaf ears. With these rebuttals, which must have been something of a humiliation for Pryme given that he had been lecturing for 47 years, and the fact that he was by then in his early 80s, Pryme notified Whewell on 23 February 1863 of his intention to resign his chair but to stay in post until a successor was appointed. The authorities at Cambridge were relatively quick to act. A Syndicate was appointed on 20 March to consider whether the Professorship of Political Economy should be given permanent status, with a report issued on 1 May recommending such a course. A proposal was put to the Senate in October, where it was approved by a vote of 98–40.

²¹ A small digression: Huskisson certainly had an eventful life: When in Paris he witnessed the storming of the Bastille, was educated in economics by the Marquis de Condorcet, and served in the British government as President of the Board of Trade and Secretary of State for War. However, he is best remembered as the first person to be killed in a railway accident having been run over by Stephenson's *Rocket* as he attempted to shake hands with one its occupants, the Duke of Wellington, at the opening of the Liverpool to Manchester Railway.

²² Pryme was an official examiner for the new Tripos from 1851 to 1860.

4 Conclusion

The Professorship of Political Economy at Cambridge has a long and prestigious history, occupied by some of the most important economists ever to have graced the discipline. This chapter has attempted to tell the story of its first occupant, George Pryme, and his battles to create the Professorship and his associated work in establishing a more prominent place for political economy within the Cambridge curriculum, beginning with him being the first person to deliver a full set of lectures in the subject at any English university.

A few paragraphs into Section 3, we referenced Hicks's observation regarding the dominance of Oxford over Cambridge in the days of Senior and Pryme. The explanation of this view of Pryme can be found in Murray Rothbard's correct assertion, blunt though it is, that Pryme 'wrote nothing in economics and contributed to no important discussions' (Rothbard 2006: 105). For these reasons, Pryme is today a largely forgotten figure, even amongst historians of economics. However, history has perhaps been a little unfair to Pryme. Granted, economics would have been formally recognised at Cambridge sooner or later for the important subject that it is. But the question has to be asked: Without the foundation which Pryme was one of the key figures in laying, might Cambridge have had to wait many more decades than it did before it was able to legitimately claim to be a leading centre for economics, a reputation it began to secure under Marshall?

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13

Charles Babbage (1791–1871)

Renee Prendergast

1 Introduction

Charles Babbage's main work in political economy, *On the Economy of Machinery and Manufactures (EMM)*, was published in 1832 while he occupied the Lucasian Chair of Mathematics at Cambridge. Babbage had sought election to 'Newton's Chair' in 1820 and again in 1826 before he was finally elected in 1828. He occupied the Chair for just over a decade, resigning in 1839 to devote more time to the exhausting demands of his Analytical Engine. The actual duties associated with the Chair were small but Babbage could not be accused of overfulfilling them. He did not reside in Cambridge during his tenure and never actually lectured, although both in 1831 and 1832 he announced his intention to deliver material from *EMM* as Lucasian lectures (Schaffer 2003: 284).

There was an intimate connection between Babbage's activities as political economist, mathematician, and inventor. Through his interest in mathematics, he was aware of the shortcomings of the available mathematical tables and of the work being done in France to apply the principles of division of labour to the production of accurate sets of tables. This, in turn, led him to consider the potential for cost reduction and the elimination of errors through mechanisation of calculation. His work on the design and construction of his calculating

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engine required him to visit workshops and factories in Britain and on the Continent in order to familiarise himself with production processes and their limitations. The generalisations arising from these visits supplemented by other material provided the basis for his writing on the economics of machinery and manufacturing. Even Babbage's venture into theology in the *Ninth Bridgewater Treatise: A Fragment* (Babbage 1837 [1838]) was connected to his work on the Analytical Engine. Contrary to those who denied that mechanical philosophers and mathematicians had anything valuable to contribute on theological matters, Babbage argued that his work on the programmable Analytical Engine allowed him to conceive of an author of the universe who foresaw the varied yet necessary laws of its action throughout the whole of its existence (ibid.: 32–34).

Babbage's early schooling was mainly at the hands of private tutors, although he spent about three years at a boarding school near London. During his time at the school, Babbage became interested in mathematics in which he made considerable progress despite being largely self-taught. By the time he went up to Trinity College, Cambridge, in 1810, he had developed an interest in analysis which he could do with equal facility using the dots of Newton or the ds of Leibnitz. His Cambridge tutors showed little interest in his mathematical precocity, responding to his questions by advising that they were of no consequence since they would not be the subject of examination by the Senate House (Babbage 1864 [1991]: Chapter 4). Enros (1983) explains that, although Cambridge was renowned amongst British universities for its tradition of mathematical study, both the courses taught and the examinations given stressed synthetic mathematics as opposed to the analytics which were gaining ground on the Continent and which mainly interested Babbage.¹ Together with friends, Babbage set up an Analytical Society to promote the Continental style of mathematics. Babbage also enjoyed a variety of other pursuits. He attended lectures in chemistry and was fond of sailing, chess, and whist. On Sundays, he discussed 'knowable and unknowable things' over a long breakfast with a group of ten to a dozen friends including Hershel, Whewell, and Jones (Babbage 1864 [1991]: 24–25). According to Snyder, discussion at these breakfast meetings was frequently based on a passage from the writings of Francis Bacon. Bacon rejected both pure deduction and pure empiricism, arguing instead for an inductive approach which used observation and reasoning about those observations to create new scientific theories. He also proclaimed that 'knowledge is power' and argued that science could

¹ The truth of synthetic propositions is determined by how their meaning relates to the world, whereas analytic propositions are true by virtue of their meaning.

transform the conditions of life (Snyder 2011: 37–43). These views appear to have resonated strongly with the young discussants, all of whom were committed to an inductive approach to science and who became leaders in their respective scientific fields.

Babbage had migrated from Trinity to Peterhouse early in his university career. Given his academic abilities, he might have expected to graduate with honours but was prevented from doing so as a result of advertising views on religion which were unacceptable at the time. Consequently, he left university with an ordinary degree without having taken an examination. After he left Cambridge, Babbage continued to make progress in mathematics. He lectured at the Royal Institution and was elected a Fellow of the Royal Society. However, he was less successful in obtaining gainful employment having applied unsuccessfully for a number of teaching posts. Hyman (1982: 39) attributes Babbage's lack of success to the fact that he was a liberal at a time when the assignment of such posts was in the hands of Tory patronage. Babbage was obliged to continue to rely on his banker father's support. This was made difficult by the poor relations between them and the fact that his father disapproved strongly of Babbage's decision to marry before he was earning a good living (*ibid.*: 31–35). When his father died in 1827, Babbage inherited a sum of the order of £100,000. This substantial amount must have been of great importance in allowing Babbage to pursue his scientific interests and in facilitating the independence of spirit he displayed throughout his life.

While continuing with his mathematical and scientific work, Babbage began to speculate about a number of inventions, including machinery to compute arithmetical tables. Advised by William Wollaston that this was a promising subject, Babbage embarked on what was to be his life's main work for which he is now justly famous (Babbage 1864 [1991]: Chapter 5; Hyman 1982: 51). Babbage perceived that the method of differences supplied a general principle which would allow a wide variety of tables to be computed using only addition and subtraction. He began to experiment with models of some portions of a machine to perform these operations. By 1822, he had devised a working model of his difference engine and had plans for a more advanced version. However, recognising that the expense of construction was such that the project was unlikely to be profitable for a long time, Babbage persuaded the government to provide support. This it did until 1834, although the final decision to withdraw funding was made only in 1842. Although Babbage had demonstrated the viability of the project by constructing an important part of it, the project was taking much longer than originally planned. Matters were not helped by either Babbage's quarrels with his chief engineer and former colleagues or his honesty in informing the government that the engine

whose construction it was supporting was likely to be rendered obsolete by his newest invention. As the writer of Babbage's obituary in *The Engineer* (1871: 284) observed: 'What but failure could attend a man who, having been given national aid to complete a given machine—goes a certain length with it, and then suddenly proposes to abandon it because he had invented the Analytical [E]ngine, one of enormously greater power?' The same writer opined that great fame would have been achieved by Babbage had 'his noble brain been united with more common sense notions of managing mankind' (ibid.).

Babbage did eventually achieve fame in the twentieth century when it came to be recognised that his Analytical Engine had all the essential features of the modern computer. Had he been of a more pragmatic disposition, his concrete achievements during his lifetime might have been greater. But this begs the question: Could a pragmatic mind have conceived of or attempted to implement a radical innovation such as the Analytical Engine? In addition to his self-belief and curiosity, Babbage appears to have had a natural combativeness which did not always further the objects he had in mind. His often antagonistic approach is clearly visible in his attack on the Royal Society and its officers in *Reflections on the Decline of Science in England and Some of its Causes* (Babbage 1830 [1970]) and even more so in his late *A Chapter on Street Nuisances* (Babbage 1864). Yet it would be a mistake to think that Babbage was without friends or influence. While *Reflections* did not lead to the reform of the Royal Society, it was a catalyst for the creation of The British Association (now British Science Association). With Whewell, Jones, Malthus, and Quetelet, Babbage was also involved in setting up of the forerunners of the current Royal Statistical Society, Section F of the British Association, and the London Statistical Society (Henderson 1996: 29–33). Babbage counted amongst his friends some of the foremost scientists and thinkers of the day. Hershel, Whewell, and Jones were friends from his Cambridge days. He conducted railroad experiments for Brunel. Ada Countess Lovelace, Mary Somerville, and Dickens admired his work. Babbage's European friends included such luminaries as Laplace, Biot, and Alexander von Humboldt.

In addition to *EMM*, Babbage published two other works that have a bearing on economics: *A Comparative View of the Various Institutions for the Assurance of Lives* (Babbage 1826) and *Thoughts on the Principles of Taxation* (Babbage 1848 [1851]). The former was a popular guide to life insurance written for the benefit of consumers. Although it was translated into German, it is not one of Babbage's more original works (Campbell-Kelly 1994). The work on taxation is more interesting in that it presents a benefit theory of taxation—the main benefits being the protection of property and the protection of the person. Babbage argued that a person's annual income provided an

appropriate basis for taxation since it could be regarded as a measure of the benefit derived from the institutions of society. He maintained that tax should be proportional to income and that exemptions should apply only to those whose incomes were below subsistence level. He also contended that the rate of taxation should be moderate because high rates would provide incentives for tax avoidance and could be accompanied by outflows of capital.

The remainder of this chapter focuses exclusively on Babbage's contribution to economics and is centred mainly on his *EMM*. It will begin with a brief discussion of how Babbage developed the text and of his methodological approach. It then focuses on his contributions in three main areas: division of labour, innovation, and firm size; transactions costs and institutional arrangements; and monopoly.

2 *EMM*—History and Methodological Approach

Babbage's *EMM* was first published in 1832 to great popular if not critical acclaim.² A total of 3,000 copies were sold within two months and three more editions were published by 1835. American editions and French, German, Italian, and Spanish translations soon followed. The book had its origins in a long article which Babbage wrote for the *Encyclopaedia Metropolitana*.³ As Babbage informs us in the Preface to *EMM*, the encyclopaedia article contains the 'substance of a considerable portion' of the present work (Babbage 1832 [1835]: iii). A perusal of the encyclopaedia article bears this out. However, the book contains a far greater number of examples, including some from reports by committees of the House of Commons (ibid.: vi). These reports enabled Babbage to support the principles he had arrived at based on his own observations with the observations of others, thereby providing greater confidence in the correctness of his inductions.

It will be evident from this that Babbage's approach to economic questions was based in the first instance on inductions from observation. This allowed the derivation of general principles from which consequences might then be deduced. Two of Babbage's associates from his Cambridge days, Whewell and

²Richard Jones, who read the draft manuscript, thought it 'a strange collection of facts taken from his [Babbage's] commonplace book' (Schaffer 2003: 284–285). McCulloch's (1833) review in the *Edinburgh Review* can be best described as lukewarm.

³The first version entitled 'An Essay on the General Principles which Regulate the Application of Machinery to Manufactures and the Mechanical Arts' was published in 1827 and did not include any material on the domestic and political economy of manufactures. An expanded version, including the economic material, was published in 1829 (see Babbage 1829).

Jones, were engaged in conscious efforts to create an inductive political economy in opposition to the deductive approach of Ricardo and Mill (Henderson 1996; Rashid 1979; de Marchi and Sturges 1973). Babbage appears to have been sympathetic to their efforts and had a prominent role in the statistical societies that they promoted but he himself made no overt attack on the Ricardians and does not appear to have shared Whewell and Jones's anti-utilitarian sentiments.⁴ Having noted that political economists were often 'reproached with too small a use of facts and too large an employment of theory', Babbage acknowledged that these 'closet-philosopher[s]' knew very little about 'the admirable arrangements of the factory'. However, he argued that the situation could be rectified if manufacturers and merchants supplied the necessary factual data. Babbage recognised that 'erroneous deductions' could be made from recorded facts but he insisted that 'the errors which arise from the absence of facts are far more numerous and more durable than those which result from unsound reasoning respecting true data' (Babbage 1864: 156).

Babbage also had some useful things to say about survey methods. He emphasised the importance of recording survey information either during a factory visit or immediately afterwards. To minimise disruption during the course of a visit, he advocated the use of a list of questions with blanks for the answers, ideally to be drawn up after a first visit to an establishment. Babbage divided his suggested questions into two groups: general inquiries and inquiries relating to specific processes. Under general inquiries, information was to be sought on the composition of output; the number of processes involved in production; the nature and cost of inputs; defects, waste, and quality assurance procedures; the wholesale and retail prices of the final output; the nature, expense, and maintenance of machinery and whether it was constructed in-house or obtained from specialist suppliers; the size of the capital normally required; the main seats of the industry whether in Britain or abroad; the quality of imports compared with local production; the sources of export demand; any duties, excise, or bounties; and whether the manufacturer sold directly to the merchant or to a middleman. Under processes, besides a description of the process itself, Babbage proposed to collect information on the number of operatives required to attend a machine; the skill level, years of apprenticeship, and rates of pay of each; working hours; whether payment was by the piece or the day; whether masters or men provided and repaired tools; and the proportion of output which was defective, what was done with defects and who bore the cost of them. He also sought information on total

⁴ Babbage had friends and associates amongst the reform-minded Benthamites. He even sent his children to a school founded on utilitarian principles (Hyman 1982).

hours in which the process was in operation and the number of times the process was repeated in any given time period and the effect of this, if any, on output and defects. Babbage showed an awareness that being under observation could affect the speed at which workmen carried out operations and recommended the use of cross-checks. More generally, he suggested that questions be shaped so that the answers to some questions served as an internal check on the answers to others (*ibid.*: 114–118).

As with the original encyclopaedia article, the first part of *EMM* is devoted to a discussion of the advantages arising from machinery. Each advantage, together with appropriate examples, is discussed in a separate chapter. There is also a long chapter on copying, which covers processes such as printing, casting, moulding, and stamping—processes in which a large fixed cost lays the basis for low per unit costs, something which was typical of the mass production technologies which were beginning to emerge in Babbage's time (Rosenberg 1994).

Having discussed the advantages arising from the use of machinery in Part I of *EMM*, Babbage turned to the economic aspects. In addition to data on the internal economy of 'manufactories', he emphasised the importance of data on the responsiveness of demand to changes in price. He also noted that even if a manufacturer was not initially motivated by the need to reduce costs, competition would force him to attend to the 'principles of the domestic economy of manufactures' (Babbage 1832 [1835]: 121–122). Babbage's notion of competition was what would now be described as Schumpeterian. Manufacturers were forced to innovate in order to undersell their rivals. A manufacturer who successfully innovated would for a time reap the benefit of his ingenuity but the successful innovation would become generally adopted until it in turn was superseded by more economical methods (*ibid.*).

3 Division of Labour, Mechanisation, and the Size of Manufacturing Plants

When Babbage summarised his contributions to human knowledge in his autobiography, he listed division of labour, cost of verification, principles of taxation, and monopoly as his contributions to political economy (Babbage 1864 [1991]: 327–331). Of these, the contribution to the division of labour is best known, having been referred to by major authors such as Mill, Marx, and Marshall. In the first chapter of *The Wealth of Nations*, Adam Smith had attributed the increase in productivity arising from the division of labour to

three factors, namely the increase in dexterity in every particular workman, the saving of time in passing from one type of work to another, and the invention of machines which facilitate and abridge labour. Babbage accepted that these causes were important, although with some qualifications and extensions. First, he did not regard the increase in dexterity resulting from specialisation as a permanent source of advantage. It was true that specialised workers would increase their dexterity more quickly than non-specialised workers but the latter would eventually reach the same level of dexterity albeit after a slightly longer period (Babbage 1832 [1835]: 173).⁵ Secondly, with regard to the saving of time in passing from one species of work to another, Babbage noted that this factor was likely to be most important in cases where great accuracy was required and set up costs were substantial. Thirdly, with regard to invention, Babbage concurred with Smith that while specialisation facilitated workmen in contriving new tools and simplifying processes, different experience and skills, including extensive knowledge of machinery and the power of making mechanical drawings, were necessary in order to combine the scattered arts into a single machine (*ibid.*: 174–175). While Babbage also noted that the division of labour had the effect of reducing the time required for learning,⁶ he emphasised that there was one additional principle without which an explanation of the cheapness of manufactured articles owing to the division of labour would be incomplete. This was

[t]hat the master manufacturer, by dividing the work to be executed into different processes, each requiring different degrees of skill or of force, can purchase exactly that precise quantity of both which is necessary for each process; whereas, if the whole work were executed by one workman, that person must possess sufficient skill to perform the most difficult, and sufficient strength to execute the most laborious, of the operations into which the art is divided (*ibid.*: 175–176).

Having arrived at this principle, Babbage found that it had already been clearly stated by Gioja in a work published in Milan in 1815. Nonetheless, the principle is generally associated with Babbage (as in ‘Babbage’s principle’ or ‘The Babbage Principle’) and is considered to provide an explanation for the

⁵ Incidentally, Babbage noticed that in factories with an extensive division of labour most of the operations were paid by piece work. This is presumably due to the fact that the work itself is more easily measured, although Babbage does not make this point.

⁶ The length of apprenticeships reflected both the time required for learning and the time required to repay to the master the losses he incurred during the learning process. If the time during which the master made losses fell as a result of specialisation, competition would ensure that the time during which he recouped losses would also fall. The facility of acquiring a skill and the possibility of entering a profitable employment at a young age would encourage entry into the trade with a resulting fall in wages.

deskilling of labour that takes place under capitalism. As Babbage made clear, the principle applies to mental as well as to manual labour. In fact, given his own background, it is possible that the principle was first suggested to Babbage by his knowledge of the system adopted by Prony in the construction of a series of mathematical tables in France. Inspired by a chance reading of chapter I of *The Wealth of Nations*, Prony had introduced a division of labour in which the work of constructing the tables was divided amongst three sections. The first section included some of France's premier mathematicians who decided on the appropriate functional forms; the second were people who had a good acquaintance with mathematics who converted the formulae into numbers; the skill required of members of the third and largest section was the ability to do simple addition and subtraction (ibid.: 194–195). As might be expected, Babbage saw the potential for the mechanisation of the routine work conducted in the final stage through the use of the difference engine (ibid.: 195–201).

Babbage understood that a large output and large capital was necessary in order to be able to purchase and apply to each process precisely the quantity of skill and knowledge which is required for it. Once this optimal level of output was established, factories could produce at the same cost as long as they produced some multiple of this output, though Babbage also understood that new possibilities might emerge as factory size increased.⁷ While the need to attain the precise division of labour that minimised costs was one of the main drivers of increased plant size, other factors such as the need to minimise the cost of transferring material between processes were also important especially where the materials were heavy or delicate or quality could not be easily ascertained (ibid.: 213). The introduction of machinery was also a factor in the increasing size of plants. The first machines devised for the production of patent net (a type of textile) were very expensive and their economical use required 24-hour operation of the plant. Once their profitable operation had been secured, increasing demand for the machines led to improvements and reductions in cost. The need to provide expensive maintenance capability and the desire to fully utilise it created further pressures for expansion as did the setting up of separate accounts, payroll, procurement, and sales departments. Another factor making for an increase in plant size was the production opportunity (economy of scope, in modern terminology) created by a substantial consumption of an item such as gas lighting or the availability of materials as a by-product of the main business of the factory (ibid.: 215–218). Babbage

⁷As Babbage put it: 'The exact ratio which is more profitable for a factory employing a hundred workmen, may not be quite the best where there are five hundred; and the arrangements of both may probably admit of variations, without materially increasing the cost of their produce' (ibid.: 212).

also noted that because of their scale of operations, large plants could afford to use agents to source rare materials from abroad, to conduct surveys into the tastes of consumers in different countries, and to carry out the experiments necessary to invent new products and to improve existing ones (ibid.: 222–223). Finally, Babbage observed that the concentration of production in large factories had led to the elimination of the class of middlemen that had been previously interposed between the maker and the merchant (ibid.: 218–221). As is discussed in greater detail below, the reason for this was that reputational effects become possible in situations of large-scale business.

Babbage's discussion of the advantages of large firms received widespread recognition. Mill drew heavily on it in the early part of his discussion of production on a large and small scale in his *Principles of Political Economy* (Mill 1848 [1973]: 132–135). Marshall drew on Babbage's discussion of division of labour in his *Principles*, while in *Industry and Trade* he discussed the limitations on the practical implementation of Babbage's principle (Marshall 1919: 224–226, 1959: 219–220). Marx's discussion of the factors making for the ever-increasing size of manufacturing establishments also drew on Babbage (Marx 1867 [1977]: 227). In addition, he worked out the implications of Babbage's principle in terms of deskilling and disciplining labour: 'The habit of doing only one thing converts him into a never failing instrument, while his connection with the whole mechanism compels him to work with the regularity of parts of a machine' (ibid.: 330). Marx also attended to the wider sociological implications of the division of labour noting that '[m]anufacture...develops a hierarchy of labour powers, to which there corresponds a scale of wages' (ibid.). In the twentieth century, interest in Babbage's principle was revived when Braverman expanded on Marx's treatment and drew out its implications for the nature of work under capitalism (Braverman 1974).

Babbage was strongly of the view that good factual information was necessary to improve the quality of decision-making, particularly with regard to the introduction of new machinery. Where a breakdown of the costs associated with any process was available, it would provide an indication of where innovation effort was likely to be most profitably employed (Babbage 1832 [1835]: 242–243). Before any new 'manufactory' was set up or a new machine was adopted, Babbage recommended that a full investigation of the associated costs and benefits be carried out. However, he was aware that it was often difficult to estimate costs and that this difficulty increased with the complication of the machinery in question. Estimates suggested that with new machinery the cost of the first machine could be as much as five times that of subsequent copies. There was also the

issue of machine durability. This depended on the perfection with which it was originally constructed and the care taken to keep it in proper repair. Moreover, long before machines actually wore out, they tended to be superseded by improved ones. In circumstances of such continuous improvement, Babbage proposed that a payback period of five years was necessary to make the introduction of any new machine profitable. Given Babbage's awareness of the costs and risks associated with the first introduction of new machinery and indeed his own interest in invention, it might be thought that he would have been a strong advocate of patent protection. But this was not the case. Babbage accepted the importance of preserving to each inventor the sole use of his invention, until such time as he had 'been amply repaid for the risk and expense to which he has been exposed, as well as for the talent he has exerted in completing it' (ibid.: 360–361). However, it was very difficult to frame a law on the issue which was not open to serious objections and, in practice, patents could be very difficult to defend. As a result, some manufacturers no longer regarded a patent as a means of securing a monopoly price but instead '[sold] the patent article at such a price, as will merely produce the ordinary profits of capital; and thus secure to themselves the fabrication of it, because no competitors can derive a profit from invading a patent so exercised' (ibid.: 361).

Babbage accepted that innovators would normally receive adequate rewards through the market mechanism and that, in general, the public were better judges of the merits of any new product than the government would be. However, he believed that this was not the case in the pure sciences. Long intervals frequently elapsed between the discovery of new principles in science and their practical application. This might be because the mechanical arts were not sufficiently developed to allow the application of the new principles at the time of the discovery. It was also the case that the talents required for advancing the pure sciences were not the same as those required for practical applications. Given that those who made advances in sciences could not expect to achieve adequate remuneration through the market mechanism, government support was necessary to increase scientific effort. Other suggestions included curriculum reform in the universities, better remunerated professorships, honours and prizes for discovery, and greater access to public appointments for men of science (Babbage 1830 [1970]: 2–39). Babbage's strongest criticisms were directed at the Royal Society, but he held out hope that the newly formed bodies such as the British Association would provide a platform for greater interaction between industry and science (Babbage 1832 [1835]: 382–383).

Babbage suggested that technological progress would be encouraged by a law permitting limited liability partnerships involving people with moderate amounts of capital and intelligent and inventive workmen (see *ibid.*: 362). Although the remuneration of labour in such partnerships would be linked to the success of the undertaking, Babbage recognised that the discoverer of an improvement to the production process might still receive only a small fraction of the total benefit deriving from it. His remedy was to propose that the discoverer should receive a substantial proportion of the profits brought about by his innovation for a fixed period of time (see *ibid.*: 254–259). Babbage also favoured the export of machinery. In his view, rather than resulting in a loss of British advantage, such exports would increase the class of skilled workmen and allow the benefits of greater specialisation to be achieved. Babbage was confident that technological progress in Britain would continue at a rapid rate and while he accepted that the incremental improvements to a particular innovation eventually grew smaller and smaller, he argued that where the number of machines in employment was already large, the impact of small improvements could be very considerable (Babbage 1832 [1835]: 373).⁸

4 Transactions Costs

As noted already, Babbage regarded his identification of the cost of verifying that an article was what it professed to be as one of his important contributions to human knowledge (Babbage 1864 [1991]: 329–330). He accepted that the classical theory according to which market prices were determined by the proportion between supply and demand and natural prices by the cost of production including normal profits was broadly speaking true. However, there were two circumstances in which this required some modification: (i) where the whole supply was in the possession of one person, and (ii) where the quality of an article could not be determined easily on inspection. The latter tended to give rise to considerable variation in the prices at which exchange

⁸Babbage regarded his recognition that a cause which at first sight might appear to be insignificant could have important consequences as one of his important insights which he highlighted in the introduction to *The Exposition of 1851*. He illustrated this point with the example of a workman raising his shovel an inch or two higher than was optimal. In the course of a day, this would produce a very sizeable difference either in fatigue or in the amount of work done (Babbage 1851 [1968]: 3). In *Industry and Trade*, Marshall commented favourably both on Babbage's example and his generalisation and credited him with having worked out in 'a considerable way' one of the chief ideas of 'Scientific Management' (Marshall 1919: 275–280).

took place. Moreover, the cost of the article to the purchaser was not just the price he paid for it but also the cost of verifying its quality (Babbage 1832 [1835]: 134–135).

Babbage noted that the transactions costs arising from the need to verify quality could be an important determinant of the decision to make or buy a particular product. For example, while it was a general maxim that government could purchase most articles at a cheaper rate than that at which it could manufacture them itself, it might still be more economical to set up its own operations when account was taken of the cost of verifying the quality of each unit purchased and in devising methods of detecting the new modes of adulteration which were constantly resorted to (*ibid.*). Babbage quoted extensively from House of Commons Select Committee discussions of quality issues relating to clover seed, the Irish flax trade, the lace trade, the stocking trade, and the watch trade. Based on these reports and other instances with which he was familiar, he argued that since the majority of purchasers lacked the skill to verify the quality of products, it was necessary for them to pay a higher price to some person with the skill and integrity to furnish articles of the required quality (*ibid.*: 139–140).

Another alleged consequence of poor quality was the collapse of trade. A report investigating the grievances of framework knitters had noted that differences in the quality of lace were not visible to the naked eye but that the inferior product deteriorated rapidly on washing. It was claimed that where the inferior product was sold as first quality, it led to a subsequent collapse of the demand for any sort of lace from the district in question (*ibid.*: 137–138). In watchmaking, it was alleged that the marks and names of respectable manufacturers were forged by producers of inferior goods who sold these in foreign markets, thereby leading to the collapse of orders. Such products were also sold in the countryside by peddlars who had departed by the time the fraud became known.

Having discussed a number of other examples including drugs and harness, Babbage turned his attention to the quality of services. Here he argued that there was scope for fraud in the charging for services, such as the conveying of parcels, because it was difficult for the customer to verify the appropriateness of a charge and it was extremely costly to recover any excess charges that had been paid. For this reason, Babbage suggested that it would be worth experimenting with government conveyance of parcels along the same lines as the post was already conducted (*ibid.*: 144).

Whereas in the *EMM*, Babbage focused on the costs associated with ascertaining quality, in *The Exposition of 1851* (Babbage 1851 [1968]) his focus was on the lack of price information. This arose because the organising committee of the exposition refused to allow prices to be posted on the various exhibits. Babbage argued that this had the consequence of defeating the effects of

competition and imposing search costs in terms of a loss of time on both buyers and sellers. Babbage also claimed that it led to price dispersion, increased prices, and reduced demand (*ibid.*: 67–69).

The evolution of market days and regular markets was also discussed in *The Exposition*. These institutions increased competition and reduced price variability but, according to Babbage, their greatest benefit was in terms of reducing the cost of transacting by saving time for all the participants involved (*ibid.*: 73–74). As more regular markets evolved to facilitate the trade of merchants, larger dealers, and international exchanges, a class of men emerged whose business it was to buy and sell on commission and whose presence provided a means of reducing the cost of obtaining market information. As Babbage explained, in a market with 100 purchasers and 100 sellers, each purchaser has to ask each seller at least two questions, namely what is the price and what quantity have you for sale at that price? This would give 20,000 questions in all whereas if there is a broker in the market each agent provides the information on prices and quantities only to his own broker, making 400 questions in all. Brokers may then meet together to pool information and arrange the necessary exchanges. Babbage was clear, however, that the need for brokers arose because individuals did not have perfect information. Referring to this discussion, Romano (1982: 401) argues that, for Babbage, the establishment of market exchange was facilitated by a reduction in transactions costs. This is certainly true but Babbage also recognised that market exchange could take different forms and that the forms which were successful at any point in time were likely to be those which best succeeded in reducing the cost of transactions.

As noted already, Babbage held that outsourcing was most likely ‘where there exists a sure and quick method of ascertaining that the terms of the contract had been fulfilled’ (Babbage 1832 [1835]: 213). Babbage also noted that the move from cottage to factory production in the calico trade led to the demise of a class of middlemen and to a reduction in the need to monitor the measure and quality of cloth. In the old situation, Babbage argued, each cottager had an incentive to cheat in the absence of monitoring provided that any fraud on his part, even if detected, did not become known to other buyers. However, once the institutional setting became one involving the merchant and a single great manufacturer, there was no longer an incentive to cheat because the parties to the transaction will know that any gains from doing so would be more than counterbalanced by losses arising from the injury to the manufacturer’s reputation (*ibid.*: 218–219):

The value of character, though great in all circumstances of life, can never be so fully experienced by persons possessed of small capital, as by those employing much larger sums: whilst these larger sums of money for which the merchant deals, render his character for punctuality more studied and known by others. Thus it happens that high character supplies the place of an additional portion of capital; and the merchant, in dealing with the great manufacturer, is saved from the expense of verification, by knowing that the loss, or even the impeachment, of the manufacturer's character, would be attended with greater injury to himself than any profit upon a single transaction could compensate (*ibid.*: 219).

Babbage argued that well-grounded confidence in the character of its merchants and manufacturers was an important advantage that an old manufacturing country always possessed over its rivals. As a consequence of this confidence, in some of the largest English towns, sales and purchases on an extensive scale were made daily without any of the parties ever exchanging a written document (*ibid.*: 219–221).

The idea that repeated transactions and the expectation of further dealing could provide a mechanism for ensuring honesty in commerce was not in itself new (Geary and Prendergast 2008). What was new was Babbage's explicit linking of the issue of honest dealing with the use of modern factory-based organisational forms and his idea that the value of reputation was linked to firm size. Babbage also understood that well-grounded confidence in the character of merchants had a wider social value in reducing the transactions costs associated with trade.

Reviewing *EMM* for the *Edinburgh Review*, McCulloch dismissed Babbage's discussion of quality arguing that adulteration was not carried out to the extent that was commonly supposed and that in any case it could not be eliminated by regulatory intervention since the corruption of the regulators was just as likely as the fraud of the vendors (McCulloch 1833: 320–322). Mill was more appreciative of Babbage's contribution. In *Principles of Political Economy* he argued that the labour expended in verifying that people fulfilled their engagements is 'so much withdrawn from the real business of production' (Mill 1848 [1973]: 111). He elaborated to the effect that the support of those who lived by pillaging or overreaching other people was a direct burden on society. The police and the whole apparatus of justice was a second burden rendered necessary by the first. This, in turn, was far outweighed by the immense increase in produce which would be obtained if labourers honestly performed what they undertake 'and by the increased spirit, the feeling of power

and confidence, with which works of all sorts would be planned and carried on by those who felt that all whose aid was required would do their part faithfully according to their contracts' (ibid.). Several of Babbage's examples were then cited both to illustrate the waste occasioned to society through the inability of members to trust one another and, more positively, to show the 'substantial advantage derived in business transactions from proved trustworthiness' (ibid.: 112). Mill, however, neglected to provide any analysis of the institutional basis which helped to sustain trust and confidence. Given Mill's importance to nineteenth century economics, his failure to carry forward Babbage's insights may help to explain their subsequent neglect. It is also possible that the increasing coordination of production by firms and the centralisation and mechanisation of production contributed to the loss of interest in organisational solutions to problems of quality. As Babbage himself noted, one of the greatest advantages of mechanisation was 'the check which it affords against the inattention, the idleness, or the dishonesty of human agents' (Babbage 1832 [1835]: 54).

5 Competition and Monopoly

Babbage was concerned that there was scope for combination of masters against the public in industries such as water, gas, and railroads where the capital required was very large and the number of competitors limited (ibid.: 312–314). He favoured some form of profit regulation in these situations, although he warned that such schemes should only be implemented with great circumspection. In general, however, Babbage took the view that, in the absence of combinations, competition would not be endangered by the presence of large firms. Babbage's view of competition was a dynamic one. He saw overproduction as an almost inevitable consequence of competition. Although this had some unpalatable consequences for both masters and workers, in the longer run, it could be beneficial for all:

When too large a supply has produced a great reduction of price, it opens the consumption of the article to a new class, and increases the consumption of those who previously employed it ... It is also certain, that by the diminution of profit which the manufacturer suffers from the diminished price, his ingenuity will be additionally stimulated; that he will apply himself to discover other and cheaper sources for the supply of his raw material; that he will endeavour to

contrive improved machinery which shall manufacture it at a cheaper rate; or try to introduce new arrangements into his factory, which shall render the economy of it more perfect (ibid.: 232–233).

If the manufacturer succeeded in these endeavours, all parties would benefit. A larger number of people would consume the article and pay a lower price than before and while the manufacturer's own profit on each unit of output would be reduced, this would be compensated for by the larger total output so that his overall rate of profit would be nearly the same as before. Workmen's wages would also return to their normal level. Babbage acknowledged that the occurrence of a glut did not always give rise to new and cheaper methods of production. In such cases, the diminished rate of profits would result in some capital leaving the trade. Ideally, the least efficient firms would leave but firms with superior capital without superior efficiency would be able to support competition for longer and could stay in business with the hope of reimbursing themselves from a higher price when the smaller capitalists had been driven from the market (ibid.: 240).

Babbage was clear that for competitive market pricing to take effect, it was necessary that the whole supply should be distributed over a very large number of smallholders and that there should be a large number of demanders each requiring only a very small quantity. If, on the other hand, the supply was entirely in the possession of one person, he would naturally seek to charge the price that would maximise total revenue. However, in setting the price, the supplier would be guided by the knowledge that an increased price would diminish consumption, and by the desire to realise his profit before a new supply reached the market (ibid.: 143). Although not spelt out in detail, the implication here is that, in normal circumstances, monopoly will only be temporary with high profits attracting new suppliers to the industry in question. This interpretation is reinforced by Babbage's statement in his autobiography that he had arrived at the principle that 'even under the circumstances of the most absolute monopoly, the monopolist will, if he KNOWS his own interest and PURSUES it, sell the article that he produced at exactly the same price as the freest competition would produce' (Babbage 1864 [1991]: 331; upper case in original). In the autobiography, Babbage claimed to have elucidated this principle in an edition of the *Economy of Machinery and Manufactures* prepared as the basis for a new Italian translation of the work. To date, this work has not been found but clues as to the nature of the argument are provided by Babbage's discussion of patent laws referred to earlier in which he argued that rather than regarding the patent as a privilege by which a monopoly

price could be secured, the producers secured for themselves the fabrication of the patented article by selling it at a price producing the ordinary profits of capital.⁹

Romano uncovered a draft letter to Senior written in 1841 in which Babbage states his view that 'the price which will be charged under the most absolute monopoly is, if the monopolist knows his own interest, the same as he would charge under free competition' (Romano 1982: 398). Babbage informed Senior that he had sent him a manuscript covering the case where the supply the monopolist can produce is unlimited, but he did not elaborate further in the letter. He then went on to argue that, in the case of a fixed supply, the price charged will be that which clears the market because even if government intervened to fix the price at a level below this, the supply would be bought up by a class of middlemen who would sell the units of output at the market clearing price and make a substantial profit in the process. Romano has commented that this example does not explain why the monopolist would charge the competitive price under these conditions. Actually, however, Babbage's point seems to be that, in conditions of fixed supply, the competitive price is the price which clears the market so that even in this case there is no exception to his rule. We know that Senior's own theory of monopoly was based on the view that potential competition placed an upward limit on the price a monopolist might charge and that the extent to which potential competition could constrain monopoly power depended on the degree to which all actors had access to the same advantages. The evidence seems to suggest that Babbage's views were along similar lines.

Babbage and Senior were writing at a time when the Industrial Revolution was in full swing and when the main source of competition was the new firm, the new invention, and the new process of production. Babbage's view of competition was a dynamic one as also was that of Senior. Although this view was carried forward by Marx, it was gradually submerged by the neoclassical approach pioneered by Cournot and further advanced by Jevons and to an extent also by Marshall. It was rediscovered in the twentieth century first by Schumpeter and later in the contestability approach which became popular in the later part of the twentieth century.

While our discussion so far has focused on division of labour and innovation, information and transactions costs, and competition and monopoly, it is worth noting that Babbage also made some interesting comments on the localisation of industry which he suggested has the effect of diffusing informa-

⁹Babbage had already arrived at this position when he wrote the long article for *Encyclopaedia Metropolitana*.

tion about markets, raw materials, and modes of working as well as reducing transactions costs. Although the benefits were such that reasons of considerable weight were required to cause the removal of industry, changes did occur in practice as a result of alterations in sources of energy (from water to coal) and the impact of combinations in refusing a reduction in wages or opposing the introduction of machinery (Babbage 1832 [1835]: 225–234). Babbage also thought that the opinion of workmen that their interests were in conflict with those of their employers was erroneous but he admitted that the commonality of interest was not always obvious. He proposed that this could be rectified by the introduction of a system based on two principles: first, that a considerable part of the wages of each person ‘should depend on the profits made by the establishment’, and secondly, that every person ‘should derive more advantage from applying any improvement he might discover, to the factory in which he is employed, than by any other course of action’ (ibid.: 253–254). While Babbage doubted the willingness of large capitalists to embark on such schemes, he cited examples of industries in which profit sharing was practised and suggested that changes in the law to allow limited liability partnerships would enable such schemes to be taken forward. Given Babbage’s enthusiasm for a graduated division of labour involving the separation of high level and routine work, it is interesting that he also believed that forms of organisation that engaged the interest and loyalty of working people would be conducive to the development of the forces of production.

6 Conclusion

Despite the efforts of Stigler (1991), Romano (1982) and Rosenberg (1994) to draw attention to the full richness of Babbage’s work, he continues to be identified almost exclusively with the Babbage principle. Commentators tend to focus on factors making for large plant size and the separation of planning and execution that became the hallmarks of the system of work organisation that dominated workplaces for much of the twentieth century. There is no question that Babbage explained the underlying logic of such ‘Fordist’ production systems which were coming into being in his own time. What is forgotten, however, is Babbage’s recognition that work organisation and overall industry structure was influenced by transaction costs as well as production costs.

While both Mill and Marx drew on Babbage’s work, the economist whose approach was closest to that of Babbage was Alfred Marshall. During his apprenticeship to economic studies, Marshall visited a wide range of

manufacturing establishments in Britain and America. Like Babbage, he tried to ground his economic studies in the reality of economic life and to accommodate a view of competition that preserved the content of actual business behaviour within his partial equilibrium framework. Like Babbage, he believed that any manufacturing operation reduced to uniformity was likely to be taken over by machinery, provided the work was on a sufficient scale. He also shared Babbage's optimism about the scope for continuing technological innovation and its potential to benefit all classes in society. Marshall, however, had some reservations about the impact of scientific management on the operatives themselves, although he acknowledged that it led to increased productivity and higher wages. As he put it, 'the most economic use of a man as an agent of production is wasteful if he was not himself developed by it' (Marshall 1959: 220).

It is well known that Marshall held that economics was an organic whole, the interpenetration of fact and theory. The same has been said of Babbage by no less of an authority than Schumpeter. Babbage, he wrote,

combined a command of simple but sound economic theory with a thorough first-hand knowledge of industrial technology and of the business procedure relevant thereto. This almost unique combination of acquirements enabled him to provide not only a large quantity of well-known facts but also, unlike other writers who did the same thing, interpretations (Schumpeter 1961: 441).

This Baconian unity of theory and practice is Babbage's great legacy to Cambridge economics.

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14

Henry Fawcett (1833–1884)

Bart Schultz

1 Introduction

In the history of economics, there is a fairly common dismissive designation to the effect that the economist in question was an unoriginal follower or popularizer of John Stuart Mill, whose *Principles of Political Economy* (1848) was the truly epochal, paradigm-establishing work falling between Smith's *Wealth of Nations* and Marshall's *Principles of Economics*. Perhaps no one has suffered more from this designation than Henry Fawcett, Professor of Political Economy at Cambridge University (from 1863), Liberal MP (from 1864), and Postmaster General under Gladstone (from 1880). Donald Winch, in his generally excellent *Wealth and Life*, captures something of the overall attitude:

Fawcett may have been present at the British Association conference in Cambridge in 1862 when Jevons gave the first public account of his new theory of value, where it was received 'without a word of interest or belief'. At this time Fawcett was completing his *Manual of political economy*, a popularisation of Mill's *Principles* that was his only academic claim on the attention of the electors to the practically defunct (never properly activated) chair of political economy in Cambridge in the same year. Cambridge connections dating back to his

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undergraduate days and including a fellowship at Trinity Hall, plus Mill's testimonial, played a part in Fawcett's success, though in the end this was mainly due to the way in which his opponents bungled the election by splitting their vote. Fawcett was to occupy this post, chiefly addressing an undemanding, because mainly conscripted, audience composed of those sitting for pass degrees, until his death in 1884 (Winch 2009: 243).

Indeed, Winch's is one of the more balanced and judicious accounts of Fawcett's place in the history of economics. It would seem that at least when it comes to Fawcett's contributions to economics, perhaps the kindest conclusion to be drawn from the overall consensus is that 'his direct, realistic, unpolished attempts to explain the substance and policy implications of elementary economic analysis to non-professionals reached a much wider contemporary audience than the writings of any other late nineteenth-century English professor of political economy' (Deane 1987: 296–297). Even Fawcett's close friend and biographer Leslie Stephen conceded that for Fawcett 'the leading principles of political economy and those which were really valuable, were few, simple, and therefore capable of exposition on the level of average intelligence' (Stephen quoted in Deane 1989: 106). Such views were scarcely apt to excite the minds of the young 1870s Cambridge revolutionaries seeking to establish economics as a serious, independent scientific discipline, included amongst them the likes of Marshall, Cairnes, and Foxwell, or their intellectual descendants who rule the profession today.

Yet, as this essay will seek to demonstrate, the received wisdom on the subject of Henry Fawcett is less than satisfactory, more received than wise. Although a radical revisionary history is not being promised here, some gestures will be made to the effect of rescuing Fawcett from what E.P. Thompson called the 'enormous condescension of posterity'.

Fawcett rose from relatively modest circumstances. His father was a draper who ran a shop in Salisbury and married well, to one Mary Cooper. As Lawrence Goldman has observed, the

Fawcetts were staunch Liberals and their strict party loyalty rubbed off: there is no need to look far for the origins of Fawcett's Liberalism. In the year of the Great Reform Act, William Fawcett was Mayor of Salisbury; eleven years later in 1843, when Cobden and Bright visited the town as they took their anti-protection campaign into the heart of rural England, they stayed with the Fawcetts (Goldman 1989: 1).

As Fawcett's wife would later recall, of his parents and home situation, 'there were, as in most country towns, rigidly defined political barriers between the shops in the city—Liberal drapers, fishmongers, and so on, and Tory drapers, fishmongers, and so on. Mrs. Fawcett would never have dreamed of going into any but those in sympathy with the Liberals' (Fawcett 1925 [1995]: 58–59).

Henry, who was born on 26 August 1833, was by most accounts not a prodigy, but he nonetheless ended up making it to Cambridge University, where he shifted from King's College, to Peterhouse, to Trinity Hall, emerging, to his disappointment, as Seventh Wrangler in the Mathematical Tripos, but nonetheless winning a Fellowship at Trinity Hall (1856). He adored Cambridge, which was to his mind a meritocracy by virtue of the struggles and possibilities afforded by the Tripos system and Fellowship competitions, which built both character and opportunity. His interest in mathematics was, however, surpassed by a lifelong interest in making an impact in politics, in the House of Commons, which meant pursuing a legal career as well. But as Goldman recounts:

Fawcett's career at the bar was cruelly short, terminated by the accident that left him totally and permanently blind—yet an accident that paradoxically changed everything and nothing for him. Ironically, Fawcett had serious problems with his eyes before the accident. In the winter of 1856–7 he was advised to take 'perfect rest' to restore his failing sight, affects by over-work. Then, on 17 September 1858, Fawcett went out shooting with his father, and William Fawcett, with a cataract in one eye failed to see his son in advance of the party. He fired at some partridges and two pellets went through the tinted spectacles that Henry had been advised to wear, blinding him in both eyes. Fawcett apparently once told a friend that it took him just 'one night to decide whether the loss of my sight should make any difference in my life or not; I decided it should not' (Goldman 1989: 3).

The accident did mean that he would enter politics from a position of academic rather than legal eminence; it was from Cambridge that Fawcett 'sallied forth into a series of electoral contests, into various public arena and into the society of Liberal intellectuals and activists' (ibid.: 6). That Mill's *Principles* had so stuck in his head is perhaps not surprising, given that it was the primary work in political economy that he had studied before losing his sight. Mill's works, he explained to Mill himself, 'have been the chief education of my mind' (Fawcett quoted in Stephen 1885: 102), though the writings were very importantly supplemented by a cordial, somewhat pragmatic friendship with Mill that followed their first personal meeting, in 1860, when both were struggling to advance

Thomas Hare's elaborate scheme for proportional representation. They were both members of the London Political Economy Club, and the Mills joined Fawcett's Radical Club in 1870, a club that proudly allowed women members.

Fawcett's blindness is of course one of the first features of him that most commentators single out, referring to him as the 'blind Victorian'. His blindness undoubtedly threw many obstacles in his path, in an era that was scarcely known for leading the way on disability rights (unless one counts Fawcett's own efforts). Yet what is remarkable is just how effective he was in making his way on his own terms. In remembering Fawcett, his younger friend and Cambridge colleague Henry Sidgwick would write:

Just now I think most of the wonderful success and example of this life, which is now beyond the reach of time and change ... He was a hero of a peculiar type, without any outward air of self-sacrifice or suggestion of idealism in his ordinary talk, and yet one felt his determination to live the ordinary life of a man, and a successful man—who gives pity and aid more than he takes it—required a continual sustained effort which did not draw its force from self-love alone; it continually demanded and obtained further force given by the consciousness of the power of serving others; and the needs of this struggle gave to a nature, which, though large, healthy, and generous, was not originally characterized by high moral aspiration, an elevation it would not otherwise have had. In spite of all that I have read of saints and sages, I feel that if grievous physical calamity came upon me, yet one that left the springs of physical energy unimpaired, I should turn for strength to this example. I wonder how many blind feel that he has opened the door of their prison-house and shown them the way back to ordinary life: steep, yet one that may be trodden by a steady and trustful step (Sidgwick quoted in Schultz et al. 1999: 391).

Sidgwick's question was in due course answered by Winifred Holt's *A Beacon for the Blind: Being a Life of Henry Fawcett, the Blind Postmaster-General* (Holt 1914), which demonstrated at considerable length just how effective and inspiring Fawcett's example had been, not least because of his upbeat temperament. Stefan Collini has even wondered how 'the ironical, skeptical, moody Stephen could ever have been so close to the straightforward, emphatic, insufferably cheerful Fawcett' (Collini 1989: 43), and it seems only fair to admit that Fawcett, for all of his 'manly Victorian' robustness, did keep life in perspective in ways that many of his contemporaries failed to do. As Stephen put it, after commenting on Fawcett's unusual combination of shrewdness and kindness:

A spontaneous and intense hatred of everything unfair showed itself in all his most active impulses; whether it took the form of sympathy for the ignorant and depressed agricultural labourer, for the children deprived of the means of cultivating their

intellectual faculties, for women ousted by men from the provinces of labour in which they could achieve independence, for the townspeople shut out from the only places in which they could enjoy healthy recreation, or for the millions of India governed by an alien race too apt to neglect the real interests of the subject or to allow their policy to turn upon totally different considerations. Fawcett was invariably upon the generous side (Stephen 1885: 180–181).

One is strongly tempted to say that on a great many matters, Fawcett saw much more clearly than most of his contemporaries. A radical champion of Darwinism, feminism, and equality between the classes, he would work tirelessly on the side of quite a few of the angels, seeking, with such young supporters as Sidgwick, to open up access to higher education regardless of sex and class. Darwin himself would write to Fawcett: ‘Pray believe that I feel sincerely grateful that you have taken up the cudgels in defence of the line of argument in the “Origin,” you will have benefited the subject’ (Darwin quoted in Holt 1914: 96).

Fawcett was deeply Millian, to be sure, and always leaned heavily towards non-interference by the state, though in an open, evidence-driven way—for him, as Stephen put it, the bottom line was that ‘Government should do what experience proves it can do efficiently’ (Stephen 1885: 160). But he fought for the preservation of public open spaces, and even helped develop the telephone network. There is no little irony in the way that such a Millian classical liberal would ascend to the government position of Postmaster General, and oversee any number of public services. Like the Mills and the Sidgwicks, he also worked in partnership with his wife, setting a concrete example of equality between the sexes.

Fawcett’s wife was the famous suffragette Millicent Garrett Fawcett (1847–1929), whom he married in 1867. They had met in May 1865, at a social gathering, and she had captured his attention by lamenting Lincoln’s assassination and expressing support for the North that would resonate with a Liberal reformer like Fawcett. She would go on to publish *Political Economy for Beginners* (1870), *Tales of Political Economy* (1894), and many essays, articles, and reviews; work with Henry and their reformist circle to found Newnham College, Cambridge (one of the first women’s colleges in England); and play a lead role in the movement for women’s suffrage. After her husband’s death, she would become even more politically active, heading the National Union of Women’s Societies (1897–1918). Curiously enough, her older sister, Elizabeth, who as Elizabeth Garrett Anderson would become Britain’s first woman physician, had also been proposed to by Fawcett in the very same month and year, though she declined out of concern for her career.

Here lies a very deep paradox indeed—how was it that one of the figures associated with the ‘manly Victorians’, with strenuous character building that both the tests competitions at Cambridge and the free market competitions of the world were supposed to advance, could at the same time seek to open up all the manly spheres to women? What did his feminism really look like, and how was it entangled with his views about building the character of the poor for purposes of economic advance? (See Collini 1989; Caine 1994.) Was it problematically masculinized, in effect allowing women equal status only when they became more like the men dominating the public sphere, and less invested in the caring relationships now celebrated by ‘care feminism’? Just how closely did the Fawcetts follow John Stuart and Harriet Taylor Mill on issues relating to the subjection of women, and how did their views on such matters figure in their economic writings? These questions will also be raised in the following section, but it is worth highlighting here some of what Millicent had to say about the Mills, which points up just how close the Fawcetts and Mills were, when it came to feminism:

During the months of the Parliamentary Session which we spent in London, I regarded it as a very great honour when we were invited from time to time to dine with Mr. Mill and his stepdaughter, Miss Helen Taylor, at Blackheath. These were delightful evenings, when we met Mr. and Mrs. Grote, Professor Cairnes, Herbert Spencer, and other celebrities, and heard, I suppose, some of the best talk from some of the best talkers in England. Of course questions concerning Womens [sic] Suffrage and the general position of women not infrequently came up, and I remember a discussion between Mill and Herbert Spencer, the latter taking the ‘anti’ line, and basing his arguments on the heavy handicap nature had imposed upon women. Mill’s reply took my fancy exceedingly. He said, ‘You look upon nature as something we should do well to follow. I look upon nature as a horrible old harridan’ (Millicent Fawcett 1925 [1995]: 60–61).

She was present when Mill (unsuccessfully) ‘moved the Women’s Suffrage amendment to the 1867 Reform Bill; its terms were to omit the word “man” from the enfranchising clause and substitute the word “person.” The speech was a masterpiece of close reasoning, tinged here and there by deep emotion’ (ibid.: 64). Her copy of Mill’s *The Subjection of Women*, presented to her by Mill himself, was one of her most precious possessions. But one of her proudest hours concerned Henry Sidgwick and the founding of Newnham, a deeply Millian cause:

As I am writing now of the early days of Newnham, I cannot forbear mentioning what I have always regarded as an honour, viz. that Professor Henry Sidgwick, the

real founder of Newnham, asked me and my husband to lend our drawing-room for the first meeting ever held in Cambridge in its support. So far as I can remember, this must have been in 1870. We were then occupying a furnished house which possessed a drawing-room of suitable size for such an occasion. I therefore recognize that the birth of Newnham under my roof was more or less accidental; nevertheless, such is human folly, I go on being proud and pleased about it. I know that Philippa was a little baby girl at the time, but was old enough to be brought in at the tea-drinking stage at the end of the proceedings and to toddle about in her white frock and blue sash among the guests (*ibid.*: 72–73).

Happily, Philippa Garrett Fawcett, born in 1868 and the Fawcett's only child, would become both a Lecturer at Newnham and one of Newnham's most famous early success stories—in 1890, when women were allowed to sit for the Tripos, though not officially classed, she triumphed in the Mathematical Tripos, beating out even the official Senior Wrangler and demonstrating to the critics with some finality that women were indeed capable of abstract thought. Part of an anonymous poem in her honour runs: 'Hail the triumph of the corset/Hail the fair Phillipa Fawcett/Victress in the fray/Crown her queen of Hydrostatics/And the other Mathematics/Wreathe her brow with bay' (Phillips 1988: 33).

Had her father but lived to see it, this would have been his most triumphant moment.

In his own life, Henry Fawcett had to settle for a series of lesser triumphs. He was, by all accounts (and admittedly), no wide-ranging, many-sided Millian philosopher. He served as MP for Brighton from 1865 to 1874, when for complicated reasons having to do with his advanced liberalism, he became MP for Hackney, remaining in that position until his premature death from pleurisy, on 6 November 1884. However, he was also appointed Postmaster General by Gladstone in 1880, quite possibly for political reasons, since Gladstone may have wanted to bridle the outspoken Fawcett, who criticized Gladstone on many counts, not least the Irish Question. Still, even as Postmaster General, he proved to be an innovative and unconventional force, introducing such significant innovations as parcel post, postal orders, and savings stamps. Very late in life, in 1883, he was also elected Rector of Glasgow University, but he died before his official installation.

The precise content of Fawcett's advanced liberalism, on economic matters, is best discussed in connection with his writings, since he was really all of a piece, working out his economic arguments and political causes in tandem. His chief writings, in addition to the *Manual of Political Economy* (which went through no less than eight revised editions before 1907), were *The Economic Position of the British Labourer* (1865), *Pauperism: Its Causes and Remedies*

(1871), and *Free Trade and Protection* (1878), though he also co-authored with his wife *Essays and Lectures on Social and Political Subjects* (1872).

2 Politics and Economics

Although Fawcett was in some sense an avowedly orthodox Millian who did much to spread Mill's influence at Cambridge, such a description can be more problematic than it may seem at first blush. As remarked, he had nothing close to Mill's range of interests, and was never counted as a force in philosophy in the way that Mill was. After all, Mill himself evolved and changed his stripes on any number of important counts. Was Fawcett an 'orthodox' follower of Mill's early editions of the *Principles of Political Economy*, or of the later editions, when under the influence of Harriet Taylor Mill, the work sounded a much more 'socialistic' note, celebrated the cooperative movement, and rejected the so-called Wages-Fund theory (or Wage-Fund doctrine) that supposedly doomed the labouring classes to always making gains solely at the expense of one another? He is usually singled out (e.g. by Deane 1987; Kadish 1989) as adhering to the Wages-Fund theory—the rather Malthusian view that any gains to the working class would only be counterbalanced by eventual losses to them—even after Mill himself had abandoned it. But matters may be more complicated. At the very least, one must agree with Stephen that for Fawcett,

the principles of *laissez-faire* commended itself by its nobler aspect. It did not mean, Leave the blind struggle to work itself out, and apply no remedy to the most cruel grievances. It meant, on the contrary, Give free play to all men's intellects and faculties; be exceedingly jealous of all restrictions upon the energies of any class, especially of the poorest class (Stephen 1885: 162).

Indeed, at every turn in the *Manual*, Fawcett shows a deep sympathy with the plight and everyday life of labourers, such that, for example, rather than condemning the allotment system—allowing 'small plots of ground, cultivated by the labourers after they have done their regular work'—as a distraction from work or the improvement of their minds, he maintained that 'the labour which they spend on their allotments is not irksome; in fact, the interest they take in watching the growth of the various vegetables which they plant, is to them a source of great pleasure' (Fawcett 1865 [1995]: 253). The same kindness is displayed in his other works, particularly *The Economic Position of the British Labourer*. This is not to mention the little pamphlets that he laboured over so earnestly, such works as the 1880 'Aids to Thrift', which he hoped would be of real use to the poor themselves.

In his essay on ‘Modern Socialism’, Fawcett explained at some length that

[t]o many, Socialism and Communism are supposed to be synonymous with confiscation and spoliation. A Socialist exists vaguely in the minds of the comfortable classes as a sort of abandoned creature who wishes to live by robbing other people of their property, and who desires to see general pillage introduced...yet nothing can be more unjust than to throw aspersions upon the character of the Socialists, and to misinterpret their motives. They no doubt have been mistaken enthusiasts, but it is impossible to deny that their motives have been pure and their aims lofty. They have been animated by a desire which must have been felt by all who are not depraved by selfishness, to lighten poverty, to alleviate human suffering, and to diffuse more general happiness among mankind. The injustice which is so generally done to Socialists will be perhaps more clearly perceived when attention is directed to the origin of the socialistic sentiment (Fawcett 1872 [1995]: 7–8).

But sympathetic as he may have been, Fawcett was principled, outspoken, honest, and very difficult to control, both politically and academically. The ‘Fawcettites’ in Parliament, such men as Charles Dilke and Auberon Herbert, among others, were the quintessential loose cannons. At Cambridge, he could drive even his friends to distraction, ‘talking Mill’s “Liberty” of the crudest kind at the top of his voice’ (Stephen quoted in Goldman 1989: 9).

Of course, Fawcett did occasionally hedge, politically. As Goldman has noted, in ‘the debate on the second reading of the 1870 Elementary Education Act, Fawcett described himself as ‘a moderate Churchman’. In truth, he was no sort of churchman at all but an agnostic whose attachment to the social virtues of industry and endeavour owed nothing to faith’ (ibid.: 19). Indeed, as his forceful and outspoken opposition to the secularism of higher education showed, he was certainly not one to allow religion to stand in the way of progress.

If one had to single out the leading demands of Fawcett’s political campaigns, the following would emerge: improvement in the conditions of the labouring classes; political and educational equality for women; opposition to primogeniture and landed, aristocratic monopolies; the reform and universalization of educational opportunities and institutions (including the elimination of the religious tests); better public services; elimination of waste and extravagance; preservation of the commons (including Epping Forest); improving the quality of life for the people of India; and the advance of cooperative societies, which was ‘Fawcett’s great panacea’ (Stephen 1885: 166). When compared to the leading claims of his writings on economics, the overlap is nearly perfect.

Fawcett's *Manual*—which underwent significant revisions, especially between the second (1865) and fourth (1876) editions—opens with the perfectly frank admission that it is simply meant as a shorter and more accessible version of Mill's *Principles*, which 'is so complete and so exhaustive, that many are afraid to encounter the labour and thought which are requisite to master it' (Fawcett 1865 [1995]: viii). The first chapter sets out to disabuse the reader of the view that political economy is 'hardhearted and selfish', and that the political economist is one 'who wishes to see everyone rich, but who has no sympathy with those higher qualities which ennoble the character of man' (ibid.: 4). The truth, by contrast, is that the political economist may 'be the most useful of all philanthropists; because a mere desire to do good without any principles of guidance is ever liable to be a futile and misdirected effort' (ibid.). 'Political Economy' is 'concerned with those principles which regulate the production, the distribution, and the exchange of wealth', but it need not ignore 'the other phenomena of man's social existence' (ibid.: 4–5). 'It is therefore a fundamental error to suppose that political economy ever asserts that the higher motives which actuate human actions ought to be discarded in favour of wealth' (ibid.: 6). 'Wealth', in turn, may be defined 'to consist of every commodity which has an exchangeable value' (ibid.: 7), which means that it 'is not determined by the nature and quality of a commodity, but rather by the circumstances in which that commodity may be placed' (ibid.). Thus, for example, 'the social condition of a nation and the state of its civilization determine to what extent natural resources may be classed as wealth' (ibid.: 8). Failure to appreciate such facts was the abiding sin of mercantilism.

From the critique of mercantilism, Fawcett launches into the topics that structure much of the book—'The Requisites of Production', which are land (or 'appropriate natural agents'), labour, and capital. 'Capital' is saved wealth available for the future production of wealth. Labour, which can be either productive or unproductive, calls for careful definition: 'Productive labour is that which either directly or indirectly produces utilities fixed and embodied in material objects' (ibid.: 17–18). This allows that 'the labour of the teacher who imparts skill to the mechanic is productive, for by this skill wealth is created'. Consumption can also be productive or unproductive, though 'even the poorest labourers in this country purchase some luxuries which they could abstain from, without in the slightest degree diminishing the efficiency of their labour' (ibid.: 18).

Thus, it is quite plain from the start that Fawcett is primarily concerned with improving the condition of the labouring class. The great spectre that hung over the effort, according to many recent commentators as well

as earlier ones, was the difficulty of any such quest, the grim Malthusian spectre of workers who multiply with every improvement in their conditions and thus undo their gains in part by flooding the market with more cheap labour. The narrower, more technical version of this was encapsulated in the Wages-Fund theory, which, as Mill put it, in his ‘Thornton on Labour and Its Claims’:

The theory rests on what may be called the doctrine of the wages fund. There is supposed to be, at any given instant, a sum of wealth, which is not regarded as unalterable, for it is augmented by saving, and increases with the progress of wealth; but it is reasoned upon as at any given moment a predetermined amount. More than that amount it is assumed that the wages-receiving class cannot possibly divide among them; that amount, and no less, they cannot but obtain. So that, the sum to be divided being fixed, the wages of each depend solely on the divisor, the number of participants. In this doctrine it is by implication affirmed, that the demand for labour not only increases with the cheapness, but increases in exact proportion to it, the same aggregate sum being paid for labour whatever its price may be (Mill 1869 [2006]: 643–644).

Now, one of the leading points on which Fawcett is pilloried for being an uncritical follower of Mill is precisely this, the Wages-Fund theory, which he supposedly defended even after Mill, late in life, abandoned the doctrine, in the article cited above. Yet it would seem that Fawcett had himself contributed to the very serious qualification of any such view, a point that his younger colleague Sidgwick put with his usual penetrating insight in his article ‘Wages Fund Theory’:

Fawcett (e.g.), *Pol. Econ.*, II. c. ix. (ed. I.), while arguing that combinations of workmen cannot permanently raise wages, affirms that they may do so *temporarily* if they demand an increase when trade is flourishing and profits high. In this passage he implies that this addition to the wages of one set of labourers will not be taken from the wages of another set; and the same conclusion is reached by Cairnes (*Some Leading Principles*, Pt. II. c. 3). But how is this result consistent with the chain of reasoning that we have just been considering? If the total amount of capital is determined by saving, and therefore independently of the haggling of the market, and if the proportion of capital that becomes wages-fund is determined by the character of the national industries, &c., together with the supply of labour, how can any action of any set of labourers (so long as these determining conditions remain unchanged) increase the total wages-fund, as it must do if they raise their own wages without diminishing those of any other

labourers? The only possible answer to this question seems to be that which Mill gives in his review of Mr. Thornton's book 'On Labour.' Although the process of increasing capital is generally voluntary—what we ordinarily call 'saving'—there is no economic law which prevents it from being compulsory; and, in fact, when an employer yields to the demands of a union and raises his workmen's wages, if he finds the money by cutting down his expenditure instead of taking it from a bank or some other investment, he does increase capital in this compulsory way. Whether we choose to call this saving or not is a mere question of words; it is at any rate a process not independent of, but determined by, the haggling of the labour-market.

But if this be so, what becomes of the wages-fund theory? If this compulsory economy be possible at all, why should it not be on the whole successful? In fact, neither Professor Fawcett nor Professor Cairnes really denies this possibility. When they say that combinations of labourers can only raise wages 'temporarily,' they do not mean to assert that the temporary rise will inevitably be balanced by a consequent temporary fall in the wages either of the same or of other labourers; they clearly imply that this will not be the case, if only the combined action of the labourers be wisely directed, and their demands for advances only made when trade is exceptionally prosperous . . . Of course the force thus exerted by any single strike is very slight; but if we make the rather ideal supposition that the whole body of labourers in their several industries are wisely led, and thus never demand an advance unsuccessfully, it is clear that the level of average wages may be steadily elevated by a continual series of slight rises. And if we suppose the movement of wages to take place not in one trade only, but along the whole line of the labour-market, what is there to prevent the compulsory enlargement of the wages-fund from being both rapid and extensive? (Sidgwick quoted in Schultz et al. 1999: 405–406; italics in original).

Thus, as Sidgwick sums it up, the last edition of Fawcett's *Manual*, which 'has even removed the qualification of "temporariness" in some cases', suggests that 'there can be no doubt that labourers can by combining secure a permanent advance in wages', and if this is so, the claim that 'wages depend upon the ratio between capital and population' seems to have lost its chief significance; since, whatever else it may mean, it has not the meaning usually attributed to it, that 'wages depend on saving and not on bargaining' (ibid.: 353). Moreover, Mill had in fact enthusiastically endorsed Fawcett's claims about the labour movement and the emergence of a new form of bargaining. As Simon Cook rightly observes:

Political economists such as Ricardo, and following in his footsteps J. S. Mill, had constructed an account of the natural value of wages with little or no

thought as to the significance of trade union activity. An exception had been Henry Fawcett, who, in his 1860 essay ‘Strikes,’ argued that trade unions could not alter the natural wage rate, but could prolong the period in which market wages remained above the natural rate and could hasten the rise of market wages when they were below the natural rate. Mill’s position in his 1869 review is not free of ambiguity, but he seems to have followed Fawcett’s analysis in his recantation, suggesting that trade unions could influence the market wage but insisting that the orthodox theory applied only to the natural rate of wages (Cook 2009: 159).

Yet for all that, the progress of the labouring classes was not, for Fawcett, best had through the mobilization of the collective bargaining power of labour or strikes. Rather, he followed Mill—or worked in tandem with Mill—in theorizing and advocating on behalf of the cooperative movement, a force that Fawcett took to nearly as soon as it first appeared on the historical scene in England, with the weavers of the Rochdale Pioneers:

During the last few years, the rapid extension of cooperative institutions has excited as much attention as trades-unions and strikes. In the last chapter we had to refer to much that was unsatisfactory and distressing; we, however, discuss the subject of the present chapter with unmixed pleasure, because we believe that wherever the principle of cooperation is carried into practical effect, the labourers enjoy a far more favourable distribution of wealth, and that this advantage is moreover secured to them without the slightest suspicion of the least injustice having been inflicted on any other class (Fawcett 1865 [1995]: 277).

Citing the Rochdale Pioneers as an outstanding, though not entirely problem-free, example, Fawcett extols the virtues of the cooperative movement at length:

The advantages which the working classes derive from a cooperative store are very apparent. In the first place, it provides them with the most eligible investment for their savings. This is most important, because the absence of good opportunities for investing small savings acts most powerfully to increase the improvidence of the poor. Even the middle and upper classes, whose superior education gives them prudence and foresight, are very much influenced in the amount they save by the profit which they believe would be realized on their capital. Hitherto the savings’-bank has been the only investment which, as a general rule, has been open to the working man. Now the ordinary English labourer must make many severe sacrifices to save 50l, and if this amount is placed in the savings’-bank, the labourer obtains an annuity not exceeding thirty shillings a year, as his reward for self-

denial and prudence. If old age or sickness compels him to cease work, his position is scarcely improved at all by the money which he had saved. If he had been improvident, and saved nothing, he would have received parish allowance; but the poor-law guardians perform their duty when they grant just sufficient relief to enable a man to live; the labourer, therefore, who possesses an annuity of thirty shillings a year will obtain from his parish, if he requires relief, thirty shillings a year less than the man who has recklessly spent everything that he has received. Under these combined discouragements, it is not surprising that our labouring classes have been extremely improvident. No labouring man, in fact, has ever had definitely placed before him the prospect that he would be able himself to employ his savings as capital, and enjoy the profits arising therefrom. Our labourers, therefore, could never be cheered with the hope of improving their social position, for they must have seen that at least 99 out of every 100 of those whose parents are hired labourers, always remain in the same condition. Now it is evident that, as far as the investment of money is concerned, such cooperative stores as those we have described afford the labouring classes opportunities for obtaining profits which they never possessed before. We shall moreover presently show, that the cooperative principle, when applied to trade and manufactures, enables the labourer to support his industry with his own capital, and in this manner to rise from the mere status of a hired labourer. The figures we have already quoted sufficiently prove the eligible nature of the investment which is provided by a well-managed cooperative store (ibid.: 283–284).

There were some complications, given the diversity of cooperative enterprises, and the possibility of their degenerating into mere joint-stock companies, but Fawcett was supportive of nearly every such effort, whether it were simply a store or a true, productive cooperative enterprise. Even if the cooperatives seemed to work best when dealing in groceries rather than with, say, clothing, this form of social experiment was to be given every opportunity. The ideal, a realistic one given the examples set in France, would be much more than a joint-stock company or a store, and would, as he put it in *The Economic Position of the British Labourer*, have the associated workers

provide both capital and labour. The whole proceeds of their industry would be their own property; wages and profits would be merged in the aggregate remuneration which they received, instead of being allotted to reward two distinct classes, viz. the employers and the employed. A complete union of capital and labour would thus be established, and this union has been termed Co-operation (ibid.: 74).

Poverty, he urged, could not ultimately be eliminated ‘as long as the labourer simply works for hire ... If the efficiency of labour is to be maintained, and if England is to continue to grow in wealth, happiness, and prosperity, the labourers must participate in the profits yielded by their industry’ (ibid.: 117–118).

Of course, the cooperative movement was seen by Fawcett as precisely the type of movement that could generate social change without reliance on either class strife or governmental interference, and it consequently does not reflect the ways in which he did allow for certain forms of interference. Again, as Stephen has noted, Fawcett ‘did not consider himself entitled to reject any proposal without further hearing on the ground of its incompatibility with some general formula. He asked whether it could or could not be supported by specific reasoning, and in his own arguments he always relies upon definite practical objections’. The ‘crucial test’ that he always applied was simply this: ‘Does the remedy tend to raise or to lower the spirit of self-help?’ The ‘moral injury’ of making the poor more dependent was no benefit at all, whereas prudence and self-reliance were real gains:

A society in which every class does not take its own part is one in which the surviving energy will be oppressed by an ever-growing and ultimately insuperable burden. To call out therefore the energy of all classes, to open the widest field for the application of all their faculties, is the aim which should preside over every genuine effort for social improvement (Stephen 1885: 161).

Under this rubric came calls for a number of interferences that would later be justified in the now familiar terms of public goods and collective action problems. Fawcett was indeed familiar enough with the basic dilemmas, as his plea for the open commons, in *Pauperism: Its Causes and Remedies*, demonstrates:

It is usually urged that whenever an attempt is made to preserve a common the rights of property are seriously encroached upon. Those who reason in this way apparently think that a common is absolutely the property of the lord of the manor and of the commoners; the public is supposed to possess no right in it whatever. In strict legal phraseology this is no doubt true, because the rights of the public cannot be legally defined. But there is this essential difference between a common and land which is owned by private proprietors: every one has a right to wander over the common by immemo-

rial usage, and the reality of this right is at once evidenced by the fact that no lord of a manor can exclude the public from a common by surrounding it with a fence. It is highly inexpedient that Parliament should intervene to facilitate the enclosure of lands, if it can be shewn that these enclosures are now no longer economically desirable, and that they deprive the public of a valuable right (Fawcett 1871 [1995]: 265–266).

But, as he also argued, in ‘State Intervention’:

Those who are most thoroughly indoctrinated with ‘*laissez faire*’ apparently consider that they are bound to oppose compulsory education because it involves State interference. Such opposition affords an instructive warning against the danger of offering too implicit obedience to any general principle. A moment’s consideration will suffice to show that interference on behalf of children and on behalf of grown up persons rest on entirely different grounds. The latter kind of interference may be objected to because it impedes the freedom of men’s actions, is antagonistic to individual liberty, and, in the words of Wilhelm von Humboldt, ‘prevents the harmonious development of the human character.’ The child, however, independently of all Government interference, must be under the control of a parent or a guardian. It is therefore idle to talk of his individual liberty and of his freedom of action; these must be more or less completely surrendered to his parent or guardian. It is therefore evident that the question of State intervention must be regarded from an entirely different point of view when it is applied on behalf of children. It most generally happens that they require the aid of the State when those who are constituted their natural protectors neglect their duty or abuse their power. The extent to which there is such an abuse of power or such a neglect of duty must be the chief element in determining the limits to which it is desirable that the State should extend its protection to children. A child having no power to provide itself with food and clothing, it will be generally admitted that the State ought to take some action if a parent either cannot or will not supply his children with the necessaries of life (Fawcett 1872 [1995]: 33–34).

Of course, his concern with the extension of postal services and the telephone network also involved such calls to resist a dogmatic adherence to *laissez-faire*. He called for, and got, ‘(1) The parcel post; (2) the issue of postal orders; (3) the receipt of small savings in stamps and the allowing of small sums to be invested in the funds; (4) increasing the facilities for life insurance and annuities; (5) reducing the price of telegrams’ (Holt 1914: 271). Strange as it may seem, Fawcett proved himself to be a remarkable public administrator capable of generating a great deal of team spirit in his efforts to bring the advantages of the postal service and the telephone network within the reach of the poor. As Holt noted:

First, he wished to give the machine a *soul* and a heart: the thought of such things in the Post Office seems comic, but in Fawcett's time this miracle was accomplished. Its whole system was waked up, shaken from its lethargy, and flooded with a new interest, and that unusual *esprit de corps*...was aroused among the employees, and alone made possible the results which he achieved. As usual, far ahead of his time, he grasped the chief principles of scientific business management ... Without labeling his principles with high-sounding names, he carried them out, insisting on economy, both of work and fatigue, which produced contentment, increased interest and zeal among the employees; and hence greater efficiency. His method was, first, to diminish fatigue, perhaps the most wasteful factor in quasi-efficient business. Working and sanitary conditions were improved, and the staff of Post Office doctors was augmented (*ibid.*: 254–255).

Importantly, Fawcett also led the way in hiring women to work in the Post Office, including women doctors, an astonishingly progressive manoeuvre at the time. He amply demonstrated in his own practice the effective extension of well-run government services to better serve the less well-off in society, and this without any compunction about failing to privatize such services. These concerns also animated his frequent criticisms of the government for the inept, profiteering financial management of India—calling one egregious example a ‘masterpiece of meanness’—and efforts to advance towards full Imperial citizenship.

But perhaps his most forceful arguments for interference concerned the growth of human capital through universal, compulsory education, a cause that he championed unstintingly, if, by modern standards, in limited terms: ‘[A]lthough I believe that a general system of free education ought to be resisted because it would weaken the sense of parental obligation, yet, in my opinion, no child ought to be permitted to grow up in ignorance because his school fees are not forthcoming’ (Fawcett 1872 [1995]: 43). Indeed, education was his second ‘great panacea’, and its importance was tightly linked to the case for cooperativism:

The main obstacle to the spread of co-operation was the want of intelligence of the classes which most needed it. The labourer who had to go to bed with the sun was cut off from the intellectual influences of the superior artisan. His mental darkness isolated him; he could not take advantage of a rise in wages elsewhere, for he heard nothing of what was going on in regions as strange to him as the remotest part of Australia. The periodical press now acts like a nervous system of the nation, spreading every central impulse to the most distant ramifications of the social body. The man who cannot read or write is out of touch with all the impulses of his day. To raise the educational standard of the labourer, especially in agricultural districts, was therefore a first condition for bringing direct impulses to bear. This, as Fawcett held, could only be done effectually by a national and unsectarian system of education (Stephen 1885: 167).

On this, Fawcett was very straightforward indeed:

A man who is allowed to grow up with his mind entirely neglected has inflicted upon him a grievous wrong; he is cut off from the surest and noblest sources of happiness, and even if he is regarded simply as an agent for the production of wealth, he is made by ignorance comparatively useless and inefficient. An unintelligent labourer is like a machine which works roughly, because no care was taken about the putting together of its various parts, which, perfect themselves, might have been so combined that the machine would achieve completeness in all its operations (Fawcett 1865 [1995]: 115–116).

Sounding the note of human capital theorists, Fawcett continues by explaining how ‘ignorance, by impairing the efficiency of labour, inflicts upon the nation a most serious pecuniary loss’, and in fact also costs it dearly by producing poverty and crime (*ibid.*: 116). If people do not ‘possess sufficient intelligence to enable them to combine and to cooperate for a common object’, the nation suffers, and the national disgrace will continue—‘nearly 130,000 criminals are annually convicted in England and Wales, and whilst one out of every twenty of our population is a pauper’ (*ibid.*).

In this nexus of cooperativism and educational reform working to mutually support one another, one finds Fawcett sounding the notes of the later, more socialistic Mill, the Mill who had influenced Sidgwick as he took a giant step towards the later welfare economics of Pigou, and who had revealed himself as having both socialist sympathies and a green bent. Just so, many of Fawcett’s campaigns and concerns were tied together here, not only cooperativism and education, but also the commons. Commenting on the spoliation of Epping Forest, he wrote, in words that remain timely:

But unfortunately, either through jobbing or blundering, government officials have permitted a very considerable portion of the forest to be appropriated by private individuals. Seldom is government carelessness likely to prove more mischievous; for who can estimate the extent to which the toiling myriads of the metropolis will be socially and morally injured by the loss of such a delightful recreation-ground? How can the health of dense masses of population be maintained, if they can never feel and breathe the air of Heaven unimpregnated by noxious vapours? How can a people continue to be contented and happy, if they can never reinvigorate their exhausted energies by some of the pleasures and amusements which the country can alone afford? And finally let me ask, How can human conceptions be elevated, how can human tastes be raised above mere sordid and worldly pleasures, when there is no opportunity of feeling that inspiration which is derived from the contemplation of the beauties and glories of Nature? ... Let us hope that a warning has been given in time, and that the

government will never again be permitted to barter away, for an insignificant sum, which would be wasted in one useless military experiment, proprietary rights which have a value beyond price, not only to countless thousands who are now living, but which may be still more precious to the millions who in future ages are destined to uphold the industrial fabric of this nation (ibid.: 65–66).

However much he may have opposed the large landed estates, and favoured breaking them up to allow the growth of small, peasant proprietors, this was not because of any lack of concern with preserving open green spaces for the enjoyment of all classes and future generations. In Fawcett's view, the preservation of the commons, the growth of cooperativism, and the growth of agriculture could work together:

As labourers gradually advance they will feel that they have the power and the capacity to raise themselves into an entirely different social condition by forming cooperative combinations; in this way, they will supply the capital which their labour requires; they will thus become their own masters, and enjoy all the profits, which their industry yields. Our rural labourers will rapidly show an anxiety to join the movement by applying the cooperative principle to agriculture; and if in agriculture the movement is checked by artificial restrictions which would tend to prevent these associations of labourers acquiring the necessary area of land, then we may depend upon it that these men will not stay here to enrich us by their industry, and to augment our national greatness by their growing wealth and prosperity; they will seek a home in far distant regions where land is abundant, and where they will prove that, if an Englishman has fair scope for his energy and skill, he will soon raise himself from the poverty by which he may have been depressed; and he will show, that he has that in him, to entitle him to take the foremost place amongst mankind (ibid.: 61).

In these respects, Fawcett was more a part of that change in the atmosphere of Cambridge economics than one would suppose from the slighting accounts of him by his younger contemporaries. Jevons, in the concluding section of his *Theory of Political Economy*, had lamented the 'noxious influence of authority' (Jevons 1871: 265), meaning Mill, Fawcett, et al. (despite Fawcett having written a testimonial for him). Moreover, Sidgwick, in his official academic work, only occasionally refers to Fawcett, however allied they were in political causes. Marshall, Foxwell, and others of the emerging Cambridge School tended to mention him only to scorn, with Foxwell famously writing to Neville Keynes about a less than impressive examination candidate: 'The Fawcetts are at the bottom of most of his errors as invariably happens. The most curious is his almost complete omission of any treatment of interest as distinct from profits—and his very bad theory of wages—both thoroughly characteristic of the Fawcetts' (Foxwell quoted in Deane 1989: 106).

No doubt there were generational tensions that made it difficult to see how much was shared in this Cambridge context. It must also be said that, whatever the merits of Jevons's *Theory* on such matters as marginal, or final, utility, progress in economics may be harder and more compromised than one would like to suppose. Jevons's work, for example, was also steeped in the growing racism of the last quarter of the nineteenth century. Fawcett's work, for all its theoretical simplicity, was at least comparatively free of such racial constructions, and he did not contribute to that part of the atmospheric change.

3 Conclusion

Given the massive changes in the structure of work and the labour market over the course of the last century and a half (Edwards 1980), it may be stretching it to say that Fawcett's account of character building for the poor anticipated in any thorough way more recent trends in human capital theory. But it is nonetheless worth pointing out the parallels between the character traits of independence, self-control, and perseverance highlighted by Fawcett and the character traits of grit, self-control, delay of gratification, and so on, highlighted in recent economic research, such as the work of James Heckman on the importance of early childhood intervention for future life success (Heckman et al. 2014). Radical critics (see Bowles and Gintis 2011) can point as easily to one as to the other, for purposes of claiming that capitalism imposes and rewards certain character formations. Feminist critics can also point to both in highlighting what may be a form of gendering in the literature on non-cognitive skills that reflects a masculinist prejudice in the structure of the labour market (Caine 1994; Collini 1989).

However, in the last analysis, in the case of the Fawcetts and the Mills, it would seem that the accounts of 'manly character' were tied, whether coherently or not, to elements of a feminist care ethics that found expression in the virtues of cooperativism, which balanced collaboration alongside competitiveness and independence (Schultz 2004). Describing the worker who invests in Rochdale, for example, Fawcett noted

the benefits which he derives from this investment are by no means confined to pecuniary profits; he becomes a member of an agreeable artisans' club; he and his family have the use of an excellent reading-room and library; he is thrown into pleasing social relationship with many of his fellow-labourers. A sympathy, a brotherly kindness, and a generosity of spirit are thus engendered, which by elevating the mind and character, give to life its greatest happiness (Fawcett 1865 [1995]: 90).

Indeed,

It is vain to expect any marked improvement in the general economic condition of the country as long as the production of wealth involves a keen conflict of opposing pecuniary interests. The force which ten men can exert may be completely neutralized, if they are so arranged as to contend against, instead of assisting, each other (Fawcett 1871 [1995]: 164).

There is little in the feminism of the Mills, the Fawcetts, and the Sidgwicks that has not been endorsed by such current feminists as Martha Nussbaum (Nussbaum 2004), and curiously enough, many of Fawcett's remarks on non-interference and the commons sound more like Colin Ward than Gary Becker. At the least, the cooperative movement still has a case to make (Curl 2012), and many of the ideas that animated it in the first place are still very much alive in the broader and ongoing concern to combat inequality through greater, more widespread employee ownership of business (Blasi et al. 2013). In any case, if one regards Fawcett as an unusually informed and progressive political figure and activist, rather than as a popularizing economic theoretician, he appears in a much more favourable and brighter light.

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Henry Sidgwick (1838–1900)

Bart Schultz

1 Introduction

Henry Sidgwick, best known today as the author of *The Methods of Ethics* (first edition 1874) and the most philosophically sophisticated of the classical, hedonistic utilitarians, was a many-sided, reluctantly agnostic Victorian intellectual, a moral philosopher, classicist, political economist, political and legal theorist, parapsychologist, educational reformer, literary critic, and much else besides. A product of Rugby, he spent his entire adult professional life at Cambridge University, starting out, after a brilliant undergraduate career there in classics and mathematics, as a Fellow and Lecturer in Classics. After much agonized soul searching in the 1860s, he famously resigned his Fellowship out of a guilty agnostic conscience about having sworn belief in the Thirty-Nine Articles of the Church of England, as the position required. But he nonetheless continued in his then role as a Lecturer in the Moral Sciences and in due course, in 1883—after subscription ceased to be required—rose to the position of Knightbridge Professor of Moral Philosophy, although by that time he was already one of the brightest intellectual stars of the era (Schultz 2004).

During his lifetime, Sidgwick published widely, contributing many essays and reviews to both popular and academic journals, and producing

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The Methods of Ethics, *The Principles of Political Economy* (first edition 1883), *Outlines of the History of Ethics for English Readers* (first edition 1886), *The Elements of Politics* (first edition 1891), and *Practical Ethics* (first edition 1898). A number of important works appeared posthumously—for example, *The Development of European Polity* (1903)—and his widow and brother also published *Henry Sidgwick: A Memoir* (1906), composed largely of Sidgwick's letters and journals (Schultz et al. 1999).

Although in the 1860s he left the Anglican faith of his childhood behind, he always hoped to defend a more rational, theistic religious outlook, and he took an expansive view of philosophy as an effort to go beyond special sciences and piecemeal, technical inquiries to gain a comprehensive vision. If he was inspired by a certain vision of science, it was that of science as a cooperative and collaborative critical inquiry, not science as antireligious, reductive materialism (as in the works of T.H. Huxley). As he put it in his pamphlet on 'The Ethics of Conformity and Subscription', which appeared in 1870:

What theology has to learn from the predominant studies of the age is something very different from advice as to its method or estimates of its utility; it is the imperative necessity of accepting unreservedly the conditions of life under which these studies live and flourish. It is sometimes said that we live in an age that rejects authority. The statement, thus qualified, seems misleading; probably there never was a time when the number of beliefs held by each individual, undemonstrated and unverified by himself, was greater. But it is true that we only accept authority of a particular sort; the authority, namely, that is formed and maintained by the unconstrained agreement of individual thinkers, each of whom we believe to be seeking truth with single-mindedness and sincerity, and declaring what he has found with scrupulous veracity, and the greatest attainable exactness and precision (Sidgwick quoted in Schultz et al. 1999: 14–15).

This attitude was given emphatic expression during his resignation crisis of 1869, but it manifested itself in all of Sidgwick's work and ultimately reflected the outlook that he had absorbed when he first joined the Apostles, the exclusive, secret discussion society that he had been invited to join when an undergraduate at Cambridge. The pure, sincere pursuit of truth, with an intimate group of fellow seekers, a true community of inquiry or the 'companions of Socrates' (Schultz 2004: 510), was an ideal that Sidgwick pursued through one interdisciplinary discussion society after another for the rest of his life, notably The Metaphysical Society and the Synthetic Society. This was

invariably part of a quest to tackle the deepest and toughest questions life could present, philosophical and religious, an effort to grasp the cosmos or find an underlying unity in the various regions of inquiry. However much he may have criticized the idealist metaphysics of his friend T.H. Green and his critic F.H. Bradley, rejecting arguments purporting to show, at least for Green, how the divine spirit or eternal consciousness was reproducing itself in human minds, he was no more in the camp of any form of materialistic utilitarianism than they were, as is evident from his posthumous work *Philosophy: Its Scope and Relations*:

[I]t seems best that the general investigation of the grounds of our belief in such conclusions as are held to be based on experience should be combined with the study of what may be known, or has been thought to be known, by a non-empirical method about mind, matter, and their relations, or about the ‘absolute reality’ that ‘underlies’ or is ‘implied in’ the world empirically known: especially since...the notion of ‘verification by experience’ appears to be inadequately analysed and defined in ordinary thought (Sidgwick quoted in Schultz et al. (1999): 118).

In truth, the poetry of Arthur Hugh Clough and Alfred Lord Tennyson captured Sidgwick’s overall worldview much more effectively than the works of Bentham. He broke with his great utilitarian predecessors Bentham and the Mills on many counts, even while favoring a subtle and sophisticated form of utilitarian ethics. Moreover, he also allowed that he was no more able to defend utilitarianism to his satisfaction than he was able to defend the theism with which he also sympathized.

In 1876, he had married Eleanor Mildred Balfour (1845–1936), the sister of the future Prime Minister Arthur Balfour (1848–1930, prime minister from 1902 to 1905), who had been one of his students. It was in large measure because of his anxious agnostic grappling with religious issues that he worked with Eleanor to help found the Society for Psychical Research in 1882, hoping to find some possible empirical, parapsychological support for the theistic views that he sought to defend. With her and her brothers Gerald and Arthur, and such friends and colleagues as Edmund Gurney, F.W.H. Myers, and William James, Sidgwick devoted endless hours to the investigation of supposed cases of telepathy, hypnotism, mediumship, ghosts, and so on. His investigations did not yield the truth he sought, though psychical research did take some strange turns, and many of those close to Sidgwick, including Eleanor and her brothers, became convinced that he had communicated to them from beyond the grave (Gray 2012: 7–103; Gauld 1968).

Together, Henry and Eleanor also helped found Newnham College, Cambridge, one of England's first colleges for women (McWilliams Tullberg 1975). Sidgwick's feminism has been subjected to some scrutiny (Caine 1994; Collini 1989; Walker 1998), but much of the criticism of it has been too narrow and uninformed by consideration of his larger sexual politics (Schultz 2004, 2005a). In any event, Sidgwick had a profound and lasting influence on education at Cambridge generally, having worked diligently not only for women's access to higher education but also to change various structures of financing and governance and to professionalize academic life. Indeed, he supported an amazing array of educational causes, ranging from Working Men's Colleges to Extension Courses.

Clearly, Sidgwick's influence went beyond Cambridge and beyond education, and his reformist activities were wide-ranging and not tightly tied to party loyalty. As an 'Academic Liberal', he tended toward a Millian liberalism, which he did not find fully realized in William Gladstone, with whom he would break over Home Rule for Ireland. In later life, he could vote more as an independent, sometimes with the unfortunate prejudices (and racism) of the new imperialism of the late Victorian era (Schultz 2004, 2005b). He was also a supporter of the Ethical Culture movement, hosting Felix Adler at Cambridge and participating actively in various progressive Ethical Societies. He served on a number of government commissions, and even considered running for office, but preferred to act in the role of public moralist and well-placed political adviser.

Sidgwick's intellectual circles included an amazing range of influential figures, from George Eliot, William James, and F.W.H. Myers to A.V. Dicey, Sir John Seeley, James Bryce, Gladstone, and, of special importance, John Addington Symonds, the erudite cultural historian, poet, and man of letters who was a pioneer in the serious historical study of same-sex love, and a founding father of what would become gay liberation. Many of Sidgwick's closest friends, such as Symonds, Roden Noel, and H.G. Dakyns, were devoted to the celebration of same-sex love on an ancient Greek or Hellenic model (Dowling 1997; Schultz 2004). Moreover, Sidgwick's sister Minnie has recently emerged as an important exemplar of Victorian/Edwardian same-sex practices (Schultz 2004; Bolt 2011), despite her life as the wife of Edward White Benson, the Archbishop of Canterbury from 1883 to 1896.

But of course, Sidgwick's circles, especially his Cambridge circles, also included such influential economic thinkers as Henry Fawcett, John Neville Keynes (and later his famous son, John Maynard), Herbert Foxwell,

A.C. Pigou, and, especially, Alfred Marshall. He was unquestionably a central figure in the early ‘Cambridge School’ of economics, particularly after the 1883 appearance of his *The Principles of Political Economy*, though he regularly published works in this field from the late 1870s on. In 1885, he was elected president of the Economics and Statistics Section of the British Association; he also contributed a number of important pieces to the first *Palgrave’s Dictionary of Political Economy*, advised his University on financial policy, and occasionally appeared as an expert witness for various government committees investigating questions of economic policy (he developed considerable expertise devising intricate taxation schemes).

Sidgwick’s interest in economics and political economy was long-standing, dating back to his undergraduate years, when he was profoundly influenced by the work of Mill. ‘Mill’s influence’, he explained, led him ‘as a matter of duty’ to ‘study political economy thoroughly, and give no little thought to practical questions, social and political’ (Sidgwick and Sidgwick 1906: 36). In the early 1860s, he was absorbed in the study of Mill’s classic work, *Principles of Political Economy*, and much influenced by Mill’s Cambridge disciple Fawcett, the blind political economist. Sidgwick’s work as an academic political economist and policy adviser put him in touch with many of the leading economists of the day—not only his Cambridge colleagues but also such figures as Walras, Wicksteed, Senior, Cairnes, Edgeworth, and Jevons. He was even familiar with the work of Karl Marx, at a time when scarcely any trained economists (except Marshall) even knew who Marx was.

Moreover, he was long and intimately involved with the work of the Cambridge Charity Organization Society, which worked independently to provide outdoor poor relief (as against the indoor relief of the poorhouses created by the Poor Laws of 1834) in cases where it was deemed appropriate. In that capacity, Sidgwick developed both a robust sense of the facts of poverty and the sophisticated, eclectic theoretical approach to the issue of poor relief, this being evident in his introduction to P.F. Aschrott’s *The English Poor Law System*, in which he wrote:

I conceive, however, that few persons of experience in England would regard it as within the limits of a probable forecast of the future that either self-help, voluntary or compulsory, or private charity, however systematized and supervised, should enable us to dispense with poor relief obtained from taxes. For a long time to come, at any rate, we shall have to work with a combination of all three methods. But it seems by no means unlikely that either or both of the former methods may be considerably extended; and in order to judge of any

movement in either direction we require a full and intimate knowledge of the actual working of the poor law, which must necessarily be materially affected by any such movement (Sidgwick 1888: viii).

Regarding both his economics and his ethics, it is a mistake to suppose that Sidgwick was a sterile academic theorist hostile to empirical inquiry and removed from the world of practical experience. He appreciated the value of both analytical/deductive and historical/inductive approaches to economic and political theory.

Sidgwick died, from cancer, on 28 August 1900, and during the first half of the twentieth century, his reputation faded, in part thanks to the growing influence of his students and fellow Apostles, G.E. Moore and Bertrand Russell, commonly regarded as the founders of analytical philosophy (Schultz 2004). But since the 1970s especially, interest in his work has revived, and he has often been celebrated as the most philosophically astute of the great classical, nineteenth-century utilitarians, and as a model of academic ethical philosophizing, given the careful, systematic, critical comparison of competing ethical theories through which he develops the utilitarian arguments. There is a frequently quoted remark by C.D. Broad, one of Sidgwick's successors to the Knightbridge Chair at Cambridge, that runs, 'Sidgwick's *Methods of Ethics* seems to me to be on the whole the best treatise on moral theory that has ever been written, and to be one of the English philosophical classics' (Broad 1930: 143). In recent years, Broad's claim has been endorsed and elaborated by such prominent philosophers as Derek Parfit, in his epochal *On What Matters*:

In the *Methods*, as Broad claims, 'almost all the main problems of ethics are discussed with extreme acuteness'. And Sidgwick gets very many things right. He gives the best critical accounts of three of the main subjects in ancient and modern ethics: hedonism, egoism, and consequentialism. And in the longest of his book's four parts, he also gives the best critical account of pluralistic non-consequentialist common sense morality. Though Sidgwick makes mistakes... he does not, I believe, make many. These facts make Sidgwick's *Methods* the book that it would be best for everyone interested in ethics to read, remember, and be able to assume that others have read (Parfit 2011: volume 1: xl).

There is clearly a great deal of Sidgwick in Parfit's claim that we have good, irreducible normative reasons to act in certain ways and desire certain things—his case for a nonreductive and nonnaturalistic but nonmetaphysical and nonontological form of cognitive intuitionism or rationalism. But the effort to ground something closer to Sidgwick's favored hedonistic utilitarianism on such a

metaethical ground is given better expression in Katarzyna de Lazari-Radek and Peter Singer's *The Point of View of the Universe: Sidgwick & Contemporary Ethics*:

[W]e have followed the main lines of Sidgwick's thinking about ethics, and tested his views both against our own reasoning and against the best of the vast body of recent and current philosophical writing on the topics he addresses. The overarching question we have sought to answer is whether Sidgwick's form of utilitarianism can be defended. In most respects we believe it can be (de Lazari-Radek and Singer 2014: 364).

The brilliant reconstructive efforts of Parfit and de Lazari-Radek and Singer are surely vitally important for an informed appreciation of both Sidgwick's philosophical ethics and his economics. Yet even so, if Sidgwick is reduced strictly to such terms, much is missed. Again, his worldview was deeply colored by Cloughian and Tennysonian religious anxieties, and his attitude was more pervasively and genuine agnostic, religiously and philosophically, than his recent academic philosophical reception would suggest. One finds in his life and work a number of paradoxes and surprises, many of which are especially relevant for grasping his economic views.

2 Ethics and Economics

Most accounts of Sidgwick's influence in the history of economics do seek to frame his political economy in the context of the ethical claims of the *Methods*, and it is consequently important to address that work.

The revival of interest in the *Methods* later in the twentieth century owed much to J.B. Schneewind's *Sidgwick's Ethics and Victorian Moral Philosophy* (Schneewind 1977). Schneewind underscored Sidgwick's debts to Kant, Joseph Butler, Samuel Clarke, and Aristotle, and to the intuitionism of such Cambridge moralists as F.D. Maurice, John Grote, and William Whewell, the Master of Trinity College when Sidgwick was an undergraduate there. This historical contextualizing of Sidgwick compellingly demonstrated just how much Sidgwick had appropriated from nonutilitarian sources and how the *Methods* was not meant to be primarily a defense of utilitarianism, but an impartial inquiry into the merits of rival 'methods' for determining what it is right to do, of the leading ethical contenders, at that particular historical juncture. Moreover, Schneewind rightly recognized that like the Cambridge moralists, Sidgwick was trying to find in moral experience some basis for religious belief, even if he was much less optimistic about his results.

Sidgwick primarily focused on three methods of ethics—rational egoism (one ought to promote one’s own greatest good), rational benevolence or utilitarianism (one ought to promote the greatest good of all), and commonsense or intuitional morality, which encompasses such familiar duties as promise-keeping and truth-telling, and the more refined versions of these found in the works of such philosophers as Whewell, author of the *Elements of Morality* (1845) and Mill’s great rival. Much of the *Methods* attempts to show not that commonsense morality is altogether wrong or misguided but only that it is too vague, rough, inconsistent, and incomplete, depending on utilitarian calculations to refine and clarify its rules, resolve moral dilemmas, and cast it in a more systematic, scientific form. For Sidgwick, it seemed clear on reflection that there were utilitarian exceptions to the rule of, for example, veracity, such that it would be ethically allowable to ‘speak falsely to an invalid’ (Sidgwick 1907: 316) to avoid a fatal shock. But just what degree of nonfatal shock veracity might demand also remains obscure without appeal to utilitarian considerations. The same holds for benevolence, justice, good faith, and other duties—so much so that in the end ‘the Utilitarian estimate of consequences not only supports broadly the current moral rules, but also sustains their generally received limitations and qualifications’ (ibid.: 425). In cases of

marked diversity of moral opinion on any point, in the same age and country, we commonly find manifest and impressive utilitarian reasons on both sides: and...the remarkable discrepancies found in comparing the moral codes of different ages and countries are for the most part strikingly correlated to differences in the effects of actions on happiness, or in men’s foresight of, or concern for, such effects (ibid.: 425–426).

Given the way in which commonsense morality initially presents its precepts as self-evident, the task, for Sidgwick, takes the form of demonstrating that those precepts are only apparently—not truly—self-evident. As he put it to the intuitionist Henry Calderwood, if

I ask myself whether I see clearly and distinctly the self-evidence of any particular maxims of duty, as I see that of the formal principles ‘that what is right for me must be right for all persons in precisely similar circumstances’ and ‘that I ought to prefer the greater good of another to my own lesser good’: I have no doubt whatever that I do not. However, prior to undergoing the reflective process described in the *Methods*, I could not always have made this distinction; and I believe that the majority of moral persons do not make it: most ‘plain men’ would probably say, at any rate on the first consideration of the matter, that they saw the obligations of Veracity and Good Faith as clearly and immediately as

they saw those of Equity and Rational Benevolence (Sidgwick quoted in Schultz et al. 1999: 565).

Genuinely self-evident claims, on Sidgwick's account, must be (1) clear and precise, (2) able to withstand careful critical reflection, (3) consistent and coherent with each other, and (4) capable of generating a genuine consensus of experts, since serious disagreement among experts would necessarily diminish our confidence in the apparently self-evident maxim in question. These criteria might be thought of, Sidgwick sometimes suggests, as criteria for reducing the risk of error, rather than for establishing final truth. In any event, his epistemological or 'philosophical' intuitionism (not to be confused with the intuitional morality of commonsense) is highly fallibilistic and only Cartesian in a limited sense.

The sustained examination of commonsense or intuitional morality takes one from consideration of particular acts to commonsense duties to this more refined intuitionism, where the better axioms are more abstract than immediate guides to action: that the good of one is no more important than the good of another, that future good is as important as present good, that it is right to promote the good generally, and that what is right for one must be right for anyone similarly circumstanced. As that last suggests, although Sidgwick rejected Kantian, intuitionist, and idealist appeals to free will, he did not dismiss Kantianism in toto but instead argued that the idea of universalizability was consistent with utilitarian (or egoist) content.

In this way, Sidgwick's intuitionism is indeed the intellectual ancestor of Parfit's 'non-metaphysical non-naturalist cognitivism', recognizing objective, value-based reasons for acting and desiring (Parfit 2011). For both, the notion of 'right' or 'ought' is *sui generis*, normative all the way down, rather than reducible to any natural property or properties, and this without resort to subjective, emotive, or expressive accounts of ethical 'truth', since these all miss the ways in which ethical judgments are simply not about one's psychological states or the expression of them, but concern what is 'really right' for all minds. Sidgwick also rejected not only the epistemology of the earlier utilitarians, but their philosophical psychology as well, agreeing with Joseph Butler that 'psychological egoism' (that people were by nature driven exclusively by self-interest) was simply false.

Even Sidgwick's hedonistic interpretation of ultimate good as pleasurable consciousness was distanced from that of his utilitarian predecessors. That pleasurable consciousness is the ultimate good is, he urged, an informative, non-tautological claim—one can after all question whether pleasurable consciousness really is good, and it takes some argument to show that

the hedonistic standard is better than excellence, perfection, or virtue at sorting out and weighing one good against another. One must ask, say, whether the life of virtue or excellence would still recommend itself if it were invariably conjoined to extreme pain (Crisp 2011: 26–44). For all that, Sidgwick's subtle hedonistic account of pleasurable or desirable consciousness did make it patently clear how difficult yet unavoidable interpersonal comparisons of utility could be. This conception would form the basis for Edgeworth's attempted 'hedonometry', or quantification of hedonism, revived in recent years by Kahneman and others (Edgeworth 1881 [2003]; Kahneman 2013).

Although Sidgwick sided with hedonism as the best account of the Good on balance, he nonetheless brought out many acute difficulties with the felicific calculus—perhaps, most importantly, the problems with maximizing happiness when not dealing with a fixed population, such that 'average' and 'total' utility calculations might diverge, which would raise serious difficulties for population policies regarding future generations. Moreover, he recognized, not only that the general happiness might 'be more satisfactorily attained if men frequently act from other motives than pure universal philanthropy' (Sidgwick 1907: 413), but also how a consistent utilitarianism might take a still more indirect, even self-effacing, form, allowing at the extreme the justifiability of a wholly esoteric morality, such that the utilitarian standard would be best met by having most, perhaps even all, people thinking and acting in terms of different ethical principles, for example, religious or commonsense ones. Bernard Williams (1982) suggested that Sidgwick, unlike other act utilitarians, might on this basis develop a consistent 'two level' system of moral thinking—that is, he might be a 'Government House' utilitarian who would have the colonial rulers reasoning by the utilitarian method, but the colonized kept to a more simplistic method, such as commonsense morality. No doubt Sidgwick would have agreed with de Lazari-Radek and Singer (2014) in thinking that an esoteric morality would in all likelihood only be justifiable in a less extreme form, given all the good that self-conscious act utilitarians have contributed to the world. Still, in some passages of his ethical and political work, he does make it all too clear that he was, like his times, too apt to think in terms of 'inferior races' needing the beneficent influence of European civilization (Schultz 2004: 509–668).

Just where Sidgwick ultimately came down on the value of commonsense morality, practically and epistemologically, has been a matter of considerable controversy. In practice, he often did his best to treat commonsense sympathetically, and to avoid upsetting the ethical and religious beliefs of ordinary people, persuaded that humanity had not yet evolved to the point where it

could happily do without these—all the while steadily working to initiate progressive changes. The same perspective informed his economic thinking.

Powerful as Sidgwick's rehabilitation of utilitarianism surely was, he nonetheless felt that he had failed to give it a sound intuitional grounding because the method of rational egoism could not ultimately be reconciled with the other apparently self-evident principles grounding utilitarianism. Thus, as he put it in 'Some Fundamental Ethical Controversies', along with

(a) a fundamental moral conviction that I ought to sacrifice my own happiness, if by so doing I can increase the happiness of others to a greater extent than I diminish my own, I find also (b) a conviction—which it would be paradoxical to call 'moral', but which is none the less fundamental—that it would be irrational to sacrifice any portion of my own happiness unless the sacrifice is to be somehow at some time compensated by an equivalent addition to my own happiness (Schultz et al. 1999: 483).

Each of these convictions has as much clarity and certainty 'as the process of introspective can give' and each also finds wide assent 'in the common sense of mankind' (ibid.). As the early editions of the *Methods* put it:

For, if we find an ultimate and fundamental contradiction in our apparent intuitions of what is Reasonable in conduct, we seem forced to the conclusion that they were not really intuitions after all, and that the apparently intuitive operation of the Practical Reason is essentially illusory. Therefore it is... a matter of life and death to the Practical Reason that this premise should be somehow obtained (Sidgwick 1877: 468–469).

A theistic principle of cosmic justice, perhaps achieved in the afterlife, afforded the best chance of harmonizing own good and the good of others, but that, of course, was precisely what Sidgwick's other inquiries had failed to yield.

There is surely something decidedly peculiar in the way that arguably the greatest work in the utilitarian tradition elaborately revises, refines, and reconciles utilitarianism with intuitional morality only to knock it apart at the end, leaving it much clarified but nonetheless as unjustified as it had been in Mill's work. Many of Sidgwick's admirers find him a bit strange on the topic of the dualism, even while allowing that he was on to something important. As Parfit explained in his reformulation of the key point:

When one of our two possible acts would make things go in some way that would be impartially better, but the other act would make things go better

either for ourselves or for those to whom we have close ties, we often have sufficient reasons to act in either of these two ways (Parfit 2011, volume 1: 137).

But Sidgwick was more given to entertaining the bleaker possibilities suggested by the dualism, and he was forever haunted by his inability to fully defend a unitary position. For him, the dualism was no mere philosophical representation of collective action problems, but rather a cosmic calamity, suggesting that reason is an illusion and the Universe absurd.

In a deep sense, Sidgwick wanted it all—the strength of utilitarianism and the strength of egoism harmonized, such that doing the right thing would always comport rather than conflict with self-interest well understood, and the norms of a sensible commonsense morality on which people would often act would have that double-barrelled justification (Edgeworth 1877 [2003]; Schultz 2004). Small wonder that he was haunted by his failures.

3 Political Economy and Economics

Sidgwick's importance for economics has too often been underestimated in part because of his difficult and ambivalent relationship with Alfred Marshall, commonly regarded as the founder of the Cambridge School, both for his scholarly work and for his eventual success in establishing economics as an independent academic discipline (Kadish 1989; Groenewegen 1995; Deane 1987; Howey 1965; Schultz 2004; Backhouse 2006; Cook 2009). Marshall and Sidgwick, once they both became powerful 'University Politicians' in the 1880s, were at times fractiously at odds with each other. Sidgwick resisted some of Marshall's efforts to hive off economics (making it more specialized and mathematical) from the history of economics, politics, and philosophy, and urged that economics should remain integrated with such studies, as in the Moral Sciences Tripos. Marshall feared Sidgwick's opposition, especially his talent for effecting compromises that were difficult to resist. But Marshall was the one who had made matters tense almost immediately upon his return to Cambridge (from Bristol) and appointment as Professor of Political Economy, in late 1884, following the death of Fawcett. In a famous incident, Marshall let fly at a startled Sidgwick, chastising him for his obsessive 'over-regulation' and his 'failure to attract men on a large scale' (Sidgwick and Sidgwick 1906: 395), the way Green had at Oxford. Marshall charged that Sidgwick was hampering his efforts, despite his, Marshall's, greater knowledge of economics. For his part, Sidgwick, sounding a note of esotericism, concluded that:

[F]eeling that the deepest truth I have to tell is by no means ‘good tidings,’ I naturally shrink from exercising on others the personal influence which would make men [resemble] me, as much as men more optimistic and prophetic naturally aim at exercising such influences. Hence as a teacher I naturally desire to limit my teaching to those whose bent or deliberate choice it is to search after ultimate truth; if such come to me, I try to tell them all I know; if others come with vaguer aims, I wish if possible to train their faculties without guiding their judgements. I would not if I could, and I could not if I would, say anything which would make philosophy—my philosophy—popular (*ibid.*: 394–396).

Yet Sidgwick had known Marshall for a long time, and the well-known friction between them must be kept in context. In the 1860s, Marshall had come to know Sidgwick’s friends H.G. Dakyns and J.R. Mozley, and he had been taken up by Sidgwick as a fellow member of the illustrious ‘Grote Club’, a remarkable Cambridge faculty discussion group that also included John Grote, W.K. Clifford, John Venn, F.D. Maurice, and James Clerk Maxwell. Sidgwick had tutored Marshall in philosophy and stimulated his interest in educational reform. He even nudged him more in the direction of political economy (and away from pure mathematics), and as Marshall blossomed in that area, Sidgwick enthusiastically sought to call attention to the importance of his work, privately circulating drafts of parts of *The Economics of Industry*. Marshall had also been recruited for the cause of women’s higher education, though he later opposed it, despite his marriage to Newnham graduate Mary Paley.

Marshall would ultimately admit that Sidgwick played a vital role in shaping both him and his Cambridge context, eulogizing Sidgwick as his ‘spiritual father and mother’ (Backhouse 2006: 40). As many have observed, Marshall was too often only willing to give Sidgwick credit for the ‘art’ side of his political economy, calling the third book of Sidgwick’s *Principles* by ‘far the best thing of its kind in any language’ (Marshall quoted in Whitaker 1996: 442). But he did at times express his admiration for a broader ethical orientation they shared:

[W]hat impressed me most was his notion that he was not at liberty to do what he liked with his own—with his faculties, or his money—the notion that he held all in trust: and allied to this was his notion that the University held all that it had—its strength and resources—in trust: that we are not at liberty to play chess games, or exercise ourselves upon subtleties that lead nowhere (Marshall quoted in *ibid.*: 441).

If Marshall would eventually win the fight to establish a separate Economics Tripos (in 1903, after Sidgwick’s death), it is much less plausible to claim that his objections to Sidgwick’s qualified version of hedonistic utilitarianism

have triumphed (de Lazari-Radek and Singer 2014). Marshall's objections to utilitarianism—for example, that a more evolutionary and Hegelian approach was called for without the fixation on the set end of pleasurable consciousness—were neither original nor spelled out in a compelling way, though it should be noted that Simon Cook, in an important and provocative work, has plausibly argued that 'at the root of nearly all of Marshall's disagreements with his liberal Cambridge colleagues stood his commitment to a form of romantic metaphysics' (Cook 2009: 47).

Such differences were probably deeper than, say, the supposed differences between Sidgwick and Marshall over the mathematical formalization of the discipline, an issue on which Sidgwick took a typically hedged and qualified position. After allowing that in a 'certain sense the affirmation is incontrovertible, since one of the principal aims of economic science is to determine the relations of varying quantities', he went on to explain:

The only disputable point is how far it is necessary or expedient to represent these quantitative relations by mathematical symbols or diagrams. The answer must obviously vary to a great extent with the complexity of the reasoning to be represented. Some quantitative deductions are so simple that it would be pedantic to express them otherwise than in ordinary English; some are so elaborate that it would be a *tour de force* to follow them without the aid of the technical language of mathematicians. Between the two comes an intermediate class of reasoning for which the use of mathematical symbols or diagrams is, on the one hand, not indispensable, while, on the other hand, it is troublesome to persons who have not had a mathematical training ... If, whilst giving an important place to the deductive method, I do not adopt a formally mathematical treatment of economic problems, it is because I am of the opinion that the deductions really useful, in the present state of our empirical knowledge, fall within this intermediate class in respect of elaborateness and complexity (Sidgwick 1901: 52).

Of course, there was much more to Sidgwick's basic approach, which reflected the same attitude that went into the construction of the *Methods*. In good Apostolic fashion, he was anxious to draw truths from all sides, but much clarification was called for just by way of stage-setting the issues, an exercise that was valuable in itself. Thus:

Before attempting to make the common notion of value clear and quantitatively precise, it may be useful to explain my general view of the work of definition, which will occupy so large a space in this part of my treatise. For, in spite of all that has been written, by authors of deserved repute, on the place of Definition in Economic Science, it still seems to me that this introductory

part of the study is rarely treated from such a point of view as would enable us to derive the maximum of instruction from it. The economists who have given most attention to the matter seem to me commonly to fall into two opposite errors at the same time. They underrate the importance of *seeking* for the best definition of each cardinal term, and they overrate the importance of *finding* it. The truth is,—as most readers of Plato know, only it is a truth difficult to retain and apply,—that what we gain by discussing a definition is often but slightly represented in the superior fitness of the formula that we ultimately adopt; it consists chiefly in the greater clearness and fullness in which the characteristics of the matter to which the formula refers have been brought before the mind in the process of seeking for it. While we are apparently aiming at definitions of terms, our attention should be really fixed on distinctions and relations of fact ... Hence in comparing different definitions our aim should be far less to decide which we ought to adopt, than to apprehend and duly consider the grounds on which each has commended itself to reflective minds. We shall generally find that each writer has noted some relations, some resemblance or difference, which others have overlooked; and we shall gain in completeness, and often in precision, of view by following him in his observations, whether or not we follow him in his conclusions (Sidgwick 1901: 59–60; italics in original).

Not for Sidgwick was the view, so common among more positivistically inclined social scientists, that one just needed to quickly fix one's operational definitions and get on with the research.

The same attitude is evident in his *Elements of Politics*, a work that serves as a vital companion to his *Principles*. Importantly, both works proceed on admittedly utilitarian assumptions, bracketing many of the concerns raised in the *Methods*. Sidgwick took the utilitarian outlook to reflect the considered convictions of advanced civilization, and both the *Principles* and the *Elements* elaborate the utilitarian side of his vision, without dwelling on his cosmic anxieties about the fate of practical reason. Both works are in part descriptive and in part normative, candidly proclaiming their utilitarian framework with respect to the latter. Although Sidgwick certainly recognized the pervasive and powerful role that a narrower form of material self-interest played in economics and politics, he also recognized, with Marshall, that 'economic man' was a bare abstraction that scarcely captured the many dimensions of human nature, particularly in its historical development. Like Marshall, Sidgwick recognized the endogeneity of preference formation as a matter of course.

To be sure, both the *Principles* and the *Elements* took a strong anti-paternalistic intermediate principle as the starting point for analyzing economic and political policy, such that

what one sane adult is legally compelled to render to others should be merely the negative service of non-interference, except so far as he has voluntarily undertaken to render positive services; provided that we include in the notion of non-interference the obligation of remedying or compensating for mischief intentionally or carelessly caused by his acts—or preventing mischief that would otherwise result from previous acts. This principle for determining the nature and limits of government interference is currently known as ‘Individualism’ ... [T]he requirement that one sane adult, apart from contract or claim to reparation, shall contribute positively by money or services to the support of others I shall call ‘socialism’ (Sidgwick 1919: 42).

But the narrative thread provided by the principle of anti-paternalistic individualism is only that—it is neither a self-evident starting point nor a final conclusion, but rather an effective and historically relevant means for organizing the arguments. In Sidgwick’s scheme, it could, after all, at best be only an intermediate principle, any justification for which would need to draw on the larger utilitarian framework. Moreover, in both the *Principles* and the *Elements*, this principle is so worked over by larger utilitarian considerations that one can see why the twentieth-century libertarian Friedrich Hayek concluded that Sidgwick paved the way for the more socialistic policies of the ‘New Liberalism’, Fabianism, and other welfare statist policies of the early twentieth century (Hayek 1960).

Now, it may seem extravagant to suggest that Sidgwick’s work in philosophical ethics and political economy carries important implications for recent efforts to challenge and reform the economics profession, as in the work of Project Syndicate, and that in economics as in philosophy he might continue to inspire, but a case can be made. In economics, too, it seems that Sidgwick’s star has risen again. There have been a number of interpretive waves in the reception of Sidgwick’s economic work, ranging from dismissing him as a minor and undistinguished Millian, to crediting him with being the source of both the formal hedonometry advanced by Edgeworth (1877 [2003], 1881 [2003]) and the welfare economics of Pigou, to celebrating him as a precursor to Gary Becker and the Chicago School of law and economics. Indeed, with each wave, the appreciation deepens. But it remains safe to say that his *Principles of Political Economy* has still not received the scholarly attention that it merits, and that more interpretive waves would be welcome, particularly in so far as they might probe more deeply the implications of the *Methods* for rational actor theory in general. After all, as is evident from the previous section, Sidgwick’s work as a whole presents a vision of truly *rational* action that is very far from the Humean, instrumentalist account still favored by

so many economists, since he held that our reasons for action are genuinely cognitive and not mere noncognitive expressions of preference, desire, or attitude. Moreover, his ethical work outlines an account of indirect utilitarianism that has the norm-guided action of individuals, as much as individual utility maximization in market contexts, serving the utilitarian end of maximal societal happiness. Such was the role of commonsense moral rules. It was up for utilitarian calculation just how often individual utilitarian or egoist decision-procedures, in market and nonmarket contexts, would in fact yield the utilitarian end of the greatest happiness. An esoteric morality may or may not, depending on historical circumstances, accord a large role to self-interested market transactions.

Also, although Sidgwick could be as pessimistic as the next economist (and his moments of cosmic pessimism went well beyond the dismal science), his work was nonetheless part of a growing effort to shake off the bleaker visions of Malthus along with the extreme schools of economic individualism. Too many of the stock narratives of economic history, for example the relevant parts of Sylvia Nasar's informed and engaging *Grand Pursuit* (2011), do not capture the subtle ways in which a figure such as Sidgwick harbored hopes for a better futurity for the poor and working classes, and this despite the hard times of the first Great Depression and the influence of the so-called Wages-Fund theory (or Wage-Fund doctrine), which had it that there was no possibility of increasing aggregate working-class income, since the increases for some would be at the cost of others. Labor and the poor were supposedly doomed, to put it bluntly. But for Sidgwick, the times were changing, and in part changing in the way Mill had changed, as part of the 'unmistakable drift towards Socialism in Western Europe' (Sidgwick quoted in Schultz et al. 1999: 236). Mill himself had late in life rejected the Wages-Fund theory, and his posthumous *Chapters on Socialism* may have shocked some of his less informed followers, but for Sidgwick, as he put it in 'The Economic Lessons of Socialism', 'we were all, I think, conscious of having received from him a certain impulse in the Socialist direction: we have at any rate ceased to regard the science of Political Economy as opposing a hard and fast barrier against the Socialistic conception of the ideal goal of economic progress' (Sidgwick quoted in *ibid.*: 243).

Still, the common elements of the early Cambridge School are not always easy to tease out. Although Sidgwick was, more than Marshall, fairly caught up in the heated methodological debates of the time, balancing both analytical and historical approaches, and although he tended to stress the continuity of his work with the older schools, he was, as much as Marshall, abreast of the so-called 'marginalist revolution':

[A]s Jevons had admirably explained, the variations in the relative market values of different articles express and correspond to variations in the comparative estimates formed by people in general, not of the *total* utilities of the amounts purchased of such articles, but of their *final* utilities; the utilities, that is, of the last portions purchased (Sidgwick 1901: 83; italics in original).

As he put it in a letter to Foxwell, from 21 November 1886:

I accept Jevons' doctrine of 'final utility' as in the main true, and as an important addition to the older theory: but I am not prepared to say that the modification thus introduced into the theory of value as expounded (e.g.) by Mill is enough to make me regard Jevons' doctrine as a new basis. But I am quite content to be described in general terms as a follower of Jevons (Sidgwick to Foxwell quoted in Schultz et al. 1999: see correspondence).

Indeed, in the Preface to the first edition of the *Principles*, Sidgwick had allowed that after his debt to Mill, 'I believe that I owe most to Jevons's *Theory of Political Economy*, the leading ideas of which have been continually in my thoughts' (Sidgwick 1883: v). It is worrisome—not that many historians of economics have actually worried about it—that Sidgwick might also have been influenced by the racist sentiments of that work.

What seems more puzzling is rather just where Sidgwick went with these new developments. No doubt his personal commitment to utilitarianism clearly and admittedly informed his economic views, especially when it came to the 'art' of political economy, which concerned the normative considerations that came to be called 'welfare economics'. Backhouse (2006) has argued that Sidgwick's emphasis on method, attentiveness to empirical evidence, and balancing of analytical and historical analysis marked important points of overlap with Marshall on the descriptive side as well, and it is clearly true that the two men influenced each other (Cook (2009) helps fill in the story of the early years of their relationship). Whatever differences there were in their conceptions of mathematicizing their Millian inheritance, neither Sidgwick nor Marshall was enthusiastic about mere exercises in formal modeling. Sidgwick's 'Economic Method' of 1879 is sharp in its point:

Indeed, what economist affects to foretell, by any method whatever, 'the exact rates at which goods of every kind will be sold'? Mr. Leslie lays great stress on the 'failure of deductive economists,' as represented by the late Mr. MacCulloch, to recognise duly the local inequalities of wages in England. But who now takes MacCulloch for a sure guide? Or who is so 'deductive' as to ignore a point that has

been enforced and illustrated for half a generation in all the successive editions of Professor Fawcett's manual? (Sidgwick quoted in Schultz et al. 1999: 303).

He was even more emphatic in a letter to Foxwell, from 16 March 1881, explaining that he hated 'the error of presenting economic doctrine as a mere tissue of barren abstractions and confident but imperfectly verified Generalisations' (Sidgwick to Foxwell quoted in Schultz et al. 1999: see correspondence).

In arguing for the significance of Sidgwick's contribution to the Cambridge School, and the value of his careful discussion of terms, Backhouse (2006) persuasively develops the claim that Sidgwick, drawing on Jevons, 'drew a clear distinction between wealth as it is conventionally measured (the sum of goods valued at market prices) and the wealth that is relevant for welfare economics (the sum of individuals' utilities) ... [P]rices correspond to marginal utilities, whereas the utility derived from consumption of a good is related to its average utility' (ibid.: 22). This move helped set the stage for the treatment of welfare economics as distinct from economics in general, marking a departure that allowed for and meant that, as Backhouse puts it, 'total utility would depend on the distribution of resources between individuals. Diminishing marginal utility implies that redistributing resources from the rich (for whom marginal utility is low) to the poor (for whom it is high) would increase utility. Greater equality will thus raise the sum of utilities even if the value of total consumption remains unchanged' (ibid.).

Wealth maximization and welfare maximization were, in short, not equivalent. Analogous arguments are evident in Sidgwick's scrupulous and skeptical account of the possibilities for comparing wealth in cross-cultural or trans-historical contexts (an area Marshall treated in terms of consumer surplus). Sidgwick explained how these two types of wealth could differ, as in the case of those goods for which no price was paid. People value such goods, but, their price being zero, they are not counted as 'wealth' under the first definition. For Backhouse and others, the radical, redistributivist potential implications of such views were evaded by both Sidgwick and Marshall:

Marshall is much more aware of the quantitative side of the problem than is Sidgwick...but no nearer a way to thinking quantitatively about how to achieve the best use of resources. They share both a philosophical viewpoint that inclines them towards egalitarianism and a conservatism that will not risk any interference with incentives, lest output be reduced (Backhouse 2006: 33).

Sidgwick probably was, as Backhouse suggests, too impressed by alarmist claims that, incentives being crucial to production, communism would

lead to splendidly equal destitution; there can be no doubt about his calling for extreme caution in developing socialistic reforms, whatever sympathies he may have harbored for, say, Henry George's claim that the community was the rightful owner of increases in land values. Indeed, as Backhouse has long insisted, it was Pigou, rather than Marshall, who most openly reflected the 'radical side' of Sidgwick's welfare economics.

Of course, one largish paradox in Sidgwick's role concerns the way in which Pigou, Marshall's hand-picked successor and author of *The Economics of Welfare*, could be so much more Sidgwickian in his approach (O'Donnell 1979; Tribe 2014). The paradox is seemingly heightened by the supposed way in which Sidgwick, on Steven Medema's account, forms a 'Bridge from Bentham and Becker' (Medema 2007), as though Sidgwick was a pivotal figure in both twentieth-century welfare economics and the Chicago School that overthrew it in the work of Frank Knight, Ronald Coase, and so many others. But Medema, who has done much excellent research on Coase and his predecessors, has produced a major and most welcome appreciation of the significance of Sidgwick in economic history that succeeds in demonstrating just how fertile the Sidgwickian influence was. In his insightful work *The Hesitant Hand: Taming Self-Interest in the History of Economic Ideas*, Medema has developed at some length his sophisticated narrative of Sidgwick's historical role:

The theory of market failure brought analytical refinement to a centuries-old concern with the impact of self-interested behavior on economic activity, and...we attempt to shed some light on the transition from the fairly non-interventionist approach of the classical tradition to the more interventionist orientation that came to characterize neoclassical welfare theory and public economics. The argument here is that this transition occurred via a two-stage process, in which John Stuart Mill and Henry Sidgwick were central players. The first step involved the elaboration of a greatly expanded theory of the failure of the system of natural liberty—akin to what we today call 'market failure'—as against the classical success story. Mill was instrumental in this expansion, and it continued at the hands of Sidgwick. The second stage involved the move to a much more markedly positive assessment of the possibilities of corrective policy actions undertaken by the state than we find in the classical tradition, and it was here that Sidgwick took center stage. All of this fed into Pigovian welfare theory, the market failure aspect of which, at least, came to dominate professional discourse (Medema 2009: 28–29).

As Medema rightly insists, one corollary of the marginalist revolution was a more refined and systematic account of market failure, and in this, Sidgwick led the way, ascribing 'an even greater set of failings to the system of natural

liberty than did Mill’ and expressing ‘a great deal more optimism about the efficacy of government intervention’ (ibid.: 53).

Medema does, to be sure, note Sidgwick’s many worries about government intervention—corruption and catering to special interests, waste, problems with regulation and supervision, the costs in taxes, and the want of appropriate incentives among government workers. But as he also observes, ‘Sidgwick suggested optimism—that in the long run “moral and political progress [in society] may be expected to *diminish* the extent and severity of the shortcomings associated with government intervention”’ (ibid.: 49; italics in original).

Medema’s account is compelling and accords quite well with the account of Sidgwick’s economic contribution given by Donald Winch in his rich, more historically contextualized work *Wealth and Life: Essays on the Intellectual History of Political Economy in Britain, 1848–1914*. Winch notes how, although both Sidgwick and Marshall stressed the role of education in helping to overcome poverty and economic inequality, Sidgwick appears to have gone beyond both Mill and Marshall in setting out on full display the various areas of market failure, the limits of laissez-faire, and the limitations of economic analysis in general, whether descriptive or normative (Winch 2009). Sidgwick, on this account, was being more categorical than Mill ever was when he wrote: ‘[I]t seems to me quite possible that a considerable extension of the industrial functions of government might be on the whole advantageous, without any Utopian degree of moral or political improvement in human society’ (Sidgwick 1883: 530).

Both Medema and Winch demonstrate in different ways how both the *Elements* and the *Principles* take the laissez-faire principle of individualism—again, that ‘what one sane adult is legally compelled to render to others should be merely the negative service of non-interference, except so far as he has voluntarily undertaken to render positive services’ (Sidgwick 1919: 42)—only as a unifying thread, on which to string a very long list of needed qualifications and exceptions: education, childcare, poor relief, disease control, countering depravity, public works or goods (the famous lighthouse example, but also pure research and defense), environmental regulations, collective bargaining, treatment of future generations, and others. Sidgwick highlights two extreme cases that are especially effective at pointing up the limitations of economic individualism where rational actor assumptions manifestly fail to apply—the ‘humane treatment of lunatics, and the prevention of cruelty to the inferior animals’ (ibid.: 141). Such restrictions hardly aim at securing the freedom of the lunatics or the animals, but are rather ‘a one-sided restraint of the freedom of action of men with a view to the greatest happiness of the aggregate of sentient beings’ (ibid.: 142).

Indeed, these are only the most conspicuous of the many difficulties with a principle that betrays a naive faith in ‘the psychological proposition that every one can best take care of his own interest’ and the ‘sociological proposition that the common welfare is best attained by each pursuing exclusively his own welfare and that of his family in a thoroughly alert and intelligent manner’ (ibid.: 146). The same points apply to, for example, the treatment of children, another area in which paternalism and/or maternalism is clearly required. Like most utilitarians, Sidgwick recognized that the good of all sentient creatures and of future generations should count in the utilitarian calculus, though it was plain that the good of the mentally ill, nonhuman animals, and future generations was scarcely going to be covered merely by protecting the libertarian rights of presently existing people.

To be sure, Sidgwick was never dogmatic about such matters, especially when treating the exceptions that arose even in purer cases, ‘in a society composed—solely or mainly—of “economic men”’ (Sidgwick 1901: 403), when markets failed in their own terms at the efficient production of material wealth. Again, as Winch notes, Sidgwick included here

the accepted range of public goods such as lighthouses, bridges, and harbours, and went on to deal with other topics that would furnish the heartland of what later became known as ‘welfare economics’, those examples of market failure attributable to externalities, neighbourhood effects, and indivisibilities. Prominent among the examples chosen by Sidgwick were those connected with the natural environment (afforestation and flood or disease control); conservation (regulation of fishing and hunting where voluntary agreement was likely to break down), and public utilities (natural monopolies, activities that promised only long-term social returns but were unremunerative to private agencies); and cases such as roads where collection of tolls could detract from their utility to the public (Winch 2009: 226–227).

But Sidgwick was also eloquent on the benefits of protection of ‘infant industries’ and other forms of protectionism. Of course, in keeping with a utilitarian outlook on such matters, on which the contingent facts of the particular case rather than any abstract principle of natural rights or duties must determine the best course, Sidgwick allowed that not all such failures called for or could be resolved by government intervention, only that such divergences of private and public interests meant that government involvement must be treated ‘as not merely a temporary resource, but not improbably a normal element of the organization of industry’ (Sidgwick 1883: 414).

Such wording was telling, characteristic, and indicative of the deeper drift of Sidgwick's arguments. True, as Coase has pointed out in his classic essay on 'The Lighthouse in Economics', Sidgwick's wording in the case of the lighthouse example of a public good was much more circumspect than that of later economic writers (e.g. Paul Samuelson). As Sidgwick put it: '[I]t may easily happen that the benefits of a well-placed lighthouse must be largely enjoyed by ships on which no toll could be conveniently imposed' (Sidgwick 1901: 407). On Coase's gloss, 'this does not say that there may not be circumstances in which the benefits of the lighthouse are largely enjoyed by ships on which a toll could be conveniently laid and it implies that, in these circumstances, it would be desirable to impose a toll' (Coase 1988: 191). Such circumspection, the unwillingness to lay down general rules about intervention or nonintervention, along with Sidgwick's utilitarian accounts of 'property, contract, tort, and, especially, criminal law' (Medema 2007: 30), was what led Medema to claim that Sidgwick's work reflects 'themes and perspectives that have since come to be associated with Chicago law and economics' (ibid.), and this while still recognizing his vital role in the Cambridge School of welfare economics.

But as Backhouse, Medema, and Winch allow, Sidgwick could on occasion be more forceful. In his Presidential Address to the British Association, he proclaimed: '[T]he absolute right of the individual to unlimited industrial freedom is now only maintained by a scanty and dwindling handful of doctrinaires, whom the progress of economic science has left stranded on the crude generalizations of an earlier period' (Sidgwick 1904: 176). Also, however much Sidgwick's critics may have complained that it was difficult to catch the drift of his arguments given all the qualifications heaped on, the recent scholarship cited above has made it much clearer just what that drift was. As Medema sums it up, Sidgwick's

Principles of Political Economy reveals an extensive list of divergences between private and social interests, both where laissez-faire's wealth-maximizing results are not in society's best interest because there are 'extra-economic considerations' that are more important than wealth, and where laissez-faire does not even generate the wealth-maximizing result ... He was concerned that orthodox political economy had 'often produced a blind confidence in the economic harmony resulting from natural liberty,' to the point where it even seemed to neglect to note the opposition between the monopolist's interests and those of the community (Medema 2007: 23–24).

Moreover, Sidgwick made it plain enough in his practical work that he believed there were few reasons to oppose taxes on luxury goods, or efforts to

develop a more eclectic model of poor relief, possibly introducing some of the statist elements found in the German model (Schultz et al. 1999).

Sidgwick was ultimately persuaded that the growth of federalism and large-scale state organizations was likely to continue, though he doubted that the social sciences were anywhere near to discovering actual laws of historical development. He could see the point of the radical positions of Marx, William Morris, and George Bernard Shaw, even if in the end he found them intellectually wanting, as speculative and inexact as the positions of Auguste Comte and Herbert Spencer. He was a gradualist, not a revolutionary, but he was a gradualist who entertained some remarkably radical possibilities for the future.

Of course, other scholars have highlighted additional contributions or confusions in Sidgwick's work, and it is worth recalling some of those. In a careful study, Melitz (1963) has argued in detail that Sidgwick's account of international trade was not as original as claimed on the subject of comparative costs, since he only rejected some points in Mill's account rather than the background theory. Blaug (1985: 461) suggested that the *Principles* may have been the first work 'to question the traditional idea that technical change is necessarily capital-using'. Interestingly, Stigler (1982: 41, fn. 11), very much part of the Chicago School, when discussing the significance of Edgeworth, Sidgwick, and Marshall in giving currency to the work of Cournot and Dupuit on monopoly and oligopoly, confessed that he was 'coming to admire Henry Sidgwick almost as much as the other two. His *Principles*...has two chapters (bk II, ch. IX and X) which are among the best in the history of microeconomics, dealing with the theories of human capital and noncompetitive behavior'.

The chapters that were singled out by Stigler are indeed illustrative of Sidgwick's careful, meliorist approach to descriptive economic analysis. They come near the end of the second part of the *Principles*, which is divided into books on production, distribution and exchange, and the *art* of political economy. Both suggest serious limits to the scope of economic forecasting. As Chapter IX concludes:

[T]he possessors of capital, real and personal, as well as persons endowed with rare natural gifts, are likely to have—by reason of their limited numbers—important advantages in the competition that determines relative wages; in consequence of which the remuneration of such persons may—and in England often does—exceed the wages of ordinary labour by an amount considerably larger than is required to compensate them for additional outlay or other sacrifices; such excess tending to increase as the amount of capital owned by any

individual increases, but in a ratio not precisely determinable by general considerations (Sidgwick 1901: 338).

Chapter X is ‘an investigation of the conditions under which self-interest will prompt to combination, and of the extent of gain which the persons combining may realize’ (ibid.: 340), and it provocatively argues—in line with the late Mill—that in many ordinary cases ‘it is possible for a combination of workmen to secure, either temporarily or permanently, a rise in wages’ (ibid.: 353) without such gain having ‘any manifest tendency to be counterbalanced by future loss’ (ibid.), it being only the bankrupt Wages-Fund theory that would suggest otherwise. More generally, and skeptically, it is

[O]nly in a partial and subordinate way that Economic Science can offer assistance in dealing with the practical problem presented to Boards of Conciliation or Courts of Arbitration when they attempt to avert or close a controversy between employers and employed in any industry as to the rate of wages. Economic science cannot profess to determine the normal division of the difference remaining, when from the net produce available for wages and profits in any branch of production we subtract the minimum shares which it is the interest of employers and employed respectively to take rather than abandon the business and seek employment for their labour and capital elsewhere (ibid.: 356).

Thus, the specter of the more Malthusian early Mill has clearly been banished, and the horizons for labor opened, though Sidgwick, very much like Mill and Fawcett, feared the strife between labor and capital and hoped that new forms of economic organization, such as the cooperative movement, might reduce the risk of violent class conflict without requiring state intervention:

And many who are not socialists, regarding the stimulus and direction of energy given by the existing individualistic system as quite indispensable to human society as at present constituted, yet feel the moral need of some means of developing in the members of a modern industrial community a fuller consciousness of their industrial work as a social function, only rightly performed when done with a cordial regard to the welfare of the whole society,—or at least that part of it to which the work is immediately useful. From this point of view great interest attaches to the development of what is called, in a special sense, ‘co-operation,’ by which the conflict of interests—either between producers and consumers, or between different sets of workers engaged in the same productive industry—has been more or less subordinated to the consciousness of associative effort for a common good. Any experiment of this kind that is economically successful is to be welcomed as a means of education in public spirit, no less than for its more material advantages (ibid.: 590–591).

But of course Sidgwick was rather more sober about the possibilities afforded by the cooperative movement. As he famously put it in ‘The Economic Lessons of Socialism’:

[W]hile the earlier Socialists were much disposed to experiment, their experiments were mostly such palpable failures that their only effect was to harden the orthodox economist in his prejudices as well as his sound conclusions. It is true that the success of the artisans’ co-operative stores—and, in a much more limited degree, of attempts at co-operative production—may be partly set to the account of Socialism; as, without the impulse given by Owen to the co-operative movement, the venture of the Rochdale Pioneers would probably never have been made. But the successes of these co-operative stores, though they have taught us something worth knowing, have not taught the lesson that Socialists have desired to teach: they have not demonstrated the great capitalist or great employer to be superfluous, but only that competition does not tend to the most economical supply of the services of the ordinarily humble and struggling retail tradesmen of the poor.

The tendency of the later school has been to discourage all voluntary essays in Socialism: on the pretext that no instructive experience can be gained except through the action of the State. From a scientific point of view this attitude is to be regretted, but I can quite understand that it is politic in those who aim at producing an immediate and far-reaching movement in a Socialistic direction: since a study of the broad results of previous experiments of the kind certainly does not tend to encourage such a movement (Sidgwick quoted in Schultz et al. 1999: 249–250).

Thus, true to form, and as the last paragraph above indicates, Sidgwick brought his trademark skepticism to bear on the cooperative movement as well, and his attitude was suggestive of the changing times. Much as he admired the movement, he could not share Fawcett’s extreme enthusiasm for it, or shake the belief that the currents of history were rendering its role less significant and that the task ahead would demand the exploration of more statist alternatives.

Still, whatever his anxieties about economic socialism, Sidgwick was openly enthusiastic about ethical socialism, the possibility of humanity growing more altruistic and compassionate, regarding their labor as their contribution to the common good. He was under no illusions whatsoever, not only about the market failing to reflect claims of desert or merit but also about the limitations of that abstraction, ‘economic man’, since historical and cultural or national context could dramatically alter the possibilities for moving beyond economic individualism, though again, in some areas, this emphasis on ‘national

character' lent itself to the racist and imperialistic tendencies of the British Empire (Schultz 2004, 2005b). It is remarkable that Sidgwick's drift toward racism may have been in significant part a result of his absorption in the work of Jevons, who held that a 'man of lower race, a negro for instance, enjoys possession less, and loathes labour more; his exertions, therefore, soon stop' (Jevons 1871: 177). Such views on matters of race, which were more extreme than Mill's, would increasingly reverberate through late nineteenth-century political economy, finding expression in Sidgwick's admirer Edgeworth, who would insist on how '[c]apacity for pleasure is a property of evolution, an essential attribute of civilization', with 'civilization' being determined in part by race (Edgeworth 1881 [2003]: 78).

Even so, Sidgwick found himself quite alienated from the jingoist, militarist political rhetoric of the 1890s. He feared the growth of a materialistic, Machiavellian form of self-interested power politics, whether domestically or globally. Against the ruder and cruder forms of self-interest—aggravated by a practical reason in chaos—he sought a continued moral and political evolution, led by the 'Concert of Europe', toward a greater federalism and cosmopolitanism that would generate the institutional mechanisms and political morality needed to avoid war and avert other forms of strife (Schultz et al. 1999: see correspondence). Ethical socialism, the fostering of humanity's sympathetic and cooperative tendencies, was the road he favored in all regions, and education, in a broad sense, remained the vehicle of reform to which he would ever return.

Indeed, the *Elements*, with its liberal elitist rather than populist democratic perspective, was perhaps most eloquent when calling for the state to encourage a class of teachers who could illustrate the ways in which the individual could find his or her happiness in the performance of social duty, or at any rate be inspired by social duty:

[T]he only teaching likely to be effective is such as will powerfully affect the emotions of the taught, no less than their intellects; we should, therefore, generally speaking, need teachers who themselves felt, and were believed to feel, sincerely and intensely, the moral and social emotions that it was their business to stimulate; and governmental appointment and payment would hardly seem to be an appropriate method of securing instructors of this type. If a spirit of devotion to a particular society or to humanity at large, and readiness to sacrifice self-interest to duty, are to be persuasively inculcated in adults, the task should, generally speaking, be undertaken by persons who set an example of self-devotion and self-sacrifice, and therefore by volunteers, rather than by paid officers (Sidgwick 1919: 214–215).

4 Conclusion

Sidgwick was to the end an educational reformer—or rather, a reformer who always came back to education as the crucial means of historical progress, in ethics, economics, politics, and other areas. His practical ethics, often only indirectly utilitarian, mostly concerned finding common ground despite foundational ethical differences, and that common ground was the cultivation of humanity. Given such views about the crucial need to cultivate the more critical, sympathetic, and altruistic capabilities of humanity, Sidgwick, it should be clear, scarcely fits the still too pervasive, confused conception of the utilitarian as someone out to defend the low-minded, narrowly useful, egoistic policies that would undermine, say, a commitment to liberal education and the humanities. But it should also be clear that his work leaves a great deal open when it comes to the future organization of society, when fewer concessions might need to be made to narrow economic self-interest.

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16

Alfred Marshall (1842–1924)

Neil Hart

1 Introduction

Alfred Marshall was undoubtedly the foremost member of his profession at a most critical stage of its development. He was the founder of the Cambridge School of Economics (at least in its modern incarnation), and played the leading role in the process of professionalising economics in Britain at the turn of the twentieth century. The significance of his role in the development of economics as a profession is captured clearly in the following words of his favourite student, John Maynard Keynes:

Marshall was the first great economist pur sang that there ever was; the first who devoted his life to building up the subject as a separate science, standing on its own foundations, with as high standards of scientific accuracy as the physical or the biological sciences. It was Marshall who finally saw to it that “never again will a Mrs Trimmer, a Mrs Marcet, or a Miss Martineau earn a goodly reputation by throwing economic principles into the form of a catechism or of simple tales, by aid of which any intelligent governess might make clear to the children nestling

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around her where lies normal economic truth” [Marshall 1897: 296]. But—much more than this—after his time Economics could never be again one of a number of subjects which a Moral Philosopher would take in his stride, one Moral science out of several, as Mill, Jevons, and Sidgwick took it (Keynes 1924: 56–57).

In terms of his contributions to the development of economic analysis, Marshall has traditionally been regarded by historians of economic thought as a leading pioneering equilibrium economist and, alongside the likes of Leon Walras, Carl Menger, and Stanley Jevons, a ‘co-founder’ of the ‘neoclassical’ approach to economics. Joseph Schumpeter (1954: 834) portrayed Marshall’s *Principles*¹ as representing more perfectly than any other the state of economics that emerged in England at the turn of the twentieth century. However, Marshall remains somewhat of an enigmatic figure in the development of economic analysis, for as Schumpeter (1941) also observed in his semi-centennial appraisal of the *Principles*, Marshall’s analytical apparatus appeared to be ‘obsolete’, a perception reiterated by subsequent commentators such as Brian Loasby (1978). This chapter reflects on the somewhat paradoxical role that Marshall’s writings have played in the development of economic analysis, and the partial equilibrium variant of ‘neoclassical’ economics in particular.

Born on 26 July 1842, in Bermondsey, Surrey (now London),² Marshall successfully completed studies towards the Mathematical Tripos at St John’s College, Cambridge, in 1865, achieving the distinction of becoming Second Wrangler to the eminent physicist, John Strutt (later Lord) Rayleigh. His choice of study at Cambridge reflected a fondness and aptitude for mathematics during his school days, as well as an apparent dislike for classical studies, much to the displeasure of his devoutly religious father. Marshall’s achievements in the Mathematical Tripos enabled him to secure a Fellowship at St John’s in 1865. However, by the early 1870s, Marshall’s intellectual endeavours had become devoted almost exclusively to political economy, which, at the time at Cambridge, was attached to the Historical and Moral Sciences. In 1877, Marshall married Mary Paley, a former student, forcing him to abandon his College Fellowship in order to conform to the celibacy rules of the University. He obtained a position at University College, Bristol, where he further progressed his teaching in political economy. Following the

¹ Unless otherwise noted, all references to the *Principles* refer to the eighth edition of Marshall’s *Principles of Economics* as published by Macmillan in 1920. C.W. Guillebaud’s (1961) *Notes to the Ninth Variorum Edition* (volume 2) provides an invaluable source from which the content and significance of the revisions to the various editions of the *Principles* can be observed.

² There has been some confusion regarding Marshall’s place of birth, largely due to inconsistent information provided at census collections, intentionally or otherwise; see the discussions in Coase (1994) and Littlechild (2012).

death of Henry Fawcett, Marshall was elected Professor of Political Economy at Cambridge in 1885, a chair he held up to 1908 when the position was taken up by his anointed successor, A.C. Pigou.

An appreciation of Marshall's perspective on the nature and role of economic theory requires a consideration of the enduring influences on his thinking that shaped the intellectual journey that led him from mathematics to the unintended destination of political economy. The nature of this journey was described in Marshall's own reflections contained in a passage that was initially intended for the Preface to his *Money, Credit, and Commerce*:

About the year 1867 (while mainly occupied with teaching Mathematics at Cambridge), Mansel's *Bampton Lectures* came into my hands and caused me to think that man's own possibilities were the most important subject for his study. So I gave myself for a time to the study of metaphysics; but soon passed to what seemed to be the more progressive study of Psychology. Its fascinating inquiries into the possibilities of the higher and more rapid development of human faculties brought me into touch with the question: how far do the conditions of life of the British (and other) working classes generally suffice for fullness of life? Older and wiser men told me that the resources of production do not suffice for affording to the great body of the people the leisure and the opportunity for study; and they told me that I needed to study Political Economy. I followed their advice, and regarded myself as a wanderer in the land of dry facts; looking for a speedy return to the luxuriance of pure thought (Marshall quoted in Keynes 1924: 10).³

On his journey towards the 'land of dry facts', Marshall had engaged in periods of intensive study and deliberation in philosophy and psychology, involving initially the philosophical foundations of knowledge and later the process by which knowledge itself is acquired. Enduring influences on Marshall's thinking were to be found in the writings of Georg Hegel, Herbert Spencer, and Charles Darwin, and in the themes that had developed within the German Historical School, amongst the evolutionists, and from the emerging discipline of psychology that had escaped the clutches of metaphysics. Most significantly, Marshall's studies occurred at a time when a new evolutionary paradigm of the functioning of the human mind was challenging existing

³The nature of Marshall's intellectual journey, covered in some detail in Keynes (1924), is now much better understood following the publication of Peter Groenewegen's (1995, 2007) definitive biography of Marshall and John Whitaker's (1996) edited volumes of Marshall's correspondence. Also of considerable interest are the recollections provided by Mary Paley Marshall (1947). Marshall's early philosophical essays are assembled in Raffaelli (1994), and his historical notes and essays are presented in Cook (2005, 2008). Amongst the philosophical essays, *Ye Machine* (reproduced in Raffaelli 1994) is of particular interest.

ideas on the nature of knowledge and its role in shaping and transforming society. From his synthesis of these ideas, Marshall insisted on the historical specificity of economic analysis, together with the imperative of analysing economic and social change from an evolutionary perspective in which the cognitive powers of ‘man’ played a central role. Marshall saw the potential for human development as being connected to the growth and application of knowledge, being driven by what he associated with the ‘moral imperative’ that shaped human behaviour. These themes were communicated directly in his 1885 Cambridge Chair inaugural lecture:

In different ways Goethe, Hegel, Comte and other writers called attention to the development of the inner character and outward institutions of man, and worked their way towards the notion of tracing and comparing the modes of growth of the different sides of human nature ... At last the speculations of biology made a great stride forwards: its discoveries fascinated the attention of all men as those of physics had done in earlier years. The moral and historical sciences of the day have in consequence changed their tone, and Economics has shared in the general movement (Marshall 1885b: 154).

Also found in Marshall’s 1885 lecture was a statement of his plans to establish economics as a distinct and respected ‘scientific’ discipline at Cambridge and beyond, with a set of institutional arrangements in place to both nurture and perpetuate its future progress. The significance of Marshall’s contributions in this regard will be briefly reiterated in the next section, with a particular emphasis placed on the nature of Marshall’s professional leadership. This will be followed by an examination of Marshall’s legacy in terms of the subsequent development of economic analysis.

2 Marshall, the Cambridge School, and the Professionalisation of Economics

Marshall’s appointment to the Cambridge Chair in 1885 reflected the high esteem in which he was held in the profession despite the absence of a major publication, with the first edition of his *Principles* not appearing until 1890. Much of Marshall’s esteem undoubtedly arose from the high regard with which he was held as an academic administrator and educator at Cambridge and Bristol, and as his Cambridge colleague Somerton Foxwell (1887: 92) noted, ‘half the economics chairs in the United Kingdom’ were occu-

pied by his former pupils. At the time Marshall was elected to the Chair at Cambridge, the status of economics in terms of its academic and professional standing appears to have been at a rather low point, and the prospects for further advancement were not immediately encouraging. Economics was looked upon in the main as a minor and disputed area of study, with many of its lecturers and professors recruited or ‘borrowed’ from other established disciplines. This perception was conveyed candidly in the introduction to Henry Sidgwick’s *Principles of Political Economy*, which refers to the objective decline of political economy and its growing unpopularity in the outside world:

In short, when the concluding quarter of this century began, it was evident that Political Economy had returned to the condition in which it was in the second quarter; and that McCulloch’s melancholy admission that ‘the differences which have subsisted among the most eminent of its professors have proved exceedingly unfavourable to its progress, and have generated a disposition to distrust its best established conclusions’ was again only too applicable (Sidgwick 1883: 6).

As stated in his Inaugural Lecture, Marshall’s first objective was the attainment of a new status for economics within the academic programmes at Cambridge, where economics had sheltered under the shadows of the Historical and Moral Sciences and struggled to attract students. In his resolute pursuit of this objective, Marshall became embroiled in a protracted and at times heated campaign against the Special Boards of Moral Sciences and History, bringing him into direct conflict with his early mentor, Sidgwick, Knightbridge Professor of Moral Sciences at Cambridge from 1883.⁴ Marshall went on to achieve his ambition with the establishment of a separate Economics Tripos in 1903, and while the number of students enrolling in economics at Cambridge, together with the teaching resources, was initially largely unaffected by the creation of the new Tripos, the Cambridge School of Economics had grown from these rather humble beginnings to become a prominent force within the academic economics profession by the closing years of Marshall’s life. Apart from his activities at Cambridge, Marshall’s

⁴Sidgwick (1838–1900) was not only highly regarded by his contemporaries in philosophy, but was also a significant contributor to the economics literature and debate. In addition to the Economics Tripos struggles, a substantial issue that fractured the relationship between Marshall and Sidgwick arose from Marshall’s prominent opposition to Sidgwick’s draft resolution designed to give women limited access to a degree at Cambridge. Marshall’s opposition to Sidgwick’s keenly devised proposal was contrary to his previously enthusiastic and functional support for the university education of women during his earlier years at Cambridge, to say nothing of his (short-lived) academic collaboration with Mary Paley Marshall. Again, Marshall’s shifting stance on the question of women’s education at Cambridge reflects rather poorly on his character and dealings with his closest colleagues (see further discussion in McWilliams Tullberg 1975).

leadership of the profession was witnessed through his role in the formation of the British Economic Association and its flagship publication, the *Economic Journal*. While the ongoing management of these ventures was left in the hands of others, Marshall continued to play a significant indirect role in the dealings of the Association and its journal. Marshall's diverse and extensive services to government inquiries also accentuated the role of the academic economist in public affairs, helping to reinforce the impression of specialist skills that define the professionally trained economist (Groenewegen 1995: 343–398). In addition, the successive editions of Marshall's *Principles* took on the role of a widely recognised textbook, albeit one that came to be consulted selectively so as to sidestep the more 'troublesome' qualifications and ambiguities otherwise encountered.

Clearly, Marshall had succeeded in guiding and conducting the affairs of a heterogeneous group of academics who shared the common ambition of establishing economics as a distinct discipline with a recognised set of professional standards and institutions. As Bob Coats (1967: 711) observed in his investigation into the sociological aspects of British economic thought during the Marshallian era, Marshall effectively 'led economics from the rear', assembling the structure that would support the professionalisation process, and then manoeuvring his colleagues in whatever way he saw appropriate to reinforce these structures. In this setting, Marshall could be described as the unpopular leader of an ambition that few economists could resist, possessing a remarkable talent for generating antagonism amongst his contemporaries, critics, and supporters alike, relentlessly pressuring his disciples to structure their careers and plan their lectures in ways that promoted the directions he believed the economics profession should journey.⁵ A key strategy for Marshall was to portray economics to the scientific community in a way which emphasised the solidarity of its principles—one that had finally overcome the 'battle of methods' that threatened its survival during the middle of the nineteenth century. While the development of economic analysis was to be seen as an ongoing programme, it was not to be associated with irreconcilable disputes over theory or methodology. Marshall acclaimed the contributions of his contemporaries that he depicted as strengthening the existing economics edifice, and defended the reputations of his revered classical predecessors. He was, for example, strongly opposed to Jevon's (1879: 1) assertion

⁵This corresponds with the conclusion reached by Maloney (1985). Such a perception emerges most directly from the diary entries and correspondence between Marshall and John Neville Keynes, who, for a time at least, was Marshall's closest and most important and trusted professional ally. As revealed through Deane's (2001) extensive examination of these archives, many instances reflect unfavourably on Marshall's character, judgement, and professional dealings with his colleagues.

that Ricardo had ‘shunted the car of economic science on a wrong line’, and that consequently, it was necessary to ‘pick up the fragments of a shattered science and to start anew’. There were, however, limits to the extent to which heterogeneity was tolerated, with a particularly notable example found in the case of J.A. Hobson, who was unable to find his rightful place in the economics profession for reasons signified by the subtitle of his co-authored *Physiology of Industry*: ‘Being an Exposure of Certain Fallacies in Existing Theories of Economics’. While a variety of theoretical and methodological approaches were acceptable within the economics profession under Marshall’s reign, the ‘exposure of fallacies’ was not to be encouraged. Similarly, criticisms from ‘economic amateurs’ were swiftly dispatched with by the ‘professionals’.

Marshall was particularly concerned not to be associated with the creation of a new doctrinal ‘school’, instead linking his own contributions with what he claimed to be continuity in economic thought. Assertions that Marshall was a ‘Menger-like’ figure who set out to establish a ‘neoclassical-marginalist’ orthodoxy that would extinguish the flames of the English Historical School rest on the untenable proposition that Marshall was in fact an opponent of the Historical School, for as Gerald Shove (1942: 309) concluded in his highly discerning estimation of Marshall’s work, if any school of thought outside the Ricardian tradition set its mark on the *Principles*, it was the Historical School, rather than the marginal utility school. Indeed, W.J. Ashley, a prominent associate of the Historical School, was moved to state in his review of Marshall’s *Principles* that ‘it brings a message of conciliation to divergent schools, and it makes it possible for “deductive” and “historical”, “scientific” and “ethical” economists to “work together in harmony”’ (Ashley 1891: 489). To some extent, the perception of a significant methodological divide between Marshall and the English Historical School and their supporters rests on Marshall’s often referred to exchanges with William Cunningham, who plainly saw Marshall as belonging securely in the deductive camp, a perception that Marshall was keen to distance himself from. The ‘urgency’ of Marshall’s public responses to Cunningham to a significant extent reflected Marshall’s displeasure with Cunningham’s opposition to the Economics Tripos at Cambridge, along with Cunningham’s proposal for the partitioning of political economy into economic history and economic lexicography.

Having successfully established economics as a distinct part of the teaching programme, Marshall’s attention at Cambridge eventually became focused on the inevitable question of the succession to the Cambridge Chair. Here we observe Marshall’s active and at times perfidious support for A.C. Pigou over Herbert Somerton Foxwell. Foxwell had been one of Marshall’s first students in the moral sciences, and went on to lecture in logic, psychology, and phi-

losophy, before moving to economics where he lectured for over 30 years at Cambridge and University College London. He was one of Marshall's most dutiful allies, and Marshall frequently turned to Foxwell for assistance in the implementation of his plans. While there were significant differences between the two on matters of public policy and the direction of the new Economics Tripos, these matters were unlikely to have weighed heavily in Marshall's plans. Rather, the most significant reason why Marshall supported Pigou over Foxwell for the Chair arose from what was perceived to be in the best interests of the consolidation of the economics teaching programme at Cambridge and the economics professionalisation process in general.

Importantly, Marshall's support for Pigou was not based on a belief that Pigou would guide economic analysis along the pathway that Marshall had championed in his published writings. Unlike Foxwell, Pigou's preferences strongly favoured the development of pure theory, despite his own credentials as a historian, a methodological stance that departed fundamentally from that which informed Marshall's contributions. Indeed, in his private correspondence and manuscript notes on Pigou's *Wealth and Welfare*, reproduced with insightful commentary in Bharadwaj (1972), Marshall expressed serious reservations regarding Pigou's unqualified usage of the statical method in dealing with the laws of return, concluding that the supply curve of the type constructed by Pigou had 'no reality' (Marshall quoted in *ibid.*: 33). However, these fundamental concerns about Pigou's method were never communicated in Marshall's published writings. This was indicative of Marshall's willingness to compromise his own views on the nature and method of economic analysis in order to facilitate the professionalisation process, and a refusal to publicly question the contributions of close professional colleagues, including Pigou but also prominent others such as Edgeworth, whose approach to economic analysis departed markedly from that he had advocated. All of this serves to illustrate the fact that Marshall's pivotal role in the professionalisation process was not driven by the desire to provide leadership on issues of economic theory and method.

3 Marshall's Economics: Equilibrium and Evolution

This section will explore in some detail the way in which Marshall set about assembling his 'well-known' theory of relative prices, and the role that equilibrium and evolutionary processes were *intended* to play in this construction. The following sections extend this discussion to factor price determination

and a consideration of the significance of Marshall's often neglected applied studies of industry. Marshall's indirect influence on the later development of the Cambridge approach to macroeconomics piloted by Keynes will then be noted. The question of Marshall's legacy in terms of the development of economic analysis is then reconsidered by way of some concluding observations.

Marshall's overall approach to the formulation of economic analysis was stated directly in the Preface to his *Principles*:

The Mecca of the economist lies in economic biology rather than in economic dynamics. But biological conceptions are more complex than those of mechanics; a volume on Foundations must therefore give a relatively large place to mechanical analogies; and frequent use is made of the term 'equilibrium' which suggests something of stactical analogy ... This fact, combined with the predominant attention paid in the present volume to the normal conditions of life in the modern age, has suggested the notion that its central idea is 'stactical', rather than 'dynamical'. But in fact it is concerned throughout with the forces that cause movement: and its key-note is that of dynamics, rather than statics (*Principles*: xiv).⁶

According to Marshall's account, the 'mechanical' or 'equilibrium' component of his economics was completed in the years 1867–1879, in a largely unpublished form, beginning with a 'translation' of Mill's version of Ricardo and Smith's doctrines into mathematics.⁷ Marshall claimed that the development of these aspects of his analytical framework occurred before he had become familiar with the work of Jevons, being inspired largely by his familiarity with the work of Antoine Augustin Cournot and Johann Heinrich von Thünen. However, Jevons's influence on Marshall's thinking was undoubtedly more than Marshall was willing to admit. Also important were the 'new views' on economics emanating from Germany, where the prevailing texts had settled on a demand and supply partial equilibrium approach not that dissimilar to the one later assembled by Marshall, and which included demand functions formulated without delving deeply into the fundamental causes of 'utility' (Streissler 1990).

Questions often arise as to the reasons for the delay until 1890 in the publication of the first edition of the *Principles*. In part, this was due to

⁶The opening sentence of the quoted statement first appeared in the Preface to the fifth edition of the *Principles* (1907), with an earlier rendition found in Marshall (1898b: 318).

⁷Many of these early publications are assembled in Pigou (1925), and with insightful commentary in Whitaker (1975).

bouts of ill health and a heavy workload at Cambridge and Bristol. However, perhaps the most important reason for the delay is captured in Keynes's observation:

Marshall...arrived very early at the point of view that the bare bones of economic theory are not worth much in themselves and do not carry one far in the direction of useful, practical conclusions. Holding these views and living at a time of reaction against economists when the faults of his predecessors, to which he draws attention above, were doing their maximum amount of harm, he was naturally reluctant to publish the isolated apparatus of economics, divorced from its appropriate applications (Keynes 1924: 34–35).

Marshall's abstract equilibrium models could not, in their existing form, incorporate in a meaningful way the observations on human behaviour and social progress that Marshall had gathered on his intellectual journey to political economy. During the period leading up to the writing of the *Principles*, Marshall was an increasingly keen observer of industry and student of economic history, two elements that he believed were indispensable to useful economic analysis.

Given his enthusiastic embrace of evolutionary modes of thinking, it is not surprising that Marshall turned to the 'speculations from biology' in his endeavour to breathe life into his 'isolated' equilibrium models. By the 1890s, his system of 'economic biology' came to epitomise a methodological ideal, reflecting an ambition to introduce a mode of thinking that departed in several important respects from that which Marshall associated as originating from the 'mathematical-physical' sciences, which failed to take into account the changing nature of 'man' and institutions. This theme was repeated throughout the successive editions of Marshall's *Principles*, and reiterated in his methodological essays, as instanced in the following passage:

But the catastrophes of mechanics are caused by changes in the quantity and not in the character of the forces at work: whereas in life their character changes also. 'Progress' or 'evolution', industrial and social, is not mere increase and decrease. It is organic growth, chastened and confined, and occasionally reversed by the decay of innumerable factors, each of which influences and is influenced by those around it; and every such mutual influence varies with the stages which the respective factors have already reached in their growth (Marshall 1898b: 317).

Importantly, it was Marshall's ambition to create a unity between his system of economic biology dealing with qualitative change and the mechani-

cal models he had developed well before the publication of the *Principles*.⁸ Marshall's partial equilibrium analysis of relative prices founded on demand and supply schedules is developed in Book V of the *Principles*. Formal mathematical representation of the theory is largely relegated to mathematical notes and appendices, reflecting Marshall's views on the role of mathematics in economic discourse:

But I know I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypothesis was very unlikely to be good economics: and I went more and more on the rules—(1) Use mathematics as a shorthand language, rather than an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the mathematics. (6) If you can't succeed in 4, burn 3. The last I did often (Marshall to Bowley, 27 February 1906, quoted in Whitaker 1996: volume III, 130).

The underpinnings of the demand and supply schedules are developed in Books III and IV of the *Principles*, and an understanding of the content of Book V requires a close consideration of these earlier books. Before proceeding, some general points relating to Marshall's partial equilibrium analysis and the nature and role of competition should be noted.

From the outset, Marshall cautions that a chief difficulty in economic theorising arises in the treatment of time, which is recognised as being continuous and irreversible. It is in this setting that Marshall developed his familiar time period analysis, which was essentially a device which breaks down the analysis into different time frames which had the effect of allowing some events and quarantining others temporarily into the *ceteris paribus* clause. Despite the flickering of 'general equilibrium' tucked away in the mathematical notes in successive editions of the *Principles*, Marshall held firmly to the view that economic problems needed to be treated 'a bit at a time', both by market participants and by those who were seeking to analyse their behaviour and develop useful policy recommendations.⁹

Unlike the later Walrasian conceptualisations, Marshall stresses that if an equilibrium is in fact to be reached, it would be the outcome of actions taken by market participants formulated on the basis of information

⁸ It should be noted that some indications of the role and content of Marshall's system of economic biology can be found in the first edition of the (Marshall's 1879) *Economics of Industry*.

⁹ For further discussion of the inconsistency between general equilibrium representations and the notion of evolution resting on the possibility of localised change, see Raffaelli (2008).

revealed in their market dealings with each other. Competition in this context is not defined in terms of a particular market structure or configuration, but rather as a behavioural activity which was a form of organisation that evolved through time. The unceasing process of adaptation and reorganisation associated with economic change ensures that market structure was itself a transitory configuration. Competition was not simply a vehicle which ensured that only the 'fittest' would survive within the evolutionary context; competition was correlated most directly with the ideal of economic freedom that was to enable individuals to develop and realise their capabilities and which both allowed and inspired the business enterprise to adapt to the ever-changing economic environment. As such, competition as an activity resembled much more closely the classical conceptualisations, departing markedly from the later 'neoclassical' definitions based on a particular market structure (Kerr 1993).

In order to avoid common misinterpretations of Marshall's economics, it is particularly important to dismiss the notion that Marshall was seeking to construct a theory of *competitive equilibrium*. The notion of 'much free competition' used in Marshall's simple example of a fishing industry undergoing a once-and-for-all change in demand (in Book V, Chapter 3 of the *Principles*) is highlighted as being a *provisional assumption*. Even within the confines of 'free competition', the assumption of perfect knowledge, which Marshall associates with 'perfect' competition, is explicitly excluded (see, for example, *Principles*: 540). Similarly, while free competition was likely to be characterised by a large number of competitors, these encompass competitors with businesses of all sizes (*Principles*: 397). Once Marshall's analysis in the *Principles* ventures beyond the simple fishing market example, and manufacturing and large-scale production are encountered, a market structure far removed from anything resembling pure competition emerges. In his discussion of manufactures in Chapter XII of Book V of *Principles*, Marshall drew a distinction between what could be termed 'general' and 'particular' markets, with 'general' markets being portrayed as the sum total of the 'particular' (or 'special') markets of the firms in the industry. Significantly, manufactures were seen as being 'adapted to special tastes', and thus tended to be confined 'more or less to its own particular market' (*ibid.*: 457–458). Essentially, Marshall's partial equilibrium industry analysis did not in any sense imply, or indeed require, the existence of pure or perfect competition, or even the classical notion of free competition.

A distinguishing characteristic of Marshall's analysis of market demand is that, while familiar concepts such as marginal utility are to be found, the demand schedules are not derived in any systematic way from choice-

theoretic approaches based on constrained utility optimisation principles. Instead, they are premised more on empirical generalisations than on any particular behavioural assumptions, or as Shove (1942: 307) surmised, one may hazard a guess that Marshall began with the objective demand and supply schedules, the phenomenon of the market place, and worked back from them to their psychological basis, not (as was the case with Jevons) the other way around. The essentially ‘timeless’ approach to utility maximisation often attributed to Jevons could not be accepted by Marshall, as it excluded the role of habits (routines) and expectations that were ultimately connected with history. This perspective is reiterated most directly in Chapter II of Book III, where, for example, emphasis is placed on the essentially evolutionary processes that linked wants and activities as being at the heart of the decision-making of consumers.

These evolutionary dimensions become even more apparent in the treatment of industrial structure and transformation in Book IV of the *Principles*. Here, Marshall combined ‘speculations from biology’ with Adam Smith’s insights into the relationship between the division of labour and economic efficiency and progress. In particular, Marshall sought to understand how the organisation of business is shaped by the interrelationships between specialisation, competition, and the transfer of knowledge. The transitory landscape of industrial structure was the outcome of cumulative routinisation and specialisation made possible by the division of labour on the one hand, and creativity through innovation on the other.¹⁰ The actions of business enterprises were not informed by, nor constrained by, technical relations encoded in given production functions. Instead, the acquisition of knowledge was of central importance, with both internal and external economies playing a substantive role, being essentially knowledge-based. The task Marshall set himself was to construct supply schedules that could play the key role in his proposed long-period equilibrium analysis, and which were derived and interpreted in a manner that was consistent with what he understood to be the realities of industrial structure and transformation as laid out in Book IV (and elsewhere) in the *Principles*. The latter clearly required an ‘evolutionary’, as opposed to a ‘mechanical’, perspective.

The first issue to be resolved was that there appeared to be no logical reason why the presence of scale economies would not lead to monopolisation of

¹⁰As emphasised in the introduction, this is part of a general philosophical idea that resurfaces throughout Marshall’s intellectual career, starting from the early philosophical papers composed in the 1860s.

industries, something that was inconsistent with his observations of industry, as Marshall indicated in a letter to A.W. Flux (dated 7 March 1898):

My confidence in Cournot as an *economist* was shaken when I found his mathematics *re* I.R. [Increasing Returns] led inevitably to things which do not exist and have no relation to reality. One of the chief purposes of my Wanderjahre among factories, etc., was to discover how Cournot's premises were wrong. The chief outcome of my work in this direction, which occupied me a great deal between 1870 and 1890, is in the 'Representative firm' theory (Marshall quoted in Pigou 1925: 406–407; italics in original).

This 'complication' could not simply be dismissed by assuming that economies of scale were exclusively external to individual firms, a rationalisation explicitly rejected by Marshall. Instead, Marshall's explanations drew on a biological analogy, where the growth process of firms was likened to the growth of trees in a forest, from which Marshall inferred that the full life of a firm seldom lasts very long, as it is likely to 'ere long quickly to decay' having lost the exceptional energy (or creative faculty) that enabled it to rise (*Principles*: 287). As a result, emerging firms would eventually draw on their greater youthful vitality and outperform their more mature rivals, thus restraining and eventually arresting the capacity of established firms to exploit the opportunities that arose from returns to scale and to gain a greater share of the market. Marshall's increasing awareness of the emergence of joint-stock companies became apparent from the sixth edition of the *Principles* (published in 1910) where it was recognised that the general rule of eventual stagnation is far from universal. However, Marshall did not consider that this qualification substantially altered his general proposition, as he suggested that it was 'likely' that joint-stock companies would eventually lose much of their progressive force. As noted below, the significance and impact of large-scale business organisations was considered in much more detail in Marshall's applied studies of industry presented in *Industry and Trade* (Marshall 1919).

If accepted, Marshall's life-cycle theory of the firm analogy implied that a position of long-period equilibrium for an industry coincided with a situation in which individual firms are at *disequilibrium*, with some businesses rising and others falling (*Principles*: 378). Therefore, the notion of the 'marginal' or 'equilibrium' firm could not have any operational role in the derivation of long-period normal supply conditions. It is in this context that Marshall introduced the much maligned and misunderstood concept of the representative firm, an analytical or statistical construct

which, in terms of the biological analogy, describes the characteristics that may distinguish a tree found in a particular forest at a point in time from other trees at different stages of development or decline. As the industry experiences growth, the representative firm would be growing at the same rate as that of the industry as a whole, being shaped by, and changing with, those factors that determine the pattern of growth in the industry as a whole. Its role was to determine how costs for a *diversified* industry could be measured in the calculation of long-period normal market prices. In order to investigate how the industry as a whole may respond to a given change, we simply analyse how the hypothetical representative firm is likely to react (*ibid.*: 317).

Marshall's representative firm theory was, therefore, intended to assist him in bridging the gap between evolutionary conceptualisations of 'organic' change and mechanical notions of equilibrium as states of adjustment to circumstances through time, leading writers such as Shackle (1965: 36) to conclude that Marshall's peculiar triumph was his creation of a unity out of the conceptions of equilibrium and of evolution. However, Marshall had to concede that the long-period equilibrium method, when confronted with the evolutionary dimensions of economic activity outlined above, led to 'unsatisfactory results', exposing 'imperfections in our analytical methods' (*Principles*: 809):

It must however be admitted that this theory is out of touch with real conditions of life, in so far as it assumes that, if the normal production of a commodity increases and afterwards again diminishes to its old amount, the demand price and the supply price will return to their old positions for that amount ... For, when any casual disturbance has caused a great increase in the production of any commodity, and thereby has led to the introduction of extensive economies, these economies are not readily lost. Developments of mechanical appliances, of division of labour and of the means of transport, and improved organisation of all kinds, when they have been once obtained are not readily abandoned (*ibid.*: 807–808).

While Marshall's long-period analysis could conceivably indicate equilibrium positions *ex post*, it could not describe the process by which the equilibrium positions may be attained or sustained. This would suggest that the long-period supply schedule that Marshall derived from the representative firm theory did not have a meaningful expository role to play in the *determination* of long-period values unless the existence of the more fundamental evolutionary forces, reflected through biological analogies, were denied. The difficulties arose because of the inability of comparative static equilibrium

analysis to take account of what Marshall understood to be the qualitative nature of economic change and irreversibility of events taking place in historical time. It needs to be emphasised that these ‘unsatisfactory results’ were not contingent upon the type of market structure being considered, or whether the scale economies were of the internal or external variety.

Marshall proposed that a more satisfactory framework in which these evolutionary economic processes could be examined would be provided in a ‘second volume’ of the *Principles*. However, the promised volume failed to materialise, and the defence of equilibrium analysis in the Preface to the *Principles* becomes more pronounced as we move through the eight editions, and the detailed discussion of the ‘imperfections in our analytical methods’ were relegated to an appendix in the later additions of the *Principles*.¹¹ At the same time, Marshall’s system of economic biology remained intact through the revised editions of the *Principles*, and the economic biology Mecca continued to be avidly proclaimed and defended. Marshall’s intention of establishing a scheme of economic biology was not so much a discarded intention, but rather an unfulfilled ambition that as a result faded with the passing of time.

4 Capital and Labour

Marshall’s explanation of the determination of factor prices was developed largely within the type of framework described above. During Marshall’s time at Cambridge, important debates over capital and distribution theory occurred, centred on the contributions of Eugen Böhm-Bawerk, Friedrich Wieser, J.B. Clark, Irving Fisher, and Thorstein Veblen. However, Marshall declined to become actively involved in these controversies. Indeed, as Christopher Bliss (1990: 227) observed, the reader has to dig deeply and even then without much success to find a theory of capital in Marshall’s writings, and a consistently applied definition or measure of ‘capital’ cannot be found in his *Principles*. Instead, what is found in Marshall’s theory is a rather intricate and at times perplexing mixture of explanations of income distribution based on technologically based and socially determined elements. On

¹¹ In the first four editions of the *Principles*, the subject matter of what became Appendix H was located directly in the long-period equilibrium analysis in Book V. Again, the subsequent banishment of this material to the appendices cannot be simply dismissed as an inconsequential element of the rather extensive rearrangement of the contents of the fifth edition. Its effect was to push to the background a detailed discussion of issues which most directly challenged the usefulness of the equilibrium analysis found in Book V of the *Principles*.

the demand side, an inverse relationship between the value of ‘capital’ and productivity is hypothesised; however, the association is established more by assertion than through tightly woven theoretical reasoning. The rate of interest is intended to provide the mechanism through which the supply of money capital (or ‘waiting’) could be equated with the demand for capital, but the relationship between the rates of interest and the normal rate of profit is not established by Marshall.

Much more attention was devoted to the theoretical and practical issues associated with the determination of wages and employment. Initially, Marshall’s thinking was heavily influenced by Mill’s version of the classical Wages-Fund theory, although by the publication of the first edition of the Marshalls’ *Economics of Industry* (1879), the ‘wages fund’ had been widened to what may be termed a wages and profits fund (or, more precisely, flow). By the arrival of the *Principles*, Marshall resorted to a more familiar demand and supply setting, with the preliminary observation being that, like capital, the share of wages in the national dividend is related to its efficiency or contributions to production. The application of this simple principle became somewhat tempered when Marshall widened the discussion to consider a range of issues which would constitute a comprehensive course in labour economics. Included amongst these were the effects of different grades of efficiency amongst workers and market segmentation, together with the existence of workers’ and employers’ groups and other ‘immobilities’ which extinguished any tendencies towards free competition in labour markets. In terms of policy debates, Marshall was highly critical of attempts by labour unions and others to move towards a national minimum wage, partly because unemployment *may* result if it were set too high. More importantly, standard wages failed to account for the need to allow for regional wage differentials, and would force employers to put relatively inefficient workers in the same payment class as more efficient workers. This is not to suggest that Marshall supported as a general principle the notion that there was a negative relationship between wages and labour productivity and employment, for, as the following discussion would suggest, we find the seeds of modern efficiency wages theory in Marshall’s writings:

But it was only in the last generation that a careful study was begun to be made of the effects that high wages have in increasing the efficiency not only of those who receive them, but also of their children and grandchildren. In this matter the lead has been taken by Walker and other American economists; and the application of the comparative method of study to the indus-

trial problems of different countries of the old and new worlds is forcing constantly more and more attention to the fact that highly paid labour is generally efficient and therefore not dear labour; a fact which, though it is more full of hope for the future of the human race than any other that is known to us, will be found to exercise a very complicating influence on the theory of distribution (*Principles*: 510).

When policy issues were being contemplated, Marshall's theoretical analysis of labour became heavily qualified. This can be observed, for example, during his extensive participation in public debates while serving as a member of the Labour Commission (1891–1894). Likewise, in the Preface contributed to L.L.F.R. Price's *Industrial Peace*, Marshall's contemplation of what constituted a 'fair rate of wages' led to the development of a detailed consideration of the potential role of conciliation and arbitration in the determination of wages, reaching the following conclusion: 'Though, however, for helping the lowest class of workers we must look elsewhere than to systems of conciliation, these systems are, as has been shown above, a powerful means of raising the working classes as a whole. They are scarcely less powerful a means of raising their employers' (Marshall 1887: 226).

Even more pronounced in Marshall's writings are the effects of general education on labour efficiency and economic progress. General education was seen as shaping human character and mental attitudes, essential for progress embodied in vigour, initiative, versatility, and spontaneity. It also provided the means for individuals to rise above the deprivations that characterised the everyday lives of the working class.

5 Applied Industrial Economics

Clearly, Marshall's industrial economics formed the heartbeat of his evolutionary perspective on economics. This analysis was informed by detailed observations of contemporary industrial organisation, together with historical investigations into its development. It was Marshall's intention that the theoretical analysis presented in the *Principles* be read in conjunction with historical and applied studies of industry. In this setting, Marshall's *Industry and Trade* was originally promised as part of a 'second volume' to extend the general introduction to the study of economic science provided by the *Principles*; unfortunately it did not appear

in print until 1919 when Marshall had reached 77 years of age.¹² *Industry and Trade* bears testimony to the enormous mass of industrial knowledge that Marshall had accumulated through time, augmenting and extending the practical examples and historical case studies found in the *Principles*. A prominent example of this is Marshall's detailed references to the increasing prevalence of joint-stock companies which meant that the productive life of a representative producing business is likely to extend beyond the limits depicted in the life-cycle biological analogy portrayed in the *Principles*.

However, Marshall continued to argue that the competitive advantages did not lie wholly with large-scale business. As firms grew larger, limitations to effective management would be likely to develop, limitations arising, for example, from factors such as 'excessive enlargement of scope' and 'mechanical methods of administration' that may 'repress elasticity and initiative' (Marshall 1919: 321–324). Throughout, the importance of organisation within firms in influencing the efficiency of labour is highlighted, together with the prominence placed on the role of general education, as emphasised in the *Principles*. There is also extensive discussion on the nature and role of competition and potential government involvement in competitive processes. Marshall's analysis of industry therefore highlighted the existence of a variety of conflicting tendencies, and his heavily qualified views on the reform of industrial organisation was premised on a call for human intervention to check that those tendencies that prevail bring long-term benefits to the social environment.

When read in isolation from the *Principles*, the content of *Industry and Trade* may appear to some readers to be mainly descriptive in nature, devoid of any specific theoretical content. However, when placed in the context of Marshall's evolutionary account of industrial organisation and progress found throughout the *Principles*, it can be seen that Marshall's applied industrial analysis plays the crucial role of providing the historical and institutional material that informs the analytical core of the *Principles*. Certainly, Marshall rejected the notion that 'theory' and 'applied work' could be developed independently from each other, a position which departed markedly from that advocated by his Cambridge successor,

¹²Mention again should also be made of the Marshalls' first book, *The Economics of Industry*, published in 1879 and co-authored with Mary Paley Marshall. As Groenewegen (2007: 58) remarked in his outline of the book's central themes, several chapters in the book 'are a splendid example of the way in which Marshall enriched conventional classical doctrine by giving it a new interpretation in the light of changed industrial circumstances of which his travels had made him aware'.

Pigou (1922). Perhaps the reason for the subsequent neglect, until recent decades, of Marshall's applied industry studies was most aptly summarised by one of his most loyal disciples, P.W.S. Andrews:

His analysis of industrial equilibrium was proved to be inconsistent with a theory of the equilibrium of the individual business which evolved from the 'Marshallian tradition' and which was believed to be basic to Marshall's own concept of competition. The difficulty has been resolved by dropping industrial analysis and retaining the static equilibrium theory of the individual business. It would have been equally legitimate to have abandoned the latter (Andrews 1951: 140).¹³

6 Marshall and Macroeconomic Analysis

Maynard Keynes included Marshall alongside J.S. Mill, Ricardo, Edgeworth, and Pigou as prominent adherers to the postulates of classical theory (which Keynes associated with an acceptance of Say's Law), highlighting the fact that the great puzzle of effective demand that Malthus wrestled with was absent from Marshall's analytical structure. Interestingly, though, not unlike Keynes, Marshall had argued that it was the 'want of confidence' that led to the spread of 'commercial disorganisation' associated with the evils of unemployment:

The chief cause of the evil is a want of confidence. The greater part of it could be removed almost in an instant if confidence could return, touch all industries with her magic wand, and make them continue their production and their demand for the wares of others ... But the revival of industry comes about through the gradual and often simultaneous growth of confidence among many various trades; it begins as soon as traders think that prices will not continue to fall: and with a revival of industry prices rise (*Principles*: 711).

Similar to Keynes, Marshall saw the 'trades which make fixed capital' (*ibid.*) as playing a key role in the fluctuations of commercial activity. However, unlike Keynes in *The General Theory*, in Marshall's view the only effective remedy was to ensure that 'credit can be based on the solid foundation of fairly accurate forecasts; and that reckless inflations of credit—the chief cause of all

¹³ For further discussion of those who kept to Marshall's vision of the role for applied industry studies, see Raffaelli (2004). An earlier appreciation of the scope of Marshall's industry economics can be found in O'Brien (1990).

economic malaise—may be kept within narrower limits’ (ibid.: 710). The error in Marshall’s analysis was, according to Keynes (1936 [1973]: 19), captured by Hobson’s comments on Marshall’s failure to grasp the critical importance of a statement found in the Marshalls’ *Economics of Industry*: ‘But though men have the power to purchase, they may not choose to use it’ (Marshall and Marshall 1879: 154; repeated in *Principles*: 710). In this respect, Marshall stood convicted in Keynes’s (1936 [1973]: xxxii) judgement of committing the important mistakes associated with extending to the system as a whole conclusions which may have been correctly arrived at in respect of a part of it taken in isolation, leading to the widespread acceptance of Say’s Law, which excluded the possibility that demand could be deficient for the economy as a whole.¹⁴

Despite the limitations noted above, Marshall’s economics did have an important impact on the approach Keynes and his Cambridge followers were to bring to the development of macroeconomic theory. While, along with many of his contemporaries, Marshall’s perspective on the aggregate economy is often linked with the quantity theory of money, the Marshall that Keynes was familiar with never claimed to be a quantity theorist, associating such views with an excessive preoccupation with the link between money *supply* and prices.¹⁵ Characteristically, Marshall instead sought to base his approach on both the supply of money *and* money demand, inspiring the ‘Cambridge cash balance approach’ to monetary economics which provided the foundations for the liquidity preference scheme constructed by Keynes and his followers.

Perhaps more substantial was Marshall’s impact on the methodological perspective employed by Keynes and his Cambridge followers. Keynes’s thinking on economic issues was strongly influenced by his close personal and intellectual associations with Marshall, beginning with his attending Marshall’s lectures in 1905–1906 and promoted through a continuing dialogue between the two close friends. The significance of this influence is aptly summarised in the following terms by the prominent and definitive Keynes’s biographer, Robert Skidelsky:

¹⁴The recognition by Richard Kahn and Keynes of the importance of the study of the short period also represented a significant movement away from Marshall’s emphasis on the long period as the main time period for analysis.

¹⁵Much of Marshall’s writings on monetary matters was not published in a systematic form until *Money, Credit, and Commerce* appeared in 1923, which was the last of Marshall’s published works and largely represented a collection of earlier work on monetary theory, much of it dating back to the 1880s.

Keynes and his fellow Cambridge economists were men of a single book, Marshall's *Principles* published in 1890, supplemented by a few other published fragments and 'oral tradition', mainly about his theory of money ... Keynes was deeply influenced by Marshall. He wholly endorsed his view of economics as 'not a body of concrete truth, but an engine for the discovery of concrete truth', a view based on Marshall's perception that the 'habits and institutions of industry' do not stay constant ... Keynes was permanently influenced by Marshall's technique of dealing with time (Skidelsky 1992: 418).

Keynes fully embraced Marshall's inclination to forgo analytical rigour in pursuit of increased realism. He shared Marshall's political perspective on individual liberty, tempered with the desire to actively promote social justice. Most significantly, Marshall's methodological perspective was to play a critical role in the Keynesian treatment of effective demand, where Keynes's application of *partial* analysis was used to reduce the complexities of time and uncertain knowledge in economics to manageable proportions. The role of money and the indeterminacy of expectations associated with uncertainty could not have been effectively portrayed within the general equilibrium framework envisaged by Hicks and embodied in the neoclassical synthesis models that followed, constructed within the 'Walrasian', as opposed to the Marshallian, tradition.

7 Conclusion

Paul Samuelson (1967: 111) asserted that 'much of the work [in economics] from 1920 to 1933 was merely the negative though necessary task of getting Marshall out of the way'. As suggested in Shove's 1942 centenary article on Marshall, this 'negative task' appears to have been largely accomplished: 'Whatever the explanation, the fact is plain. In those parts of economics with which the *Principles* was concerned, there has been a distinct reversion to Ricardo's method and away from the Marshallian blend of realism and abstraction: a return to the mechanical as against the biological approach' (Shove 1942: 323).

The 'negative task' was most directly prosecuted through the Marshallian cost controversies of the 1920s, culminating in the symposium on 'Increasing Returns and the Representative Firm' commissioned by Keynes for the *Economic Journal* in 1930. These controversies, originating from the criticisms emanating from the 'Empty Economic Boxes'

debates of the early 1920s involving John Clapham, Dennis Robertson, and Pigou, came to a head with Sraffa's (1926) well-known critique, centred on the restricted role that could be permitted for increasing returns because reductions in costs connected with an increase in the firm's scale of production (internal economies) had to be put aside as being incompatible with competitive conditions. Consequent to the exchanges in the symposium, Sraffa (1930: 93) concluded that it was Marshall's theory that should be discarded. However, while Sraffa's critique may well have exposed the futility of the 'astonishing gymnastic contortions' of much of the Marshallian analysis of the 1920s, the object of Sraffa's critique was somewhat distanced from what was mistakenly portrayed to be 'Marshall's theory'.

Marshall had no interest in defending a theory of *competitive* equilibrium in the presence of increasing returns to scale, which were both internal and external to the firm. Rather, the controversies were founded on a caricature of Marshall's own theory, with superfluous elements such as perfect competition and Pigou's external–internal economies and equilibrium firm shrouding Marshall's key conceptualisations of knowledge-based increasing returns and the representative firm mirroring industries as populations of heterogeneous firms at various stages of development.¹⁶ The extent to which the questions left open by Marshall were addressed was largely dependent upon the degree to which they were amenable to treatment with the 'algebra' and diagrams refined within the static framework. In a sense, Marshall's *Principles* had been turned upside-down, with the formal representations lurking in the footnotes and appendices of the *Principles* elevated to the main text of the Marshallian literature. Those attacking and defending Marshall's economics had in effect missed the 'dark spot' in Marshall's economics, that being the struggle to represent within an equilibrium framework the outcomes of economic actions which are acknowledged to proceed within an evolutionary setting. This dark spot, together with its implications, was most effectively uncovered in the contributions by Joseph Schumpeter (1928) and Allyn Young (1928); however, the significance of these arguments was overlooked by the Marshallians who had by this time become committed to the static equilibrium theory pathway. Those parts of Marshall's economics that could be used as 'handy tools' in this endeavour were enthusiastically extracted and applied, while the ambiguities and apparent inconsistencies created from the methodological dilemmas that

¹⁶For further discussion, see Prendergast (1992), Hart (1996), and Blankenburg and Harcourt (2007), with the opposing interpretation stated most directly in Stigler (1990).

confronted Marshall's structure were cast aside. In this setting, as described by Samuelson (*ibid.*: 112), Marshall was seen as 'a victim of what the modern Freudians call self-hate. He was a good chess player who was ashamed of playing chess, a good analytical economist who was ashamed of analysis'.

However, during recent decades, the 'traditional' assessment of Marshall's economics developed along the lines enunciated by Samuelson has been strongly challenged by a growing number of Marshall scholars. This re-evaluation and rehabilitation of Marshall's work has been founded on detailed archival studies of his unpublished writings and correspondences, with the key findings and interpretations conveniently assembled in the recently published edited volume, *The Elgar Companion to Alfred Marshall* (Raffaelli et al. 2006).¹⁷ The general thrust of this 'new view' of Marshall's economics is described succinctly in the following terms by one of its leading proponents:

The 'new view' regards the latter [Marshall's thought] as being chiefly a scientific treatment of human history, based on a philosophical vision which is almost an outgrowth of a personal variant of evolutionary psychology extended to social and industrial organisation. For those involved in this historical revision, the most surprising aspect has been the ease with which, set against the backdrop of his general philosophy of history, different parts of Marshall's economic canon—partial equilibrium and period analysis, as well as the theory of industrial organisation in its connection with social and ethical progress—fit together like those of a puzzle that form a coherent pattern. Not bad for an author who had a reputation for lacking in rigour and for a certain fuzziness! (Dardi 2010: 522).

It may well be the case that this is the legacy Marshall would have preferred to have left the profession in terms of the development of his discipline; however, in the final analysis, his reluctance to provide doctrinal assertiveness instead amounted to an abandonment of leadership in advocating his ideas amongst his direct Cambridge disciples and their critics. Perhaps a fitting conclusion is found in Joan Robinson's (1953: 259) remark that 'the more I learn about economics the more I admire Marshall's intellect and the less I like his character'.

¹⁷ An earlier collection of articles in this tradition can be found in Arena and Quere (2003), with Raffaelli's (2003) detailed account of the evolutionary dimensions of Marshall's writings being particularly influential. This literature has fostered a revival of interest in Marshall's theory of industrial organisation which now proves itself to be of long-lasting relevance; see for example Raffaelli et al. (2011). The growing recognition of Marshall's importance amongst modern evolutionary economists is most distinctly portrayed in Metcalfe (2007). Many of these themes are discussed further in Hart (2012, 2013).

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17

Herbert Somerton Foxwell (1849–1936)

Rogério Arthmar and Michael McLure

1 Introduction

Herbert Somerton Foxwell was born in Shepton Mallet, civil parish of the Mendip district of Somerset, in South West England, on 17 June 1849, the eldest of the six children of Thomas Somerton Foxwell (1806–1866) and Jane Handcock (1824–1878), his second wife. Thomas Foxwell was a successful merchant who dealt in hardware, slate, and timber, although much of his wealth was wiped out during the Overend Gurney crisis of 1866. The infant Foxwell received his early education at home, and later attended the Wesleyan Collegiate Institute, now Queen's College, Taunton, graduating from London University in 1868. That same year, he entered St John's College, Cambridge, as student of the moral sciences and was awarded the Whewell Scholarship in International Law in 1872. In 1874, at the young age of 25, he was elected Fellow of the College and, the next year, appointed to examine in the Moral Sciences Tripos, remaining associated with St John's for over 60 years. With a distinctive affinity to teach, he became Lecturer at University

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College, London, in 1876, succeeding his friend William Stanley Jevons in the Chair of Political Economy there in 1881. A few years later, in 1896, Foxwell started lecturing also on Currency and Banking at the newly created London School of Economics. He died on 2 August 1936, in Cambridge, at the age of 87. About his teaching skills, it was once remarked that he had been an extremely reasoned lecturer, always concerned about the ongoing issues in economics: ‘Of incomparable lucidity, with an unsurpassed knowledge of economic literature, his ingenious mind was always alert to notice the chances and changes of present day life’¹ (*The Times* 1936: 12; see also Bowley and Freeman 2004; Koot 1987; Bonar 1936).

Foxwell lived all his adult life in Cambridge, first at St John’s and then, after marrying Olive May (1870–1930) in 1898, at 1 Harvey Road, close to the Keynes family. The most conspicuous trait of Foxwell’s scientific life, and the one which brought him eternal gratitude from his fellow economists, was his lifelong dedication to building a definitive bibliography of economics. A relentless book-hunter, his bibliomania was initiated at Jevons’s suggestion in 1875, which ultimately led to him putting at least three large collections together, comprising over 70,000 volumes. Foxwell not only chased down and bought the books, pamphlets, and manuscripts on economics that he was interested in, but also read, annotated, and catalogued each and every piece, a practice that, with time, made him a specialist in the history of thought from a wide variety of economic fields ranging from the seventeenth to the nineteenth century. His first major collection, with some 30,000 books, was about to be sold to an American library when, following an appeal to the public from the British Economic Association,² it was suddenly acquired by the Goldsmiths’ Company in 1901 for the sum of £10,000, and donated to the University of London in 1903. In Foxwell’s own words, the library was to ‘serve as a basis for the study of the industrial, commercial, monetary and financial history of the United Kingdom, as well as of the gradual development of economic science generally’ (Foxwell 1908a: 720). Foxwell kept working on the improvement of the Goldsmiths’ Library until 1814, when he began to assemble another collection, sold to Harvard University through

¹ Maynard Keynes described Foxwell’s lecturing style as being always directed to practical problems from a realistic perspective: ‘But he held that the reasoning must be applied, if it is to be fruitful, to a wide range of facts furnished by historical and contemporary experience, and not to simplified and artificial hypotheses’ (Keynes 1936: 592).

² The British Economic Association’s plea appeared in the 25 June 1901 issue of *The Times* in quite convincing terms: ‘No such library of economic literature has ever been formed before, and it is doubtful whether any future collector, however learned, leisured and wealthy, will be able to rival it’ (*The Times* 1901: 8).

the agency of Allyn Young in 1929, which forms the basis of the Kress Library of Business and Economics at that institution (Rogers 1986; Keynes 1936).

Although Foxwell excelled in his multiple endeavours as a lecturer and book collector, the unavoidable travelling around and time-consuming nature of these tasks compromised the extent of his intellectual activity. Most of his erratic work, composed of speeches, articles, and introduction to books, however, carried a sense of energy, erudition, and indeed heresy that, in the words of his contemporary Joseph S. Nicholson (1850–1927), made it seem as if he had brought back to life ‘the spirit of the old pamphleteers’ (Nicholson 1919: 323). So, for instance, we find Foxwell in 1884 editing and writing the introduction to Jevons’s *Investigations in Currency and Finance*, praising his late friend’s willingness to analyse economic phenomena based on real data and with the same inductive method so successful in physics (Foxwell 1884: xxv). In 1899, Foxwell composed a well-known introduction to Anton Menger’s *The Right to the Whole Produce of Labour*, where the British socialist literature of the nineteenth century is outlined and linked to Ricardo’s doctrine of value. In addition, the book is complemented by a full bibliography of British tracts on socialist proposals running from 1756 to 1885, along with a complete and chronological list of periodicals dealing with the social question over the same period (Foxwell 1899: i–x, 191–267). Yet, in his 1909 Preface to Andréades’s *History of the Bank of England*, Foxwell puts forth a strong defence of that institution’s policies during the restriction period of 1797–1815, when the Bank was not required to convert banknotes into gold, hailing its commitment to the stability of business instead of conferring priority to the stabilization of the exchanges (Foxwell 1909: vii–xxvi). In spite of the diversity of subjects dealt with in Foxwell’s papers, his main interests gravitated around the importance of the historical method to economics, banking and monetary issues, the instability of the economy and its effects on employment, and, lastly, the history of economic thought. Foxwell’s main contributions to each one of these subjects will be examined in the next four sections.

2 The Historicist Movement in England

When Foxwell started his lecturing activity in Cambridge, British political economy was going through a period of deep commotion. On the one hand, Jevons was proposing a more mathematical approach to economics while fiercely reacting against the received authority of Ricardo and Mill for their roles, according to him, in having ‘shunted the car of Economic science on to a wrong line’ (Jevons 1871: li). On the other hand, the disconnected but highly

vocal historicist movement in England, headed by William Cunningham, John Kells Ingram, Thomas E. Cliffe Leslie, and Arnold Toynbee, among others, was directing an all-out attack against the very foundations of the classical—and even the emerging neoclassical—orthodoxy. The historicists reacted, first, against the narrowness of the scope of political economy, calling for economic thinking to consider social factors that were so prominent in other fields of science such as sociology, biology, and morals. Second, they condemned the excessively universal and abstract Ricardian style of reasoning, based on self-interest and competition, for its not taking into account the role of non-economic motives and the ever-changing condition of society. Lastly, they argued that political economy should have a more practical character, looking systematically into matters of an applied nature (Schumpeter 1963: 821–824; Koot 1977; Coats 1954). Cunningham, Lecturer of economics and history at Cambridge from 1884 to 1897, writing in 1878 on the moral aspects of political economy, remarked that economic phenomena ought to be interpreted not as the outcome of one single factor, but more properly as the result of ‘many forces which are qualitatively, not merely quantitatively, distinct’ (Cunningham 1878: 372).³ Subsequently, Cunningham (1892) went on to attack the application of Marshall’s economics to the study of historical matters.

Foxwell was not opposed to abstract theory outright, as his praise of Jevons’s work testifies, but he was sympathetically inclined towards the precepts of the historicist movement. The 1887 article ‘The Economic Movement in England’, deemed by Maynard Keynes to be one of Foxwell’s best pieces (Keynes 1936: 592, fn. 2), begins by offering a somewhat overstated portrait of classical economics, accusing it of being a strong materialistic doctrine, of sacrificing national welfare to individual riches, and of relying on mechanical laws to describe the nature of human action as purely selfish. Mill’s contribution, Foxwell argued, had added nothing substantial to this picture, being more like the transfusion of ‘new wine into the same bottles’ (Foxwell 1887: 85). This kind of approach, he continued, could not fail to raise the opposition of the best minds in literary, artistic, and spiritual circles, and to bring economic science into disrepute in many quarters. Most importantly, the old doctrine of competition had been

³ Alternatively, as argued by Cliffe Leslie in his *Essays in Political Economy*: ‘The main questions respecting the influence alike of the “desire of wealth” and of expenditure and consumption are: To what kinds of wealth, what modes of acquisition, and what actual uses do they lead in different states of society, and under different institutions, and other surrounding conditions? To what laws of social evolution are they subject in the foregoing respects? On these points we learn nothing from abstract political economy’ (Leslie 1888: 171).

overshadowed by the advent of a new historical reality, marked by the rise of the working class, the end of the period of prosperity in 1874, when silver was demonetized, and the profound reorganization of industry on a worldwide scale towards a new monopolistic phase.

Three factors, in Foxwell's opinion, would bring about the downfall of the old laissez-faire doctrine. First, the rise of a truly scientific approach to economics, initially introduced by Jevons and improved by Marshall, characterized by the use of mathematics in economic reasoning, the exactness of definitions, and the setting of a clear boundary between theory and reality. Second, the strengthening of the historical approach, which emerged from the work of intellectuals like Cliffe Leslie and Henry Maine, from discoveries regarding the social implications of the evolutionary views in biology, and also from the German historical philosophy. In a clear depiction of this new historical perspective of society's dynamics, Foxwell wrote:

[T]he new school take comparatively little interest in the deductions, because they hold that the facts have not yet been carefully observed, that the assumptions have only a remote relation to the facts, that the facts themselves are in the process of evolution and change, and that the nature and direction of this social evolution are a far more important object of study than elaborate and complicated deductions (ibid.: 89).

The last factor to cause the decline of the classical school was, for Foxwell, the resurgence of the old British moral and humanist spirit, once so active against slavery and in favour of factory legislation, which now stood up once more against the materialistic bent of the times professed by the old economic doctrine. But Foxwell was also quick to dismiss any possible friction between these quite diverse views on the main object of economics, and immediately proposed a strategic alliance among the moralist, the mathematical, and the historical schools. That association was to be built upon a tacit compartmentalization of economic knowledge, that is, by the assignment of precise limits to each approach within the new science: 'If the theorist, the historian, and the moralist keep strictly within the bounds of their several inquiries, they will aid and not conflict with each other' (ibid.: 91). The interesting aspect to be noted here is that Foxwell was recommending essentially the same solution, devised by his Cambridge colleague, John Neville Keynes, in his 1891 *The Scope and Method of Political Economy*, to the general state of dissension pervading economics. That book proposed a clear separation among the distinct fields of economic inquiry as the most effective medicine for methodological disputes. Commenting on the frequent confusion between theoretical propositions,

moral considerations, and policy prescriptions, Neville Keynes remarked: 'In other words, the theoretical and practical enquiries should not be systematically combined, or merged in one another, as is maintained by those who declare that political economy is an indivisible whole of theoretical and practical investigations' (Keynes 1897: 65).

It must also be recognized that Foxwell's readiness to directly attack the Ricardian heritage may have played a role in the demise of his friendship with Marshall, who considered his own work a natural evolution of the classical tradition in British political economy (Shove 1942). Furthermore, by the turn of the century, even work that was inspired by Marshallian methods was being considered as 'too Ricardian' by Foxwell.⁴ When Marshall's successor to the Chair of Political Economy at Cambridge was being considered in 1908, Marshall's surreptitious manoeuvres in the lead up to the ballot assured that his young pupil, A.C. Pigou, was elected to the post against the candidacy of long-time lecturer Foxwell, who felt betrayed by the outcome and thenceforward distanced himself from Marshall (Coase 1972; Coats 1972; Groenewegen 1995: 622–627).⁵ That, however, does not mean that Marshall was opposed to the historicist movement in Britain, at least in its most elaborated version which claimed an inevitable role for history in economics—as against the purely inductive empiricism of some more radical thinkers.⁶

3 Banking and Currency Studies

During the extended period from 1860 to 1913, banking and currency specialists were mainly interested in proposals related to bimetallism put forward in certain countries; the use of central bank discount rate as a means to achieve some monetary goals; and the most appropriate way of dealing with panics and crises. All these diverse aspects of banking analysis related, in

⁴When reviewing and reporting on Pigou's successful King's College Fellowship thesis (Pigou 1901), which was a Marshallian economic history of relative agricultural prices in the second half of the nineteenth century, Foxwell reported that Pigou was 'too much of a Ricardian; too much enamoured of his technical apparatus' (Foxwell quoted in McLure 2013: 275).

⁵After the episode, Foxwell acquired a distaste for Pigou which lasted all the way through the First World War and beyond. During Pigou's trials for exemption from military service, Foxwell, in a less-than-noble attitude, offered himself to fill Pigou's place at Cambridge in case the exemption that had been granted at the first trial was reversed (Aslanbeigui 1992). Also, Pigou's application for membership of the British Academy was denied until 1927 due, to a great extent, to Foxwell's opposition (Winch 2014).

⁶Marshall was not only content to express his sympathy for the historicist movement but also for the evolutionary approach to economics that draws upon biological analogy (Hodgson 2005).

direct or indirect form, to the convenience of the country possessing an elastic currency to avert a liquidity crisis and to attend as well the needs of business in general (see Mints 1945: 178–197).

Foxwell too had well-defined positions regarding the proper role of the banks in the economy. The subject is examined in his 1917 article ‘The Financing of Industry and Trade’, where he remarks that the chief challenge of modern times is to provide sufficient means to facilitate the consolidation of large corporations. Although he understood that the primary task of banking was the provision of finance to productive firms, he also mentions that British banking institutions had historically specialized in a very particular type of operation. As distinct from Continental banks, which had less strict regulations on the use of their reserves and were, therefore, more prone to embark on industrial enterprises, British banks had a peculiar preference either for short-term loans, in close connection with the bill market and the Stock Exchange, or else for operations related to foreign lending. The unfortunate consequence of this state of affairs, warned Foxwell, was that domestic industries and trade had to go without the necessary long-term funding from British banks. Considering that foreign banks had a more active interaction with their national industry, this situation meant that Britain was losing position in the international competition for new markets and the introduction of new technologies, while her domestic industry languished for lack of proper liquid capital:

The loss to our home industry is only too obvious. It cannot obtain from the banks the means of making adequate original installations, or the extensions and reconstructions made necessary by the progress of industrial technique, or by the developments of rival firms in other countries; nor, again, the large loans which are often, in practice, essential to the securing of big foreign contracts (Foxwell 1917 [1919a]: 128).

With respect to the workings of the British banking system under the pre-war gold standard, many voices advocated reforming the Bank Act of 1844, which had set very limited scope for discretionary action by the Bank of England. The main concern of bankers, and others, was the reduced size of the Bank’s gold reserves and the relative frequency and intensity of changes in its discount rate (Hume 1970: 127–133). Foxwell, in his 1909 article ‘The Bank Reserve’, was particularly alarmed by the fact that, while Continental central banks were increasing their gold holdings, Britain had remained in the same situation, with just a modest amount of gold held at the Bank of England. One of the drawbacks of such a situation was the high sensitivity of

the Bank's discount rate to small movements in its reserves. Moreover, given the substantial volume of foreign deposits in London banking houses, Britain was very much exposed to sudden and violent monetary turbulence which, through its effects on the cost of money, had been taking a heavy toll on domestic business and employment: 'There are scores of people today who could move a million or many millions of gold if they chose to do so. Is it right that our trade should be exposed to a tax of this kind by reason of such casual operations?' (Foxwell 1909 [1919]: 155).⁷

As already noted, Foxwell's 1909 Preface to *History of the Bank of England* praises that institution for giving priority to domestic stability over gold convertibility during the Napoleonic Wars. It is on this particular issue that Foxwell's concerns re-emerge with Ricardo who, in his 1809 pamphlet 'The High Price of Bullion', had criticized the Bank for its excessive fiduciary issue in the restriction years. Ricardo characterized the behaviour of the Bank as the root cause of the depreciation of the pound in foreign exchanges and the increased price of gold, a process which had created an inflationary process that he condemned, in the most strenuous terms, for its hurting public and private creditors alike.⁸ Foxwell, after censuring Ricardo and his followers on this matter, congratulated the Bank and its directors for having steered the nation through a most challenging period without sacrificing the British economy in order to correct the premium on gold:

No doubt the Bank had the defects of its qualities; it may have laid rather too much stress upon the urgency of accommodating the trade and commerce of the country: but if so, its vices leaned to virtue's side. Its principal difficulties were due to its unwavering loyalty to the State; and to its endeavour, so far as lay in its power, to avert undue pressure on the commercial community. These are the ends for which a National Bank exists (Foxwell 1909: xxii).

A series of measures to provide the banking system with a large gold reserve and a more elastic currency, capable of better managing panic episodes and making the business world less vulnerable to financial crisis, was conceived

⁷Or still, when commenting on Mr George Goschen's proposals to increase the Bank of England's gold holdings: 'The most conclusive proof that our present reserve is inadequate is to be found in the nervous state of the money market in ordinary times, and in the fact that comparatively small withdrawals of gold, of a kind to which the London market is constantly liable, will produce unforeseen and mischievous advances in the current rate of discount' (Foxwell 1892: 142).

⁸As expressed by Ricardo: 'What security has the public creditor that the interest on the public debt, which is now paid in a medium depreciated fifteen per cent, may not hereafter be paid in one degraded fifty per cent?' (Ricardo 1809 [2005]: 96). The same position, of stressing the need for restraint by the Bank in order to keep the exchanges stable, would be reinforced in 1810 by the report of the Bullion Committee (Fetter 1965: 39–43, 49–54).

by Foxwell in his 1909 article ‘The Banking Reserve’. First, he advocated greater transparency and publicity of banking statistics, including the Bank of England’s gold holdings, for he understood that such practice would not only curb speculation but would also allow the public to keep strict control over the financial institutions. Second, there might be a second large reserve held privately, made out of contributions by the joint-stock banks, to be used in times of stringency by the Bank of England under the condition of it paying some progressive interest rate on every £5 million withdrawn. Lastly, the government should build a considerable reserve to help the savings banks in case of a war-emergency run on deposits (Foxwell 1909 [1919]: 160–170).

In an earlier article on Goschen’s monetary proposals, Foxwell had also indicated that the introduction of £1 banknotes was another practical measure to reduce the transactions demand for gold coin and would thereby allow greater use of sovereigns as Bank of England reserves. These notes would have the additional benefit of a lesser likelihood of being presented to the Bank for conversion to gold (Foxwell 1892: 145–146). In his 1895 article, published in the same year as the pamphlet, ‘A Criticism of Lord Farrer on the Monetary Standard’, Foxwell pressed the case for the restoration of bimetallism on an international scale in order to avoid the deflationary tendency of the gold standard, since the supply of silver had been growing uninterruptedly while its price had remained steadier than the wildly fluctuating price of gold (Foxwell 1895: 4–6; for an insightful discussion of the Great Recoinage of 1696, see Foxwell 1896). At the end of the First World War, during which Britain suspended its commitment to the gold standard, Foxwell also appeared before the Cunliffe Committee (Moggridge 1972: 18), whose recommendations paved the way for the eventual reintroduction of the gold standard in Britain in 1925.

4 Competition, Monopolies, and Employment

In line with his personal view about the true nature of economics, Foxwell developed a quite realistic approach to dealing with the evolution of industrial markets. For him, classical economists had lost touch with reality by preaching around the alleged benefits of an all-powerful competition which actually no longer had any consistent correspondence with the facts. Writing in 1888, in a paper about the relations of the State with large companies, Foxwell held that the abolition of the old type of monopolies, based on privileges and political power, allowed the free play of the forces of competition which, after a while, unleashed the unrestrained growth of a new kind of monopoly, built over the foundations of abilities, possessions, and opportunities (Foxwell 1888 [1919]: 264–265).

Competition, for Foxwell, meant open economic warfare, having as its unavoidable outcome the survival of the fittest. He very cleverly indicated that even small advantages by a few firms can lead them to grow into a full monopolist, for every victory makes the victor stronger and the loser weaker. Furthermore, advances in transport, communications, and finance have helped the constitution of monopolies operating on an international scale.⁹ Foxwell admitted, though, the existence of some economic limits to this process, mainly due to the difficulties of managing very large enterprises, coupled with the sometimes swift transformations in the economic environment:

Then there is a constant change in the economic situation, giving new men a chance. The rapid progress of science, the incalculable caprices of fashion, the altered habits of the public, all disturb the routine of business, and help to dislodge the monopolies. The imperfection of heredity, too, places a term on the most successful enterprises. A father may leave his property to his son; it is a very different matter to leave him his business (*ibid.*: 266).

The most conspicuous advantages accruing to a monopoly, as Foxwell saw them, were the economies in administration, the concentration of knowledge and skills, the avoidance of wasteful costs of competition, such as litigation and advertisement, and the end of industrial anarchy. The consumers benefited also from the better quality of products and more stable prices, while public opinion would hold a strong sway over the monopoly actions, for the company's reputation must always be kept above suspicion. However, abuses would inevitably happen, as in the fixing of excessively high prices, with corresponding large profits, or in the economic and political tyranny exercised over the employees and even over entire towns in some cases, not to mention the practice of large-scale corruption and bribery of public officials. The general interest, though, would be better served not by unconditional State intervention, but rather by the full disclosure of all economic transactions, giving public opinion the knowledge to best direct each one's expenditures towards this or that product, one of the most powerful ways, according to Foxwell, of containing the monopolies' reproachable actions. Whenever necessary, control should be exercised by local trade bodies familiar with the situation instead of by some representative of central authority (Foxwell 1888 [1919]: 270–275).

Foxwell elaborated further on this same theme in a speech delivered at the Royal Institution on 19 April 1917 entitled 'The Nature of the Industrial

⁹ See Foxwell's observations on the history of Barclays Bank for a conspicuous case of a successful banking amalgamation (Foxwell 1908b).

Struggle'. Competition, he said, should not be interpreted as a universal principle, but as a state of things that happens under some historical conditions. Although Smith and others believed it to be a fair process, modern industry gives evidence of three basic forms of competition: first, through the search for greater efficiency; second, through the pursuit of profits at all costs and means; and, third, through the ruthless destruction of competitors, be that at the domestic or international level. The first case, of honest competition, unfolds itself necessarily into the emergence of a monopoly. The second and third cases, of dishonouring competition, make use of adulteration, low quality materials, espionage, corruption, and a whole assortment of furtive means, leading to the survival not of the fittest, but of the most deceptive firm. Two codes of morals, therefore, govern these two radically distinct competitive activities, the consequence more often being a sort of Nash equilibrium, with worse practices commonly displacing the better ones: 'There is a kind of Gresham's law at work in business', told Foxwell to his audience and 'The best men often find their own standards degraded by an irresistible pressure of circumstance ... This is the "iron law" of the business world under unregulated competition' (Foxwell 1917 [1919b]: 78).

The main flaw of dishonourable competition lies in the circumstance that the most ruthless, and not the fittest, is the very one to survive the struggle, so that instead of evolution, the process ends up in degeneration. As such, the State should not keep itself peacefully away from the market, as recommended by laissez-faire economists, but must step resolutely into the arena to make sure that only fair competition prevails. Foxwell proposed as the most important measure here the adoption of a National Trade Policy (NTP) to protect domestic markets from foreign aggression. Moreover, he added, large sections of industry, trade, and transport were under the control of some form of cartel or combination, with powers comparable only to the State, standing in need, therefore, of some form of locally managed regulation. The NTP would also have to address the problem of the isolation between science and production in Britain, a combination by then already well advanced in Germany's large corporations, for otherwise the nation would lose its prominence among the industrialized economies. Once again, Foxwell is keen to alert his readers to the dangers of big business, such as State-aided predatory competition, seconded by all forms of corruption, and even military wars, orientated to the capture of foreign markets or to the establishment of colonial enterprises as a way of securing the provision of raw materials. In any event, he argued that these perverse side effects of monopolies could be minimized by State-controlled but self-managed organizations, thus avoiding the build-up of a gigantic bureaucratic structure, so that the positive aspects of modern industry could come to

the forefront more easily (ibid.: 90–96). Foxwell considered himself to be the first person to put forth this particular reading of the reality of his day, since the British mind, according to him, was completely subjugated by the alleged benefits of competition:

English feeling is well expressed in our Common Law, the whole spirit of which is adverse to combination as ‘in restraint of trade.’ But it is beyond doubt that unregulated competition has destroyed more honest trade than all the combinations in the world. Even in England, legislation has been more occupied in restraining competition than monopoly. The social history of the nineteenth century has been one long protest, one great legislative reaction, against the mischiefs of unregulated competition. I think I was perhaps the first English-speaking economist to put in a word in defence of business combinations (ibid.: 88).

From some lectures delivered at the Industrial Remuneration Conference in the summer of 1885, in Scotland, Foxwell put together the following year a booklet entitled *Irregularity of Employment and the Fluctuations of Prices*, dealing primarily with the question of the stability of employment, which he judged the worst evil of modern society. Considering then the recurrence of economic fluctuations, Foxwell lists a number of difficulties arising out of them, such as the lack of protection for the weaker classes in times of industrial distress, the waste of capital and skills during the transference of resources among the sectors of the economy, the trail of suffering left behind by the introduction of machinery and the competitive struggle, and, lastly, the reduction in wages in times of unemployment (Foxwell 1886: 10–24).

The reasons for the economic instability of contemporary capitalism, as understood by Foxwell, lie, on the one hand, with the nature of the contemporary monetary system, and, on the other hand, with a general disposition to disturbance by modern industry due to both the advanced stage of the division of labour and the development of credit. In regard to the first factor, Foxwell blamed the scramble for gold by Continental central banks as the main reason for the shortage of money and deflation all round. Price reductions, although prima facie beneficial to labourers, would be often associated with a depression in industry and trade which increased unemployment. As costs do not fall as quickly as prices, profits are squeezed by the lag between receipts and expenditures, while debt contracts have to be honoured with a more valued currency, putting in this way an additional burden on the producers’ shoulders. The second reason for the deflationary pressure is

derived from the high specialization of industry, with any misdirection of resources being amplified by speculation and easy credit, a process often prone to end in a financial crisis (*ibid.*: 38–56):

Industry and trade have worked at a degree of high pressure previously unknown, and population and wealth have increased; but with all this there has been enormous waste, both of men and material, unheard of suffering and squalor, debasement of commercial principle, adulteration of products, and degradation of taste. Industry has made great strides in its methods and results under the fierce competition. But, after all, it is not to this anarchical struggle that we owe the greatest achievements of the age. It will be distinguished in history not so much for the rapid increase of material products, as for its remarkable discoveries in natural and physical science (*ibid.*: 71).

Along with this argument, Foxwell elaborated a complex programme of reforms to remedy price fluctuations and the instability of markets, comprising altogether an agenda which he subsumes under the heading of ‘organization’. Besides the already mentioned restoration of bimetallism and the public control of monopolies, he proposes an ample set of measures directed to provide transparency and information to the public. That would be the case for the compilation, by State agencies, of aggregate statistics on employment, consumption, income, and the distribution of wealth, to be made available for all public bodies and boards of trade. Speculation and corruption, according to Foxwell, thrive on ignorance and disinformation, secrecy being the cover of all kinds of abuse in modern society, the essence of democracy laying precisely on publicity, which makes public interest supreme. In regard to lessening business fluctuations, Foxwell conceived of the introduction of some kind of organization, like the medieval guilds, uniting masters and men of the same trade, who would work as one to keep prices steady, moderate competition, and regulate their common output. These guilds, additionally, would be in charge of providing technical education and apprenticeships, quality control of products, and the punishment of falsifications, while searching for better channels of trade with a view to eliminating the middleman. Lastly, to address the unemployment issue, Foxwell suggested, first, the creation of a Bureau of Labour to assist the unemployed and provide them with information on work opportunities, savings, and retirement; second, the expansion of the use of long-term wage contracts, with sliding scales and standard lists; and lastly, the introduction of permanent staff on public agencies, museums, hospitals, and the like, in order to increase the overall stability of employment (*ibid.*: 73–90).

5 History of Economic Thought

Surely, the most enduring contribution of Foxwell to the field of economic thought was his lifelong and relentless dedication to building a definitive compilation of the literature of economics, encompassing the works not only of the luminaries of political economy, but also of almost all lesser known authors or even of completely obscure and popular writers. As reported by Foxwell in his own description of the origins and development of the Goldsmiths' Library, he always endeavoured to acquire works that covered all sides of a controversy, no matter what had been at stake or the different viewpoints involved. In total, the resulting pamphlets, broadsides, and tracts that he collected from the most diverse sources accounted for almost two-thirds of the complete Goldsmiths' Library.¹⁰ In Foxwell's words:

Whatever the subject in question, a collection made on these lines would necessarily include a large quantity of tracts and fugitive publications. But in economics, more perhaps than in any other subject, its historical sources are largely of an occasional, non-formal character; and the brief contributions of practical men are often of much greater value than the more systematic disquisitions of professed writers (Foxwell 1908a: 721).

Before going into Foxwell reflections on the history of economic thought, however, it might be opportune to mention his personal views on some of his contemporary economists.

Marshall, of course, comes first, not only for his prominence at the time, but also for the turbulent relationship between these two Cambridge professors. In Foxwell's 1887 'The Economic Movement in England', Marshall appears three times, two of which place him together with Jevons, who are both portrayed as the true pioneers of scientific economics in Britain. In the third mention, Marshall's *Economics of Industry* is hailed as an important work in the reconstruction of economics in Britain, being deemed a book accessible to the layman and without a doctrinaire approach to applied problems. Besides that, Foxwell reminded the reader that Marshall had a large personal influence in academic circles throughout the country, half of the economic chairs in Britain being occupied by his pupils (Foxwell 1887: 88, 91, 92). However,

¹⁰The once curator of the Kress Library, Ruth Rogers, wrote the following about this second collection put together by Foxwell: 'The major categories of the collection are political economy, commerce, finance, taxation, money and banking, trades and manufactures, transportation, labor, socialism, and the economic aspects of agriculture. The published materials span the years from 1474 to 1850 and includes works in all Western European languages' (Rogers 1986: 282).

as alluded to above, Foxwell's deferential posture towards Marshall changed radically in the early years of the twentieth century, especially after Pigou's 1908 election to the Chair of Political Economy at Cambridge (Groenewegen 1995: 670–679).

It has also been suggested that Foxwell was one of Maynard Keynes's major influences (Koot 1977). In 1913, Foxwell wrote a review of Keynes's just released *Indian Currency and Finance*. After some considerations on the benefits of bimetallism, Foxwell praises the book as a fine example of how economic science should be presented, that is, by keeping theory behind the scenes, dispensing with technical terminology or fancy diagrams, and displaying instead a sober handling of statistical work applied to practical issues. In the end, Foxwell characterizes Keynes as one of the leading economists of the time, even comparable to Jevons, with the book under review showing lucidity, sagacity, and caution at the same time, along with the facility to be read by the public in general: 'The author, indeed, gives proof of a remarkable and very rare union of qualities which place him in the front rank of living economists. He combines with first-rate analytic power and grasp of principle a thoroughly realistic bent and wide familiarity with business and administration' (Foxwell 1913: 572).

As for Jevons, Foxwell nurtured a mix of friendship and admiration. Jevons was actually the one who introduced Foxwell to the activity of book hunting, when the former bought a copy of Lardner's *Railway Economy* in 1875 (Foxwell 1908a: 720). In his introduction to Jevons's *Investigations*, Foxwell commends his late friend for dealing with a most pressing issue, namely the instability of the economy, which was at the very foundation of many modern economic problems. In addition, stated Foxwell, Jevons had done this by striking the right balance between theory and practice, particularly through the judicious use of pertinent statistics to check his forecasts (Foxwell 1884: xxiv–xxv). Setting himself away from pure empiricism, Foxwell defends Jevons's theory of business cycles against the critics by calling attention to the mass of data amassed in the book and to the sheer impossibility of taking all causative factors into account in any explanation of reality: 'The truth is that these "practical" objections, pushed to their logical conclusion...would confine us strictly to the observation of isolated and, therefore, unintelligible facts' (Foxwell 1884: xxxiv–xxxv). Still, Jevons's *Theory of Political Economy* is seen by Foxwell as the opening of a new era in English economics giving, on the one hand, organic unity to economic science through the formal precision of definitions and assumptions while, on the other hand, setting strict boundaries to both theory and practice, a crucial difference often neglected by the old doctrinaire economists (Foxwell 1887: 88).

William Cunningham, who died on 10 June 1919 in Cambridge, was the subject of Foxwell's obituary in the pages of the *Economic Journal* in September of the same year. For Foxwell, Cunningham, throughout his academic life, always stood against the individualistic doctrine of laissez-faire economists, being more properly a defender of organizations such as the family, the guilds, and the realm, which he thought essential to the economic life of the nation. In his role of full-time historian, Cunningham, as reported by Foxwell, was mostly resented by his Cambridge colleagues for his contempt towards economic theory, a position which Foxwell himself had fought against in the early days of their friendship. As the years passed, though, Foxwell recognized that he had moved closer to Cunningham's standpoint, that is, of total rejection of a theory that placed self-interest as the supreme guide of human behaviour (Foxwell 1919: 388–389). Economic phenomena, for Cunningham and, admittedly for Foxwell, should be studied as the outcome of a complex interaction of human motives, involving not only material reasons, but also, and sometimes most importantly, ethical and psychological considerations. Cunningham's lasting legacy, concluded Foxwell, lay in him raising British economic history to distinction within the nation's academic world (*ibid.*: 390).

However, perhaps the most articulate of Foxwell's presentations on the history of economic thought appeared in his 1899 introduction to Menger's *The Right to the Whole Produce of Labour*. At the very beginning of his argument, Foxwell highlights what he considered to be the theoretical propensity of classical economists to neglect the aspects of economic action connected with the law, customs, and history. This most regrettable state of affairs, though, had been shaken by the advent of the Historical School, which highlighted the importance of positive law to the progress of society. The notions of ideal right and fairness, according to Foxwell, although repealed in some epochs, are always ahead of history, being the prime cause of change in habits, thinking and the law. In his own words: 'It might with equal truth be said that the equity of one age becomes the law of the next. If positive law is the basis of order, ideal right is the active factor in progress' (Foxwell 1899: xi).

The key for social stability, in Foxwell's particular interpretation of social dynamics, lies in the gradual development of the ideas of right, and the concomitant development of positive institutions framed by these ideas. This proposition, however, does not mean, for Foxwell, that the ideas about the ideal right are always correct or free of contradiction. He understood this task as the most difficult one in view of the fact that few socialist proposals had been put into practice, particularly those of a more radical nature. In England, however, cradle of the modern industrial world, the socialist thinkers had addressed almost every malady of the capitalistic society, advancing all kinds

of corrections and changes such as co-operativism, land reform, factory laws, trade unions, and even the revolutionary overthrow of the existing economic system (ibid.: xxv–xxix).

Making use of his vast dominion over the pertinent literature, Foxwell travels easily across the works of the main British reformers of the late eighteenth century and the whole of the nineteenth century. William Godwin (1756–1836) is considered to be the head of the reform movement, his *Political Justice*, owing to the combination of the ‘purest communism with the most individualistic anarchism’ (ibid.: xxix), being too optimistic and naive to exert any appeal to the revolutionary mind. William Thompson (1775–1833), for his part, is deemed to be the chief of the English Socialist School, due to the wide influence of his writings and his lifelong dedication to the movement. Like Godwin, Foxwell sees Thompson as excessively reliant on voluntary methods as the proper way to achieve that social equality so cherished by the critics of capitalism. This aside, Thompson’s lasting contribution to the socialist movement was to raise to centre stage of political economy the question of the most appropriate distribution of income (ibid.: xxxviii–xlvii). Less sympathetic towards orthodox classical economists, Foxwell reserved for John Stuart Mill (1806–1873) nothing less than the epithet ‘soporific’ (ibid.: lxxviii) for his overall negative influence by instilling in his readers a sense of finality. As for the father of British co-operativism, Robert Owen (1771–1858) is interpreted by Foxwell as the one person who really took the socialist ideal from books to the streets, making it the basis of a popular agitation. All advances in British social life of the time, from popular education to trade unionism, co-operativism, factory legislation, and sanitary reform, had been due, according to Foxwell, to Owenite campaigning (ibid.: lxxix–lxxxvii).

Foxwell also commented on the contributions made by other British socialists of the nineteenth century, such as Thomas Hodgskin (1787–1869) and John Francis Bray (1809–1897) (ibid.: lv–lxxi). However, the main recurring theme of his introduction to Menger’s volume revolves around the conception that although Owen could be named as the man who took the socialist ideal to the popular mind, Ricardo was really the one who provided the intellectual framework to the British socialist rhetoric. For Foxwell, almost all nineteenth-century socialist writers, even the revolutionaries, underpinned their critical arguments with portions of Ricardo’s writings about the class antagonism nurtured by the direct opposition between profits and wages.¹¹ The instances

¹¹ Modern studies, though, have shown that much of the nineteenth-century literature on socialism was influenced by the works of Smith (Thompson 2002: 82–110) and Owen (Claeys 1987: 130–165). Foxwell here does not take into account how difficult it was, even for the most loyal Ricardian, to master

where Foxwell repeats this claim are quite numerous, so to quote just one of them here will be illuminating enough:

It was Ricardo, not Owen, who gave the really effective inspiration to English socialism. That inspiration was stimulus indirect and negative, but it is unmistakable. Thompson and the rest took for granted the accuracy of Ricardo's unfortunate and strained deductions, and quote him as an unquestioned authority. Finding that certain of his conclusions were abhorrent to their sense of right, and assuming that he had taken the existing conditions of society as his premises, they naturally directed all the force of their attack against these conditions. This was the real intellectual origin of revolutionary socialism, and it is for this reason I have called it Ricardian (Foxwell 1899: lxxxiii; see also xl, xli, lvi, fn. 1, lxxi).

Foxwell closes his presentation by highlighting the key contradiction in socialist thought, that is, the fact that the labourer should have the right to his whole product but in a system where the division of labour makes it impossible to ascertain how much each one has actually done in a complex productive process. This conceptual difficulty, explained Foxwell, can only acquire some meaning at the aggregate level, so that any socialist discourse tends to end up in a plea for equality and communism. An effective alternative to a revolutionary change, however, would be the gradual implementation of social legislation and progressive taxation to promote a better distribution of unearned incomes (*ibid.*: cv–cx).

6 Conclusion

On balance, Foxwell may be characterized as an economist of transition in a period of the fast decline of the classical tradition and the rise of the marginalist and the historical schools. He was always at his best when directing his erudition and sharp intellect to point out the weaknesses and limitations of the common tenets of the prevailing political economy. His realistic criticisms towards the orthodox conception of *laissez-faire*, the detailed specification of its destructive nature, and the recognition of the social costs brought about by the competitive struggle were too convincing to not have an impact on his contemporaries. Perhaps this influence was more profound on the young Keynes, who could not have failed to notice the unwavering concern of

the complex intricacies of Ricardo's labour theory of value (see Peach 2009: 145–240), while Smith's and Owen's theories were distinctly simpler than Ricardo's and, therefore, easier to assimilate and diffuse.

Foxwell with the evil effects of unemployment and the necessity of some form of measuring aggregate economic indicators. Indeed, Foxwell's interest in industrial fluctuations and their impact on employment and the poor resonated across the entire Cambridge tradition, including through Pigou's definition of welfare improvement (Pigou 1912: 20–32).

On the positive side, Foxwell's remarks on the importance of monopolies in modern society were essentially accurate, although not pervasive enough to move him towards a more comprehensive view of their action and how to properly tackle this new type of market structure. His progressive distancing of pure theory and growing attachment to history, however, drove him towards the past in search of new solutions for the challenges of his day. Caught between these conflicting trends, he could only conceive of restoring both bimetallism and some modern version of the medieval guilds as the most proper remedies against the problem of economic instability. He never thought of abandoning the metallic monetary standard or of using public works as a means of combating unemployment. Notwithstanding this, and in view of his extensive lecturing engagements and vast bibliographic legacy, Foxwell's contribution to the economic debates of his time, like that of many of the pamphleteers of the past whom he admired so much, has probably not received the recognition that it warrants.

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18

John Neville Keynes (1852–1949)

Rita McWilliams Tullberg

1 Introduction

In 1925, John Neville Keynes, at the age of 73, resigned from the office of Registrary, the most senior administrative position at the University of Cambridge. He had held the post for 15 years. To mark his retirement, the Master and Fellows of Pembroke College and his many colleagues and friends subscribed to have two portraits painted of him, one to be kept by the University and hung in Pembroke and the other to adorn the walls of that iconic address, 6 Harvey Road. The finished portraits, painted by Sir Gerald Kelly, were handed over in a ceremony in Pembroke in 1927. In thanking his well-wishers, Neville Keynes spoke of a life spent working for the University: ‘I have always thought myself fortunate in being able to spend my life in University work. I cannot imagine any other surroundings that would have been so congenial to me, or where I should have had such consideration and kindness as have always fallen to my lot here’ (Neville Keynes quoted in Florence Ada Keynes 1950: 109). The pleasure was reciprocated. Many members of the University acknowledged their gratitude for the work Keynes had done on their behalf in maintaining the smooth running of the administrative body that was responsible for the expansion and modernisation of teaching and research in Cambridge. As the *Cambridge Review* wrote: ‘No University official has been more loved and respected in the memory of man

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than Dr J. N. Keynes who retired from the office of Registrar at the end of 1925' ('News and Notes', 11 March 1927).¹

However, the University Registrar was not simply a retreat post taken on by an ageing academic tired of teaching and examining. Neville Keynes had dedicated over 40 years to the administration of the University, first as the Assistant Secretary and later Secretary to the Local Examinations Syndicate (LES) from 1881 to 1910, and from 1892 as Member and unpaid Secretary of the Council of Senate,² in addition to membership and chairmanship of various faculty boards. Why then is there an entry for the University's top 'civil servant' among this collection of Cambridge economists? Was Neville Keynes's association with Cambridge economics anything more than that of the father of the University's brightest economic star, John Maynard Keynes? A brief sortie into the world of academic reform is necessary to understand this conundrum.

By the mid-1800s, it had become obvious that Cambridge must change. It was no longer reasonable for the University to draw its students from a narrow band of male, public school-educated Anglicans who spent three years honing their skills in mathematical manipulation which they then followed with a career in the church or possibly the law.³ Students needed to be prepared for the challenges of scientific industrialism, foreign trade, the administration of a developed domestic market economy and of an expanding Empire. Younger universities and university colleges were being established that offered degree courses in applied sciences, languages, economics, and engineering—in short, those subjects that Cambridge ignored. Reforms were to transform mid-nineteenth century Cambridge from a collection of independent, medieval, monastic seminaries into a more homogeneous University with first-class teaching and research in the natural and social sciences. Economics as a subject of academic study in Cambridge would not be as we know it without reforms in the University's religious affiliation and celibacy requirements, in the subjects offered for degrees, the expansion of

¹ Another speaker at the presentation referred to the great work that Dr Keynes had done for the University and said that he had changed a regime of autocracy at the Registrar to a kindly welcome which some of them in the early days had hardly hoped to see.

² The Council was the elected 'Cabinet' of the Senate, the University's governing body. Keynes was immediately put on eight sub-Syndicates and in 1893 became Secretary to the Council, a task that was later amalgamated with that of Registrar.

³ Most students at this time did not attempt a Tripos but satisfied themselves with a general studies course that led to the Ordinary Degree, also known as the Pass or Poll Degree: 48% of male students took the Ordinary Degree in 1887, this figure dropping to 43% in 1912. After the Second World War it became a practice that all students would read for Honours degrees. See McWilliams Tullberg (1998: Appendix B) for a fuller discussion of the Ordinary Degree.

the student base, and the organisation of teaching in what we now would call faculties. Neville Keynes's education and career serves as a useful illustration of how these reforms impacted on a scholarly young man seeking an education that was relevant to the social and economic problems of his day. After graduation, during his long Cambridge career as lecturer and administrator, Keynes contributed significantly to a climate in which the Economics Tripos could be born and Cambridge economics could achieve a high status in research and teaching.

Neville Keynes was born in 1852 in Salisbury where his father, John Keynes, after inheriting the family brush-making business handed it over to his brother and turned to his life's hobby, horticulture.⁴ The family were active Congregationalists, and Neville Keynes could expect to suffer socially and academically the disabilities still attached to Nonconformism by the largely Church of England establishment. In the balance was the support of a widespread circle of like-minded, Congregationalist friends and acquaintances reaching beyond Salisbury. John Keynes Senior was also a staunch and active Liberal and could count prominent Salisbury Liberals, such as William Fawcett and his son Henry, as good friends. Henry Fawcett was to play a major role in Neville Keynes's career, not least by his early death in 1884 that left the Chair of Political Economy at Cambridge empty.

Neville Keynes suffered a further disadvantage, for although he was the son of a nationally recognised dahlia expert, he was, nonetheless, the son of a tradesman. This accounts for both the emphasis given to and anxiety involved in the choice of education as the route for clever children of successful tradesmen to move into the ranks of the professional classes, and for the unfamiliar academic road taken by Neville Keynes to a Cambridge degree. Unlike his own children, Neville Keynes did not attend any of England's better-known public schools. He was sent at the age of eleven to Amersham Hall, 'a small, exclusive Dissenting Academy of about a hundred boys' (Skidelsky 1983: 7). There he showed promise in both classics and mathematics, but was of a 'nervous disposition' and inclined to lose marks in examinations because of anxiety. As an adult, this nervousness seems to have been accommodated by a 'belt and braces' approach to his career and a pessimistic outlook on the outcome of events over which he had no control.

Neville Keynes's first public academic achievement was to win a Gilchrist Scholarship in 1869 to University College, London. The College had been

⁴In *Kelly's Directory for Salisbury* 1875, Keynes & Knight, brush makers & turners can be found on Winchester Street and Keynes John, nurseryman & florist on Castle Street. John Keynes served his town as Mayor 1876–1877.

founded in 1826 as a secular alternative to the Universities of Oxford and Cambridge, offering broader and more practical courses of study in the arts and sciences than was the case at the ancient universities. It was particularly attractive to Nonconformists, who had been excluded by the Religious Test Acts from degrees at Oxford and Cambridge.⁵ Keynes entered University Hall, a non-denominational Hall of Residence attended not only by some of his Amersham school friends, but also by like-minded sons of Nonconformist tradesmen climbing the social ladder in a hostile academic and professional world.⁶

In July 1870, after a year in London, Keynes passed Part I of the London B.A. with First Class Honours, which gained him a further scholarship. The following year he was placed equal first in Part II, where he took Logic and Moral Philosophy Honours. The possibility of an academic career is said to have been mooted with his parents by Henry Fawcett, now Professor of Political Economy at Cambridge, and the path to a Cambridge college Fellowship was mapped out. After two false starts, the first the result of an ice-skating accident and then severe toothache, Keynes was awarded a scholarship to Pembroke College in June 1872 to read for the prestigious and competitive Mathematical Tripos (i.e. the Honours Degree examination) at Cambridge.

In October 1872, Neville Keynes became one of 24 freshmen at Pembroke College that year. His sights were set on a college Fellowship that would be awarded for a successful Tripos result.⁷ Within days, the narrow regime of the Mathematical Tripos for which he was being coached filled Keynes with despair. He began to discuss leaving Cambridge or dropping mathematics for 'morals' (the Moral Sciences Tripos). His friends advised him against it, as did Professor Fawcett. When Keynes's Tutor at Pembroke heard about his plans, he strongly advised Keynes to continue with mathematics, suggesting that he would surely come in the top twenty students and 'thought' that this would secure a Fellowship. Keynes wrote 'this would not satisfy me' (Diary:

⁵The Test Acts, requiring graduates to pledge allegiance to the 39 Articles of Faith of the Church of England, were lifted in the case of Cambridge in 1856, but other disabilities, such as proceeding to the M.A. or the holding of Fellowships and University offices, remained until 1871.

⁶University Hall was founded in 1849 by Nonconformists to extend the principle of religious freedom to the sphere of education and to encourage dissenting families to allow their young men to spend time in London gaining a university education. It was non-confessional, and although daily prayers and lectures in theology were offered, they were not obligatory.

⁷This was a paid post without specific teaching duties which allowed its holders to write, research, teach, coach, shoulder college office—or do nothing, as they wished. In the second half of the nineteenth century, a small college like Pembroke was only likely to vote into the fellowship men who were of outstanding academic merit and willing to teach, research, and hold a college office.

18 January 1873).⁸ Keynes agreed to stick with mathematics until the end of the summer term but was determined to switch to moral sciences from his second year. His Tutor, feeling this would be intellectually too undemanding, suggested that he combined moral sciences with law. Keynes however hit on another method of proving his intellectual capacity. Simultaneously with his Cambridge degree course, he would enter for other, broader degrees and prizes at London University.⁹

The reasons for Keynes's dislike for drilling in the set problems, set books, and set answers which candidates for top results in the Mathematical Tripos had to submit themselves are not spelt out but are easy to comprehend. An 18-year-old undergraduate was unlikely to challenge the received wisdom that the Cambridge Mathematical Tripos was the best possible intellectual training in logic and mental gymnastics available at the best university in the country. But Neville Keynes was twenty and had already a B.A. degree from London where he had been required to read classical literature and history, some natural science, philosophy, English literature and history, and so on, as well as mathematics. He also understood that he would have to achieve First Class Honours (known as a 'Wrangler'; c.f. Alfred Marshall's placing as Second Wrangler in 1865) in the Mathematical Tripos to be sure of a college Fellowship. The attraction of the moral sciences is also understandable. Coming as he did from a Congregational and Liberal home, school and Hall of Residence, Keynes had already been confronted with many of the social and moral problems of the age and was active in debate and in seeking solutions. During the Michaelmas (autumn) term at Pembroke, he spoke twice in college debates, once in favour of the government buying up the railways and on another occasion in favour of women's suffrage. The following year Keynes joined the University's Moral Sciences Club and became a regular contributor to their meetings.

His Congregational and Liberal turn of mind was reinforced at Cambridge by the warm welcome that he received in the home of William Bond at Brookside, just a short walk away from Pembroke. Bond was a successful Cambridge grocer and a central figure in Congregational life in Cambridge, and the Keynes and Bond families knew each other well.¹⁰ At the Bonds, Keynes met the bright young people of both the University and the town, including a future colleague, James Ward, who was reading moral sciences.

⁸ Neville Keynes's Diaries, which he kept from 1864 to 1917, are archived at Cambridge University Library, Reference GB 12 MSS.Add.7827-7869.

⁹ Phyllis Deane describes this, no doubt correctly, as a 'brave decision' (Deane 2001: 46).

¹⁰ Bond is described by Binfield (1968: 251) as 'the leading chapel personality, a forceful enlightened man to whom (Emmanuel) church owed much'.

In the Tripos finals of December 1874, Ward was placed first and crowned Senior Moralist.¹¹ Informally sitting the same papers were two students from a group of women living in Cambridge who were receiving tuition from some reform-minded Cambridge men. The first of these was Amy Bulley, a Congregationalist whom Keynes had met at the Bonds. The second, Mary Paley, he would come to know well. Placed in the Tripos degree examination between the First and Second Class, she would later marry Alfred Marshall. It was also at the Bonds that Keynes met the most important person in his life, his future wife Florence Ada Brown.¹²

The Moral Sciences Tripos covered Logic, Moral and Political Philosophy, Mental Philosophy, and Political Economy. Founded in 1848, the Tripos had attracted neither good teachers nor ambitious candidates. But the mood of questioning the absolute truths of both received religion and Euclid, as well as the irrelevance of studies in the language and literature of Ancient Greece and Rome to the problems of an industrial and increasingly democratic society, turned the attention of a number of important Cambridge thinkers to the moral sciences giving the subjects both prestige and sound academic credentials. Keynes had already proved himself in two of the Tripos subjects, and he began to enjoy the extensive reading the course required. Yet uncertainty about his career prospects as a graduate in moral sciences led him to take Parts I and II of the London BSc, gaining a First in geology and chemistry in October 1874. Even this was not thought to be sufficient and he prepared himself for the London M.A., aiming at the Gold Medal awarded to the candidate placed First. In December 1875, he passed his Cambridge Tripos as Senior Moralist and in the following June claimed the Gold Medal in the London M.A. All this was the result of a strict regime of hard reading, carefully noted in his Diary as the number of hours spent at work morning, afternoon, and evening, a habit he continued throughout his life.

Keynes did consistently well in all the subjects of the Tripos, but his best subject was logic. He was elected a non-teaching Fellow of Pembroke College, Cambridge, in 1876 and of University College London, in the same year. Alfred Marshall and the philosopher John Venn were particularly enthusiastic about Keynes's work and Marshall began plotting Keynes's career, a habit which was to recur. There was, however, no shortage of lecturers in economics when the student numbers were small and could be covered by Henry Sidgwick, Herbert Foxwell, and Marshall, with Mary Paley taking

¹¹ Another school and University College friend, Alfred West, had read moral sciences and had graduated as Senior (i.e. top) Moralist in 1869.

¹² Ward too married a Congregationalist student from Newnham who passed informally the Moral Sciences Tripos with a First in 1879.

over Marshall's lectures to women at Newnham College. Keynes agreed to give lectures in logic twice weekly for men reading for the Moral Sciences Tripos and a similar course of logic lectures for women. He also coached several Girton women students in Political Economy. A popular and conscientious teacher, Keynes's students did well in their Tripos examinations while Keynes was drawn even further into the circle of reform-minded men who were working to establish modern subjects and a system of intercollegiate lecturing at the University. He was also involved in the daring experiment of offering tertiary education to women of which Sidgwick and Marshall were leading lights in the early 1870s. By the time Keynes graduated as Senior Moralists in 1875, there were over 100 women in Cambridge preparing for the Cambridge Higher Local Examinations and Tripos examinations, following courses and taking examinations given and marked by senior male members of the University sympathetic to their ambitions.¹³

Although the subject does not appear to have interested Keynes, Marshall wanted him to specialise in Economic History in order to attract the attention of the Board of History to economic science.¹⁴ Marshall also encouraged him to enter for the University's newly established Cobden Essay Prize,¹⁵ writing an extended essay on 'The effects of machinery on wages', and Keynes even signed a contract to write an elementary book on economics. However, he did not win the Prize¹⁶ and a book project to which he had rashly agreed was dropped since Mary Paley had already agreed to write an elementary textbook in economics. The Marshalls left Cambridge for Bristol in 1877 and Keynes handed over his economics teaching to Foxwell and concentrated on logic.

Among the early students of Newnham Hall (later Newnham College) was Florence Ada Brown who came from the important Bedford Congregationalist family. She was the eldest daughter of Dr John Brown, minister of the Bunyan Meeting, often considered the centre of Congregationalism in England.

¹³The tertiary education of women at Cambridge was highly controversial and opposition to their presence in Cambridge grew as women began to demonstrate their academic prowess. The two women's colleges, Newnham (first residential students 1871) and Girton (founded 1869 and moved to Cambridge 1873), approached the issue differently and opinions varied on the 'correct' route to academic excellence amongst the women and their male supporters. For the story of this controversy, see McWilliams Tullberg (1995, 1998).

¹⁴These Boards or Special Boards organised the syllabus and teaching in Tripos subjects. The more familiar term today would be Faculty.

¹⁵An essay prize of £20 and a silver medal was offered every third year by the Cobden Club to recent graduates of Cambridge. The Club had been founded in the year after the death of Richard Cobden to promote his ideas of free trade, peace, and international co-operation.

¹⁶Marshall remarked on Keynes's essay that it had been weakened by the changes that he (Marshall) had advised him to make so as not to offend the external examiner Prof. T.E. Cliffe Leslie (Marshall to Foxwell, 3 July 1878, quoted in Whitaker 1996: volume 1, 101–102 and fn. 8).

She entered Newnham in 1878 on a scholarship where she studied for the Cambridge Higher Local Examinations with a view to school teaching. Being only 17, her father had particularly requested that her social engagements outside the College be limited. But, as she wrote herself, she returned home in 1880 secretly engaged to the young don, John Neville Keynes, whom she had met at the Bonds.

Neville Keynes's father died in 1878 leaving him with a considerable legacy from which he had a comfortable income of about £800 annually, assuming his investments were sound. But a man about to marry and prepare for a family needed an additional source of income if he wanted to live without the anxiety of managing an investment portfolio. Unlike his son, who was to love the excitement of stocks, shares, and futures, Neville Keynes's anxious disposition, together with an inherited work ethic and sense of duty, did not permit him to live without a paid occupation. Under existing Pembroke Statutes, he would have to relinquish his Fellowship on marriage. This celibacy requirement was a serious hindrance to many aspiring Cambridge academics and a further obstacle to the modernisation of teaching at the University.¹⁷

Alfred Marshall, when he and Mary Paley married, lost his Fellowship at St. John's College and reluctantly took the post as Principal and Professor of Political Economy at the newly established University College in Bristol.¹⁸ Faced with the same problem three years later,¹⁹ Keynes had to choose between remaining a Fellow and single, leaving Cambridge to find a teaching post in London²⁰ or in one of the university colleges which were springing up in provincial centres, getting himself appointed to a university or college teaching post in Cambridge, or turning to administration. The first two options involved leaving Cambridge and were unthinkable, so he chose a combination of the latter two. The unreformed statutes which prevented Keynes from holding a Fellowship as a married man were Pembroke College's loss but most certainly the University's gain. No longer a college fellow, Keynes was able as a skilled administrator to contribute enormously to the financial and educational well-being of the University in the following decades.

¹⁷ Generally speaking, only professors, heads of colleges, and holders of certain college and University offices could marry and many promising academics were lost to the University when they married and left Cambridge.

¹⁸ Mary Paley had to resign her post as Lecturer in Economics at Newnham. She too lectured in economics at Bristol. See McWilliams Tullberg (1995).

¹⁹ With his solid independent income, Neville Keynes was however in a much stronger position economically than was Marshall.

²⁰ Keynes briefly considered applying for the Chair in Economics at University College, London, that Jevons resigned in 1880. Keynes was aware that Foxwell was the more merited candidate and withdrew from the race.

In March 1881, Keynes was appointed Assistant Secretary to the Local Examinations Syndicate (LES).²¹ The post was no sinecure. Local Examinations and lectures were important projects in the armoury of Cambridge reformers. The academic reputation of Cambridge—and of individual colleges—rested in part on the quality of applicants choosing to study there.²² Secondary education in mid-nineteenth-century England was still an ad hoc affair and the teaching that boys received was not subject to any standardised assessment. Here the interests of the schools and Cambridge coincided: schools wanted to establish a reputation for good academic results and the University wanted to admit students of known merit and ability to its halls of higher education.²³ The organisation of Junior Local Examinations (under 16 years) and Senior Locals (under 18 years) for secondary-school children expanded rapidly, from 370 boys at seven centres in England in 1858 to almost 16,000 boys and girls in England and overseas, sitting examinations set and marked by Cambridge in 1898. Both the examination and the lecture schemes created jobs for a growing band of academics wishing to pursue careers in secondary and higher education or who needed to augment their incomes as fellows and lecturers at Cambridge colleges.²⁴

With his income and occupation settled, Keynes and his future wife bought a large semi-detached house which was to be built in Harvey Road, at that time on the outskirts of Cambridge although still within easy walking distance of the town centre, colleges, and administrative buildings. His marriage in 1882 and birth of his first son, Maynard, in June 1883 seem to have energised Neville Keynes and increased his interest in academic work. In 1884 he published *Studies and Exercises in Formal Logic*, based on the lessons and lectures which he had given his students since his own graduation. By all accounts, the book was an excellent pedagogical textbook in formal Aristotelian logic, valued for Keynes's ability to devise and formulate: '[S]imple, elegant and powerful methods of treatment, which involve no technical mathematics and depart to the least possible extent

²¹ The word 'Local' in the title meant just that. The University took its learning 'extra-mural'—outside its walls to people and places wanting higher education where they lived, were examined, and attested on what they had learnt.

²² In the matter of setting standards for schoolchildren, the University and colleges were for once in agreement. Similar reform movements were afoot in Oxford at this time.

²³ There was a certain conflict. The University was interested in assessing the ability of individual students by determining the syllabus and standards for subjects studied at secondary-school level. Many schools, however, preferred to have the quality of teaching to all students assessed, emphasising the role of a school in moulding the whole pupil and not just academic achievements.

²⁴ On the significance of the Syndicate's financial reserves for use by the wider University, see Raban (2008: 25–26).

from the traditional logic of “simple” propositions’ (Broad and Pigou 1950: 404–405), and for the large number of interesting and ingenious problems in formal logic provided for the student to work on. Contemporary critics noted that the book did not include any of the modern symbolic logic which was growing in importance but was nevertheless in general well received.²⁵ Encouraged by this success, Keynes began to think of developing his academic career further by writing a new book, focusing on inductive logic. However, as with his earlier plans for an elementary textbook on economics, news that someone else, in this case Venn, was planning such a book led him to drop his own idea.

In 1884, as part of the reforms shaping the modern University, the General Board of Studies appointed for the first time 23 Lecturers and three Readers across the disciplines as part of Cambridge’s plan to move the centres of teaching from the colleges to the University. In the light of his success and experience as teacher, examiner, and author and supported by Sidgwick, Neville Keynes was elected in April 1884 to hold the one Lectureship made available to the moral sciences, a position he held until 1911. Sidgwick wanted him to consider his Lectureship as being in Logic rather than Political Economy. Keynes wrote in his Diary that he was prepared, on behalf of the Moral Sciences Board ‘to do the work which is most needed’ (Diary: 29 January 1885).

A further opportunity arose for Keynes to pursue a strictly academic career path in January 1885. On the unexpected death of Henry Fawcett, Marshall was appointed to the Cambridge Chair as Professor of Political Economy in December 1884. Marshall returned to Cambridge from a post at Balliol College, Oxford, where he had been lecturing to Indian civil service candidates. Admiring the man and his work as well as that of his wife, Mary Paley, Keynes welcomed Marshall’s return to Cambridge and the boost that this would give to the modern study of economics. He was perhaps less prepared for the commotion that the new professor would cause in the moral sciences dovecotes. Marshall took the opportunity to set out his platform for the new Cambridge-style analytical economics which he would defend against both internal and external attack. His tactic was to strengthen the subject ‘institutionally’ by organising economists into a professional body with a respected house journal²⁶

²⁵ The book went through four editions and many reprints, the last being in 1906.

²⁶ The Royal Economic Society was founded as the British Economic Association in 1890 and volume 1 of the *Economic Journal* appeared in 1891.

and by placing his disciples in academic posts throughout the country's expanding tertiary education sector. Although London University and the provincial university colleges were gaining in reputation during this period, it was still the case that Oxford and Cambridge were in a separate league. It must therefore have been tempting to have a sound and reliable colleague in the Drummond Chair of Political Economy at Oxford smoothing the path from methodological controversy to consensus. Already in December 1884, Marshall set about persuading Neville Keynes to take over Marshall's Balliol lecture post which he felt would surely lead to Keynes's election to the Oxford economics Chair as soon as it became vacant.²⁷ He bombarded Keynes with letters and telegrams filled with flattery and appeals to his sense of duty and would not accept Keynes's refusal of the job.²⁸

Keynes, however, had a number of good personal reasons for not wanting to exchange Cambridge for Oxford. As far as his career was concerned, his prospects in Cambridge were tangible and more promising than the occupation of the Oxford Chair at some point in the future. He held the first Lectureship under the Moral Sciences Board, where he was well regarded as a logician as well as an economist and given the opportunity to lecture in both subjects. He had indeed published a valued textbook on classical deductive logic. His peers throughout the University had him in their sights as a skilled and hard-working future administrator. He felt himself that he had a strong claim to become the Secretary of the LES when the current Secretary retired.²⁹ He positively enjoyed administrative work and was good at it. Domestically, he had finally succeeded in solving one of his greatest worries—the welfare of his widowed mother—by settling her in a house in Cambridge close to, but not in, Harvey Road.³⁰ Neville Keynes was not a man who sought fresh fields and new experiences and preferred to minimise stress by dealing, with increasing competence and tact, with familiar people and familiar problems.

²⁷Details of this episode are given in Deane (2001: 123 *passim*).

²⁸This brought on Keynes's chest pains.

²⁹G.F. Browne did not pull his weight at the Syndicate. He retired in 1892 after being appointed Canon of St. Paul's and Keynes to his delight became 'master of the Syndicate Building' (Deane 2001: 196).

³⁰He was also under stress in the early months of 1885 because Florence had given birth prematurely to their second child, Margaret. Keynes asked himself what might have happened had he been in Oxford at the time.

2 *The Scope and Method of Political Economy*

Unexpectedly—his Diary gives no explanation—Keynes capitulated to Marshall's urgings and agreed to lecture in Oxford for the Trinity term of 1885 only, lecturing on the Methods of Political Economy 'and have an idea of writing a small book on the same subject' (Diary: 16 February 1885). He set about it with a will, realising perhaps that he already had the material for his lectures and that a book would enhance his merit list. Keynes travelled to Oxford one day each week, and his Saturday lectures there were popular and well attended. He could not, however, be persuaded to extend the arrangement nor to apply for the Oxford Chair when it became vacant in 1888.³¹

When Keynes initially told his colleague J.S. Nicholson about his book plans, Nicholson had exclaimed that 'anyone could write such a book as that. He says he is sick of disquisitions on Method and what we want is useful applications of the right method' (Diary: 2 June 1885).³² There was no doubt about the substance in Nicholson's criticism. Methodological controversy—*Methodenstreit*—still raged among economists as to which method, the deductive or the inductive, should be used to examine economic phenomena. German writers were regarded as the leading exponents of an exclusive use of inductive techniques applied to historical and sociological data while the classical economists such as Ricardo and Mill approached economics by deducing from first principles the laws that govern the behaviour of the abstract 'economic man'.

Classical theory in the 1860s and 1870s still centred on the doctrinaire adherence to *laissez-faire* despite its failure to solve ethical problems of an industrial economy or explain growth. Critics of the approach that deduced economic laws from 'first principles' held that humanity and justice called for more rather than less government intervention in economic matters. Marshall felt the disputes were inhibiting the development of modern economic analysis and the use to which the subject could be put in solving the pressing problems of the day. When a depression took root in the 1870s, the complacency of the 'classical' economists and their lack of answers to these urgent and serious economic difficulties were exposed. Meanwhile, the Historical School, an important movement in German-speaking universities, was becoming accepted in England. Proponents of the 'historical' school in

³¹ Although his lectures had been popular and he had enjoyed the exchanges with some of the better students, Keynes felt that the Balliol Fellowship had not really made him welcome.

³² J.S. Nicholson was second Moralist in 1876 and won the triennial Cobden Essay Prize in 1877 and again in 1880. He was Professor of Political Economy and Mercantile Law at Edinburgh University from 1880 to 1925. Nicholson read Keynes's chapters at the proof stage and gave constructive comments.

England could be found in Oxford University and in Cambridge in the person of the economic historian William Cunningham and later Herbert Foxwell. Why could not economists give an account of the processes of growth or decline? Historians challenged the idea that there were ‘economic laws’ independent of specific legal, institutional, and cultural contexts. Nor did they believe that introspection on the part of economists could produce meaningful economic theory deduced from ‘first principles’. This could only lead to a speculative and ideologically biased science.

Marshall and the new generation of economists were sensitive to charges that they were biased in their assumptions and quarrelled over their fundamental principles. They looked with envy at the progress being made in the natural sciences where methods and laws were accepted and well understood and they were tempted to use the logical rigour of mathematics to bolster the image of economic science. Just as history was a respected academic discipline in Germany, mathematics was, in English universities in general and Cambridge in particular, a high-prestige subject.³³ Jevons’s paper on the mathematical theory of Political Economy read to the British Association for the Advancement of Science (BAAS) in 1862 had demonstrated the analytical potential of mathematics, reworking the laws of supply and demand with the concept of diminishing marginal utility.

Popular feeling also ran high. Fierce criticisms of economic science were aired less than a month after Marshall’s appointment at the three-day Industrial Remuneration Conference held in London in January 1885. Topics for discussion were the changes in the distribution of the products of industry that had occurred in the past hundred years with reference to shares of capital and labour, the effects on the continuity of employment and wage rates, and, finally, policy proposals. Delegates were drawn from all sections of society, capital, labour, politics, and academia. Special mention was made of the attendance of the newly elected Professor of Political Economy at Cambridge, Alfred Marshall.³⁴ Many speakers were highly critical of the science of economics, regarding it as bogus, abstract, irrelevant, irresponsibly doctrinaire, and immoral. The subject matter was the economic activity of man in his specific social environment and as such should be treated as a branch of sociology or a moral science (Kadish 1982: 127–130).

These methodological disputes and the lectures he delivered at Oxford provided the impetus for Keynes’s only book on economic topics, *The Scope*

³³Until the mid-century reforms, students could not proceed to the Classical Tripos without first having succeeded in the Mathematical Tripos.

³⁴*The Times*, 13 January 1885: 4. The other delegates from Cambridge were Foxwell and Nicholson but not Neville Keynes.

and Method of Political Economy, which he started writing before the Oxford course was finished. He recorded in his Diary the rapid progress he was making and felt that the book should be ready for the publisher after the Christmas of 1885. This was not to be as he became bogged down with teaching, examining and Syndicate duties, and the publisher's urgent request for a new edition of his *Formal Logic* book that was almost sold out by 1886. Picking up the task again in the long vacation of 1887, his own work was interrupted by the proofs of Marshall's *Principles* that he had agreed to give a critical reading. Also, the death in January 1888 of the incumbent, Bonamy Price, of the Oxford Drummond Chair meant that Neville Keynes had to submit to Marshall's schemes to try to have him elected as the Professor of Political Economy there.³⁵ The work of writing *Scope and Method* dragged on but Keynes was finally ready to send the manuscript to Macmillan in August 1889. Marshall, Nicholson, his wife Florence, and his close logician friend W.E. Johnson agreed to give the proofs a critical reading. Keynes was most concerned by the comments that came from Marshall who urged him not to be so cut and dried about his definitions, to consign many controversial points to footnotes and appendices and worried him with reference to many German writers.³⁶

Keynes warned in the Preface to his book that questions of scope and method in Political Economy tend to be controversial and discussions heated. Ethical judgements and practical policies adopted to achieve specific ends could be the playgrounds of controversy. Keynes sought to examine the nature of these disputes, to identifying possible misunderstandings and teach his students and readers how to avoid such pitfalls.³⁷ His central pedagogical tactic was to identify three strands of thought on economic matters and, by careful definition, he attempted to isolate those parts of the subject that he felt could be discussed in neutral, scientific terms. By carefully separating the positive science from the normative science, the positive science would be immune from such attacks. Thus Political Economy is:

- (a) A positive science—an examination of regularities, 'a body of systemized knowledge' dealing with what is;
- (b) A normative science concerned with what ought to be; and

³⁵ The Drummond Chair in fact went to Thorold Rogers.

³⁶ Keynes confided in his Diary (21 April 1888) his embarrassing lack of fluency in German. Fortunately his wife was able to help him with translations.

³⁷ As Deane reminds us, 'Neville's perspective was that of a teacher rather than a "prophet"' (Deane 2001: 137). Note that John Maynard Keynes called Marshall a 'prophet' in his necrology (Keynes quoted in Pigou 1925: 65).

(c) An art—a method of achieving given ends (J.N. Keynes 1891: 34–35).

As regards economics as a positive science, Keynes wrote:

The abstract discussion of methods may appear to some to have mainly an academic interest, since it does not directly extend our knowledge of economic phenomena. Whilst, however, we ought to be upon our guard against allowing any such discussion to obscure the greater importance of actual economic investigations, the subject is one to which all students of economics must necessarily give some attention in the course of their reading, and its indirect bearing on the solution of practical economic questions is very far indeed from being without importance (*ibid.*: xvii).

As an example of the confusion of (a) economic science with (b) maxims of economic conduct Keynes cited the prevalent misunderstanding that arises when Political Economy is identified with a policy of *laissez-faire* (*ibid.*: 65). The assumption of conditions of *laissez-faire*, by which Keynes means an assumption of free competition and absence of government interference, is a convenient simplification when beginning an analysis of market forces and because it has been the general rule ‘in modern economic societies’ (*ibid.*). Other socio-economic models for societies exist, such as socialism or communism, and even under *laissez-faire* the total absence of government interference is rare. Most societies have, in some way, control over their currencies, foreign trade or trade in specific commodities, such as opium (*ibid.*: 66–67). The need to analyse and understand economic phenomena, such as cost of production or rent, remains even if society is organised on socialist principles or if our analysis relates to modern industrial times or historical periods. Similarly, *laissez-faire* may be a sound practical rule, but it is not sacred and there would necessarily be exceptions made to it. Adam Smith, for example, explicitly recognises exceptions to the policy of free trade and discusses cases where protection may be preferred.

In examining Political Economy as (c) an art, Keynes recognises two strands: the principles under which individuals and societies organise their economic affairs and the principles that guide the economic activity of the human race as a whole. There can be a conflict between these principles of which the economist should be aware, as, for example, the impact nationally and internationally of duties on trade. Ever the pedagogue, Keynes provides the student with many examples of the scope of Political Economy: What are we trying to achieve? Do we simply want to maximise wealth or are we

concerned with how wealth is distributed according to some concept of social justice? (*ibid.*: 80).

Keynes devoted a chapter to the use of symbolic and diagrammatic methods in *Political Economy*. Like Marshall, Keynes feared that the mathematical approach to the subject would alienate further those commentators who found economic science irrelevant. Yet unlike Marshall, he chose not to hide his mathematical tools in an appendix but informed the reader: 'The fact that political economy is essentially concerned with quantitative relations, and therefore involves mathematical notions, needs to be insisted upon, because to some economists the very idea of a mathematical treatment of economic problems is not only repugnant, but seems even absurd' (*ibid.*: 237).

Keynes assured his readers that numerical accuracy is not required before mathematical techniques—symbols and diagrams—can be used. The benefits are precision of thought and expression, a better understanding of the significance of continuity and multi-variability. Thus: 'One of the most important functions of mathematical analysis is to discover determinate relations between quantities whose numerical values are unassignable' (*ibid.*: 242). Unlike Marshall, Keynes is wholehearted in his approval of the use of mathematics and symbolic or diagrammatic methods in *Political Economy* and concludes: 'Mathematics may not up to the present time have proved an absolutely indispensable instrument of economic investigation and exposition; but it would be difficult to exaggerate the gain that has resulted from the application of mathematical ideas to the central problems of economic theory' (*ibid.*: 251).

In his chapter on 'Political Economy and Economic History', Keynes again drives home to readers that '[t]he propositions of economic history are accordingly statements of particular concrete facts; economic theory, on the other hand, is concerned with the establishment of general laws. Neither of the two can take the place of the other' (*ibid.*: 252). Keynes gives several examples of how economic theories can be illustrated, confirmed, or criticised using historical material. In questions relating to progress and growth, 'abstract reasoning is reduced to a minimum' and the use of historical generalisation becomes crucial (*ibid.*: 267). There is also a role for sociology to play in such investigations. Nevertheless, if problems of partiality are to be avoided, familiarity with economic theory is essential, especially when considering the events of the past hundred years.

The book, when finally published in 1891, was widely appreciated as solving or smoothing over points of methodological difference which had caused so much conflict in the 1870s and 1880s. This had been achieved

by using the same skills of common sense, patience, and tolerance which Keynes had used to such effect in his administrative work: careful definition of the problems involved, combined with a logician's clarity of exposition. The book received mostly favourable reviews, although the Oxford economist, L.R. Phelps, attacked it on the grounds of its lack of humour and, more seriously, because the 'book is careful, accurate and clear but it is not distinguished by ideas' (Phelps 1891: 570).³⁸ What students made of it is more difficult to appreciate. Familiarity with the general outlines of Political Economy was expected of the reader (see J.N. Keynes 1891: xviii) but much more was in fact required. In his Introductory chapter, Keynes expects of his readers to understand the rules and method of logic and to have done some background reading in the early economic masters' writing in both English and German. Students reading moral sciences could be expected to follow the logical terminology but the civil service probationers at both Oxford and Cambridge, students coming from the History Tripos, men aiming for the Pass degree or 'general interest' students may well have found it challenging.³⁹

Keynes's Introductory chapter is ambitious with reference made to the writings of over twenty English, German, or American writers.⁴⁰ The organisation of the book may have caused weaker students to despair, the text being broken up by lengthy footnotes and chapters extended by lengthy 'Notes' or appendices, again with extended footnotes. Some of this arrangement was the result of Marshall's criticisms. He felt that Keynes's views were presented too unambiguously and urged him to put controversial material in footnotes and difficulties in appendices. The book reflected what Marshall called Keynes's 'tidy mind' and he complained that Keynes made 'all [his] contrasts too sharply for me' (Deane 2001: 140).

³⁸While giving his course in Oxford in 1885, Keynes had dined with Phelps in Oriel College. Keynes wrote in his Diary that 'Phelps is an old Charterhouse boy. He has the teaching of the Junior Indian Civil Service men; but does not profess to know a great deal of Political Economy. He is immensely interested in the Charity Organisation Society. He is very frank and friendly' (Diary: 11 May 1885). Phelps, who was later known as an authority on social administration, was at the time of his review a member of the Board of the Oxford *Economic Review*, a journal that was in competition with the *Economic Journal* and reflected the views of the Historical School of Political Economy.

³⁹For example, while it could be expected that the educated reader was familiar with the pitfalls of a *post hoc ergo propter hoc* fallacy, they may have found *ignoratio elenchi* fallacies more difficult to follow.

⁴⁰The following luminaries appear in the text and some in footnotes of the Introductory chapter: Smith, Malthus, Ricardo, Mill, Senior, Bagehot, Cairnes, Sidgwick, Jones, Cliffe Leslie, Walker, Roscher, Hildebrand, Menger, von Thünen, Knies, Sax, Wagner, Schonberg, Schmoller, Ingram, von Scheel, Cohn, Seligman, and Dunbar.

3 Conclusion

With the successful publication of *Scope and Method* and the announcement in 1891 that he was to be awarded the coveted Doctor of Science degree by Cambridge on the basis of his two books, Neville Keynes's enthusiasm for publication waned as did his willingness to do further work in economics. Although he had misgivings about the project, he wrote as promised 17 articles 'suitable for the educated adult' on methodological matters for Palgrave's *Dictionary of Economics*. He also wrote a number of reviews for the *Economic Journal* and, after the death of Henry Sidgwick, saw the revised edition of Sidgwick's *Principles of Political Economy* through to publication. He resisted Marshall's efforts to make him editor of the new periodical the *Economic Journal*—the job was finally taken by Edgeworth—though he did agree to sit on the Council of the new British Economic Association, the professional body that became the Royal Economic Society.

Keynes was flattered to receive an invitation from the University of Chicago offering him a Professorship in Political Economy in 1895 but seems not to have given it much more thought than he had the vacant Drummond Professorship at Oxford a few years earlier (notwithstanding Marshall's manoeuvrings with respect to the Drummond Chair). He was flattered again the same year when the vice-presidency of Section F of the BAAS was suggested but again he refused since accepting it entailed taking over the presidency the following year.⁴¹ In 1892, he realised his ambition of succeeding G.F. Browne as head of the LES, remaining in the post until 1910 (see above, fn. 29). As Secretary, he was closely involved in the University's reactions to the Royal Commission on Secondary Education (Bryce Commission) from 1896. He retained his post as University Lecturer in Moral Science lecturing in both logic and economics until 1911. This involved him in a considerable amount of work as an examiner, both setting the papers and marking them. It also led to his appointment as Chairman of the Special Board for Moral Sciences, 1906–1912, as well as Chairman of the Special Board for Economics and Politics, 1908–1920, after Marshall's retirement in 1908.

Marshall's retirement also brought to an end many years of struggling on Keynes's part to remain patient with the man whom he had admired since his student days and so warmly welcomed to the Cambridge Chair in Political Economy in 1885. Keynes's diaries for these two decades contain many references to time-consuming skirmishes at meetings of the Moral Sciences Board

⁴¹ Diary: 5 January and 13 February 1895.

as Marshall tried to improve the status of economics within the existing Tripos and then sought to found a stand-alone Economics Tripos. Keynes was Marshall's faithful lieutenant in this latter project but was relieved when the campaign was over. Keynes's relationship with Marshall was further strained by the Professor's stiff-necked opposition to any integration at Cambridge of women students with their male counterparts.⁴² This was not the view taken at 6 Harvey Road. Life in this family home also played its part in steering Keynes away from the demands and stresses of life as a cutting-edge, academic economist. Keynes clearly found great pleasure in the activities of his wife, his three children, and their large circle of relatives and friends whom they entertained regularly. His Diaries reveal a father encouraging and participating in his children's hobbies, involved in the minutiae of their education, supremely happy with his wife, with his golf, tennis, and chess, as well as philately and butterflies, novels, and the theatre. It is significant that Keynes when writing the Preface to the first and subsequent editions of *Scope and Method* adds, after his name, not the name of his University but his home address: 6 Harvey Road, Cambridge.

In December 1942, Neville Keynes was congratulated by his College newspaper, the *Pembroke Gazette*, on his 90th birthday and his and Florence's 60th wedding anniversary.⁴³ The College journalist mentioned the role that Neville Keynes played in College life, and then added: 'By most of us he is best remembered as Registrar of the University. He held that office from 1910 to 1926 and throughout that time, his knowledge and his great kindness were always at the University's disposal'. The following year Neville Keynes and his wife were interviewed at 6 Harvey Road, possibly on the occasion of Maynard Keynes's maiden speech in the House of Lords.⁴⁴ Aware of Neville Keynes's early interest in economics, the journalist asked his opinion of the 'world economic plan proposed by Lord Keynes, the economist'. 'But Dr Keynes shook his head ... "That is a complicated business, too complicated. All that is beyond me now"'. Much loved and respected, Neville Keynes died in 1949 aged 97 in his beloved Cambridge after 232 terms of unbroken residence at the University and three years after the early death of his world-famous son.

⁴²For more on these stories, see Deane (2001: Chapter VIII) and McWilliams Tullberg (1990, 1995, 1998).

⁴³*Pembroke Gazette* (1942: no. 16).

⁴⁴Maynard Keynes had been made a peer, Baron Keynes of Tilton, in June 1942 and gave his maiden speech in May the following year. This reference is from an undated and untitled newspaper deposited in Pembroke College.

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19

John Harold Clapham (1873–1946)

Martin Daunton

1 Introduction

In 1928, the University of Cambridge established a Professorship of Economic History—and it was clear that there was only one credible holder of the new post: John Clapham, who had been teaching the subject at Cambridge for the previous 20 years, and had recently published the first volume of *An Economic History of Modern Britain*. The new Professor was formally assigned to the Faculty of History, but was also an ex officio member of the Board of the Faculty of Economics, a dual identity that well suited Clapham's intellectual formation as a protégé of Lord Acton, the Regius Professor of Modern History, and of Alfred Marshall.

Clapham came from a business background in Lancashire which clearly informed his writings in economic history, with their close attention to the details of industrial organisation and social conditions. His father left the family farm in Yorkshire and served an apprenticeship with a jeweller in Bradford, before moving to Manchester as a salesman, where he rose to be the head of a firm of silversmiths and jewellers. His mother was the daughter of a smallware manufacturer in Manchester (Trevelyan 1949: 22). The family lived in Broughton, a suburb of Salford, before moving to the neighbouring

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town of Prestwich. Broughton was markedly different from the descriptions of the town's deprivations in Engels's *The Condition of the Working Class in England* or Robert Roberts's memories of 'the classic slum' (Roberts 1971). It was a more salubrious area, developed by the local landowner as a suburb with large villas and terraces for professional and merchant families, including Greeks from the Ottoman Empire who built a large Orthodox church.¹ The Claphams were more prosaically Wesleyan Methodists, and Michael Postan—the second holder of the Professorship of Economic History—commented that Clapham 'absorbed to the full the sober Nonconformist spirit of his paternal household' and inherited from his father 'the intellectual and moral virtues of the Victorian middle class at their best—a head which was shrewd and cool, an outlook which was wholly unsentimental and a rule of life disciplined to the point of being hard' (Postan 1946: 56). It was certainly a background which provided insight into the development of the industrial economy that was to be Clapham's main scholarly achievement: close to Manchester and the cotton trade, and to the cosmopolitan merchants and professional classes who created a commercial, industrial society in the north of England.

In 1887, Clapham was sent to the Leys School in Cambridge, a Wesleyan establishment set up in 1875 to provide a public school education for the sons of affluent Methodists. Clapham was a successful sportsman, coming second in the public schools' quarter mile and playing cricket. A fellow historian, G.N. Clark, remembered him as 'tall, strongly built and energetic' (Clark 1946: 340)—and he remained an active mountaineer, climbing in the Alps or Lake District every summer for 40 years and being elected vice-president of the Alpine Club. He took part in debating and in the literary society at school, where he was also a star pupil in history. His father's leisure activity was reading the works of Lecky, Buckle, and Freeman, and Clapham was inspired by the history master at the Leys, G.E. Green, who had graduated from Cambridge with a First in history in 1885. Clapham was excused ordinary routine lessons and placed under Green's guidance. In 1892, he won a history exhibition to King's College, Cambridge, graduating with a First in 1895.²

During his history degree, Clapham was taught economic history by William Cunningham, to whom he paid tribute in his Inaugural Lecture in 1929 as one of the two outstanding economic historians of his time (Clapham 1929: 7). Cunningham was appointed an examiner in the new History Tripos

¹ On Broughton, see Farrer and Brownbill (1911: 217–222) and Trevelyan (1949: 22).

² See the recollections of A.C. Pigou (1949: 17–19), with whom Clapham often climbed, and his school contemporary H.C. Gutteridge (1949: 12–13); see also Postan (1946: 56) and Clark (1946: 340).

in 1878, with responsibility for lecturing in economic history, a duty that led to the publication of his textbook on *The Growth of English Industry and Commerce* in 1882. He was appointed a University Lecturer in the subject in 1884. Clapham also attended Marshall's lectures and classes, who recorded on his registration card that he 'shows force', and remarked that he was the best 'Historical man' he had taught (see Groenewegen 1995: 327, and Marshall to Browning, 21 November 1895, quoted in Whitaker 1996a: 138). Clapham stayed on to read economics in Part II of the Moral Sciences Tripos; a separate Economics Tripos did not start until 1903. As we shall see, Marshall and Cunningham disagreed about the methodology of economics, over the role of induction and deduction, of theory and fact, with Clapham taking a more consensual and emollient line.

At this stage, the route to an academic career was through a dissertation to secure election to a college Fellowship. In 1896, Clapham secured the Lightfoot Scholarship in Ecclesiastical History, and started work on his Fellowship dissertation on the causes of the war of 1792. The dissertation won the Prince Consort Prize awarded for the best dissertation involving original historical research by members of the University who were not more than four years from admission to their first Cambridge degree. It also secured him a Fellowship of King's College. The dissertation dealt with the first war between revolutionary France and Europe, and was a study in conventional diplomatic and high political history. He thanked Acton in the Preface to the book—and it was Acton who suggested that his next project deal with the French Revolution. His study of *The Abbé Sieyès: An Essay in the Politics of the French Revolution* was published in 1912. However, neither study explained political events and ideas in terms of economic conditions; they provided an essentially political narrative. But Clapham was also encouraged, by Marshall, to redirect his energies to economic history which marked a significant change in his intellectual direction.³

Clapham was teaching economic history in the History Tripos, as well as assisting Marshall in his teaching responsibilities for the general course in economics—a considerable burden caused by the refusal of Foxwell to assist which left Marshall 'the whole of the drudgery side of economic teaching' (Marshall to Foxwell, 14 May 1901, quoted in Whitaker 1996a: 319; italics in original). Marshall turned to Clapham for support. In November 1897, Marshall wrote to Acton that Clapham was 'looking over the papers done by my "general" class; and I see much of him. I think he is a splendid

³The dissertation was expanded and revised as *The Causes of the War of 1792* (Clapham 1899); see also Clark (1946: 341, 346).

fellow'. Marshall had clear plans for Clapham to fill a serious lacuna in economic history:

I feel that the absence of any tolerable account of the economic development of England during the last century and a half is a disgrace to the land, and a grievous hindrance to the right understanding of the economic problems of our time...but till recently the man for the work has not yet appeared. But now I think the man is in sight. Clapham has more analytic faculty than any thorough historian whom I have ever taught: his future work is I think still uncertain: a little force would I think turn him this way or that. If you could turn him towards XVIII or XIX century economic history, economists would ever be grateful to you (Marshall to Acton, 13 November 1897, quoted in Whitaker 1996a: 206).

The 'little force' set the direction for Clapham's future career. He acknowledged in his Inaugural Lecture that Marshall pointed him in the direction of economic history: 'I hesitated then, for Acton's power was on me, as I hope it still is. But Marshall has prevailed' (Clapham 1929: 8–9).

By 1901, Marshall was concerned that Clapham's assistance—invaluable though it was—had created a problem for 'it raised a wall of division between me and my class: I did not get inside their minds', and in any case, Clapham's 'turn of mind has always been dominantly historical'. Marshall hoped that he had the solution in A.C. Pigou (Marshall to Foxwell, 14 May 1901, quoted in Whitaker 1996a: 320).⁴ This concern over the future provision of teaching was linked to Marshall's desire to create a new Economics Tripos—a proposal that was dividing historians and economists. In 1902, Clapham took the Chair of Economics at Yorkshire College, soon to be the University of Leeds. According to G.M. Trevelyan, the appointment was possibly the result of Marshall's influence, and was a solution to the problems caused by the lack of career opportunities in history at Cambridge and doubtless a means of creating an opening for Pigou (Trevelyan 1949: 23).

At Leeds, Clapham turned to a study of the local worsted and woollen industries, somewhat in the style of Marshall's concern for industrial districts. The book appeared in 1907, and rested, in the words of Clark, on a 'vivid imagination of the physical facts' and a careful use of statistics. Clark pointed out that the book was not historical but could only have been written by a

⁴ See also Marshall to Neville Keynes, 4 March 1900, in which he commented that Pigou was the 'ideal man' but not yet ready. Foxwell was not impressed: he [Pigou] was 'least qualified to deal with a general class as he is such a prig' (quoted in Koot 1987: 132).

historian with an orderly mind (Clark 1946: 344). In 1905 Clapham married Mary Margaret Green, the daughter of a surgeon from Ross in Herefordshire, who was working in Leeds for the Yorkshire Ladies' Council of Education; they had one son and three daughters. Clapham remained in touch with Marshall during his time in Leeds. He signed the paper in support of the new Economics Tripos, despite his criticism of its excessively theoretical nature. He also offered support to the free trade manifesto in 1903 (Groenewegen 2012: 70–71).

The stay in Leeds was professionally and personally successful, but Clapham was relieved to return to Cambridge in 1908 as a Fellow of King's, succeeding Browning as Assistant Tutor in history and, from 1913, as Tutor. Clapham took responsibility for the lectures in economic history which he delivered from 1908 to 1935, covering the entire period from prehistory to the present (Trevelyan 1949: 24). According to Clark, these lectures were highly regarded for their 'complete mastery in delivery and presentation'. They formed the basis of Clapham's *Concise Economic History of Britain from the Earliest Times to 1750* (Clark 1959: 20).⁵ Marshall welcomed Clapham's return, and thought that he was well suited to the task of lecturing in economic history (Groenewegen 2012: 70). When Marshall retired in 1908, he supported Pigou's appointment to the Professorship of Economics, but he also argued that if a second chair were created, he would back Clapham as intellectually superior to Foxwell. Marshall expressed his 'eager admiration' for Clapham, and 'would always go to him as counsellor of the first weight in any difficult matter of judgement. I think his achieved work is of a very high order, full of individuality and strength' (Marshall to Neville Keynes, 13 December 1908, quoted in Whitaker 1996b: 214–215). Marshall's support for Clapham is indicative of his willingness to accept a more historical approach to economics than Cunningham would admit.

In 1916, Clapham joined the Board of Trade, and served as a member of the Cabinet Committee on Priorities, for which he was appointed CBE (Commander of the Order of the British Empire) in 1918. This experience of economic policy led him to resume his interests in economic history, and to write his first major book on the subject: *The Economic Development of France and Germany* (Clapham 1921). He also embarked on the project which Marshall had proposed in 1897: an economic history of modern Britain from

⁵The book was published posthumously: its origin is explained by John Saltmarsh in the Preface to Clapham (1949). Clapham did not complete the second volume, which was undertaken by W.H.B. Court and appeared in 1954.

1820 to 1914. This study appeared in three volumes, starting with *The Early Railway Age, 1820–1850* in 1926 which was dedicated to the memory of Marshall, ‘who told me, 25 years ago, that it was my business to write something of the sort’, and to Cunningham who taught him economic history. As Clapham said in his Inaugural Lecture, the foundation of the Professorship of Economic History was a memorial to both men (Clapham 1929: 9). The second volume, *Free Trade and Steel, 1850–1886*, appeared in 1932; and the final volume, *Machines and National Rivalries (1887–1914) with an Epilogue (1914–1929)*, in 1938. The first volume was welcomed by *The Times* for offering ‘a picture of normal society in a past age in the same fullness of detail as we can picture our own age ... It is the beginning of what we have never had before, a history of the English *people*’ (quoted on the jacket of *Machines and National Rivalries*; italics in original).

In 1928, Clapham was appointed to the newly created Professorship of Economic History at Cambridge, which he held until his retirement in 1938. He became a Fellow of the British Academy in 1928, and served as President from 1940 until his death in 1946. He was vice-provost of King’s College between 1933 and 1943, and held a number of public and charitable appointments. He chaired the Cambridge Employment Committee and Refugee Committee, and served on the panel to consider conscientious objectors. He was a leading member of the Society for the Protection of Science and Learning which assisted refugee scholars from Nazi Europe (Trevelyan 1949: 25). At the time of his death, he was chair of the Committee on the Provision of Social and Economic Research (Clapham Committee); he saw its report in draft, and its recommendations were adopted with the creation of an Inter-Departmental Committee on Social and Economic Research in October 1946 ‘to survey and advise upon research work in government departments’, suggesting ways in which material could be used for research in government and beyond. During the war, his academic work continued. The absence of colleagues on war service meant that Clapham taught throughout the war, and the death of Eileen Power in 1940 meant that he was solely responsible for editing the first volume of the *Cambridge Economic History of Europe* which appeared in 1941; he was well advanced with editing a second volume at the time of his death. In 1944, his two-volume history of the Bank of England up to 1914 appeared, marking the two hundred and fiftieth anniversary of its charter. It was the only book for which he carried out archival research. Clark pointed out that Clapham ‘always disparaged his own work in comparison with work based on unprinted records’, but that he showed an ability to use such sources to write the history of an institution for which he was well suited

to be the historian—‘solid, undemonstrative, unique’. Postan thought it the ‘perhaps the best of his books’ (Clark 1946: 349; Postan 1946: 58).⁶

Arguably, Clapham’s most significant achievement was in providing the first detailed overview of British economic history since the Industrial Revolution—but it was not entirely what he wanted. As he explained to Clark in 1930, ‘I decided to shift into economic history for 20 years—if I were allowed—and to begin at 50 to build up some sort of history on its economic frame’. He feared that by the time he completed his account of the economic development of modern Britain, he would not have the ‘years, learning and vigour’ for the wider task (Clapham to Clark, 9 February 1930, quoted in Clark 1946: 343). Some of his sense of disappointment is apparent in the Introduction to the *Concise Economic History*: ‘Of all varieties of history the economic is the most fundamental. Not the most important: foundations exist to carry better things’ (Clapham 1949: xvii). This comment must not be taken to suggest that Clapham was a historical materialist. He assumed that economic and political history could be written as separate narratives without any causal superiority; he never made any general reflections on the connection between economics and politics.

Clapham remained a serious and somewhat forbidding product of his Wesleyan upbringing. He remained a committed Christian throughout his life, retaining an interest in biblical scholarship, and gradually moving from Methodism to Anglicanism (Clark 1946: 339). A fellow undergraduate saw him as somewhat apart from the world of King’s:

He was not the man to throw up the old lightheartedly and plunge without restraint into a more highly coloured world. I imagined him fighting a strong defensive fight for each old position and, when it was finally abandoned, taking resolute care that all that was good in the old one was retained. Such a progress would be quiet, a little dour, and very self-contained (Giblin 1949: 15).

He had a reputation for being kind and just, but also for being somewhat stiff which prevented him from forming easy relations. His general demeanour was ‘professorial’, ‘rather formal and perhaps a little important. He did not cultivate airs and graces; he was never effusive in praise or complaints, and he sometimes expressed disapproval or disagreement with uncompromising bluntness’ (Clark 1946: 351). Postan did see a mellowing in older age when Clapham became more tolerant, ‘kindlier and freer’. His forbidding and dour

⁶ For more on the Inter-Departmental Committee on Social and Economic Research, see The National Archive: RG25. Available at: <http://discovery.nationalarchives.gov.uk/details/r/C13350>.

manner was linked to a sense of moral responsibility and engagement in charitable activities which was expressed in his work for refugees from Germany, and support for gradual reforms to create a fairer society. Postan felt that ‘his attitude to most things was essentially rationalist, positivist and matter-of-fact. In politics he was essentially a Liberal, but his politics had nothing of the latter-day radicalism in it’. Indeed, Clark saw him as attached to ‘old standards of morality and patriotism that might be called conservative and conventional’, disliking speculation and distancing himself from ‘the more surprising intellectual adventures’ of other Fellows of King’s (Postan 1946: 56, 58; Clark 1946: 345).

In the next three sections, I will explore some of the implications of Clapham’s intellectual and personal formation by addressing three themes. First, what were his attitudes to economics, and above all his relationship with Marshall and the emergence of a more theoretical approach to economics? Second, what was his approach to economic history? Third, what were his attitudes to politics and policy, shaped by his economics, his study of economic history, and his personal values?

2 Clapham and Economics

What was Clapham’s attitude to economics and above all to the work of Marshall which was strongly criticised by Cambridge historical economists? Clapham commented in his Inaugural Lecture that Marshall ‘was a greater economic historian than he let the world know. He had discarded as irrelevant to his main purposes more historical knowledge than many men acquire’ (Clapham 1929: 8). This assessment was more generous than that of Cunningham, who severely castigated Marshall’s outline of economic history in the *Principles*. One view is that Clapham was a ‘minor Marshallian’ who was willing to be critical when necessary and to provide assistance in modest ways. For example, Marshall thanked Clapham for checking his historical reflections in *Industry and Trade* (Groenewegen 2012: 79–80; Marshall 1919: Preface). Alon Kadish is right to say that Clapham cannot be called a disciple of Marshall, but he did provide economists with a factual account of recent economic history in a less partisan manner than Cunningham (Kadish 1994: 225).

How, then, did Clapham fit into the debates that were dividing Cambridge at the start of his academic career, between the historical economists—above all Foxwell and Cunningham—and the more theoretical approach of Marshall and Pigou? Clapham shared some of the concerns of the historical economists

for historical specificity, without going so far as them in their strident hostility, and without entirely accepting their condemnation of individualism and laissez-faire economics. He shared many of Marshall's assumptions about a liberal, free trade economy that could deliver benefits to most people, while retaining a sceptical attitude towards the economists' assumptions of *homo economicus* and their generalising theories. Whereas Foxwell and Cunningham mounted a vigorous challenge to inductive theory which they feared would destroy historical economics, Clapham was not so partisan. He did query theoretical economics in 1922, but had accepted by 1929 that historical economics was no longer viable. He simply turned away from any formal engagement with economics, retreating into a form of economic history that did not challenge the prevalent assumptions of neoclassical economics. The ideological war between historical economics and theory gave way to mutual indifference between economic history and neoclassical economics.

Although Marshall is often seen as the creator of theoretical neoclassical economics, he was sympathetic to the German Historical School (Hodgson 2005). He studied in Germany with members of the School and continued to value their work. He was aware of the need to consider historical specificity. In his Inaugural Lecture of 1885, in his *Principles* of 1890, and in his essay on 'The Older Generation of Economists and the New' of 1897, Marshall made the point that economics differed from the constant and unchanging laws of physics, and could not be applied without modification as society changed: 'Though economic analysis and general reasoning are of wide application... every change in social conditions is likely to require a new development of economic doctrine' (Marshall 1949: 30–31). Marshall quoted Gustav Schmoller with approval: 'Induction and deduction are both needed for scientific thought as the left and the right foot are both needed for walking' (ibid.: 24, and see also Marshall 1890: 72–77). Marshall argued that

[e]ach study supplements the other: there is no rivalry or opposition between them; every genuine student of economics sometimes uses the inductive method and sometimes the analytical, and nearly always both of them together. There is a difference in proportion between different students; as one may eat more solid food and another drink more fluid: but everyone must both eat and drink under pain of starving or dying of thirst (Marshall 1897: 133).

Deduction should rely on short chains of reasoning so that it did not escape from reality; equally, induction should not rely on facts to speak for themselves, without appreciating the implicit theories and assumptions that led to their selection:

When...it is said that a certain event in history teaches us this or that, an element of deductive reasoning is introduced, which is more likely to be fallacious the more persistently it is ignored. For the argument selects a few out of the group of conditions, which were present, when the event happened, and tacitly, if not unconsciously, assumes that the rest are irrelevant. The assumption may be justifiable: but it often turns out to be otherwise (Marshall 1925: 166).

Although Marshall spent his own intellectual energies developing ideas that were of wide application, he saw the need to understand through induction how society (and economic ideas) changed over time. His comments on the teaching of economics at the London School of Economics (LSE) brought out his approach. He told the Director—W.A.S. Hewins, himself a historical economist—that he ‘holds Economics to be an organic whole, and has as little respect for pure theory...as for that crude collection and interpretation of facts without the aid of high analysis which sometimes claims to be part of economic history’ (Marshall to Hewins, 12 October 1899, quoted in Whitaker 1996a: 256). Pure theory was no more than ‘elegant toying’ and he urged an approach to economics as ‘the application of powerful analytical methods to unravelling the actions of economic and social causes, to assigning each its part, to tracing mutual interactions and modifications; and above all to laying bare the hidden *causas causantes*’ (Marshall to Hewins, 29 May 1900, quoted in *ibid.*: 280).

Despite these nods to the historical method, Marshall’s instinct was for theoretical, neoclassical models which combined Ricardo with later economists. The historical economists in Cambridge were fiercely critical. We have seen that Foxwell was a constant irritant to Marshall over teaching, in part because of their intellectual disagreements (see the chapter on Foxwell in this volume). Cunningham similarly rejected the cosmopolitan liberalism of Marshall. Foxwell summed Cunningham up as ‘a great National Economist, the modern representative of an old English tradition, unfortunately interrupted by the atomism and premature cosmopolitanism of the *laissez faire* age’ (Foxwell quoted in Koot 1987: 135). He looked to the restoration of an organic society based on the state. Like Foxwell, Cunningham rejected Ricardo’s abstractions and the reduction of human motives to *homo economicus*. Free trade was the epitome of everything he detested: individualism, cosmopolitanism, materialism, and *laissez-faire*. In *Growth of English Industry and Commerce*, especially the later editions, Cunningham portrayed the Tudor period as a time when church and state came together to create a ‘national consciousness’. He was guarded in his

interpretation of mercantilism which led to war and selfish interests, but he saw it as a means of securing national power. He was critical of the replacement of Tudor regulation of wages and prices by competitive individualism and laissez-faire, and he saw that free trade was just another selfish policy for industrialists to sell British goods to foreign markets. He welcomed the return in his own day of a policy of ‘national husbandry’ (Koot 1987: 136–142, 150–153).⁷

Cunningham was combative, once declaring in a sermon that the joys of Heaven would not be complete without the pleasure of conflict (Maloney 1976: 441). Not surprisingly, he created discord over the teaching of economics, for he did not accept the balance between deduction and induction proposed by Marshall. In his opinion, facts had primacy, and economics was an empirical science. He challenged Marshall with *Political Economy Treated as an Empirical Science: A Syllabus of Lectures* in 1887 and again in ‘The Perversion of Economic History’ in 1892. In his view, the role of theory was to produce taxonomies:

Instead of aspiring to be a sort of pure physics of society which assuming a single force—the individual desire for wealth—states the laws of the operation of this force in the supply and demand of different articles of value, political economy might for the present be content to *observe* and *classify* and *describe* and *name* as other sciences have been ... No real advance can come from the statement of laws of phenomena which only hold good when a considerable number of cases are excluded as abnormal; if political economy is to rank with other empirical sciences one must try to classify the widely varied phenomena of industrial life...as an empirical science in its classificatory age (Cunningham quoted in Hodgson 2005: 339; italics in original).

Cunningham objected to the assumption of neoclassical economics that ‘economic principles have mathematical character of being true for all times and places alike’. He rejected the idea

[t]hat the same motives have been at work in all ages, and have produced similar results, and that, therefore, it is possible to formulate economic laws which describe the action of economic causes at all times and in all places ... If this assumption were sound, it would seem to follow that these economic laws could

⁷Cunningham’s views on the economic history of England were developed in the later editions of *The Growth of English Industry and Commerce*. The second edition covered *The Growth of English Industry and Commerce in the Early and Medieval Ages* (1890), and there were two volumes on the modern period, *The Mercantile System* (1890) and *Laissez Faire* (1892).

be most conveniently studied in the present, under our own eyes, as it were; but that when once recognised and stated, they serve to explain the past ... If we already understand the *principles* which explain industrial and commercial affairs, all that we need do is to look to history for illustrations of what we already comprehend clearly (Cunningham 1892: 493; italics in original).

In Cunningham's view, 'economic doctrine about the actual world we live in is all built up as a branch of empirical knowledge; it has no universality' (ibid. and Cunningham quoted in Hodgson 2005: 340). There was 'no royal road by which we may get to comprehend the evolution of social structure and of economic conceptions', yet economists with no experience in weighing historical evidence, and on the basis of a few badly chosen books, 'will decide the most difficult problems off hand, or sketch you the history of the world with easy confidence'—a jibe directed against Marshall's outline of economic history in the *Principles* (Cunningham 1892: 491–492). One example of error was Marshall's use of Ricardo's theory of rent to explain the determination of rent in all ages, regardless of actual evidence.⁸ Cunningham was also critical of scholars such as Thorold Rogers who collected facts, but then misinterpreted them by too easily assuming the pursuit of self-interest by economic man, and the isolation of economic phenomena from other considerations (Cunningham 1892: 498). Economics had to involve the wider context and move beyond individualistic, atomistic, rational 'economic man' to appreciate economic action through membership of collectivities, and above all the nation. Cunningham rejected what he called 'Cosmopolitan Economic Science' and argued instead for a political economy that proceeded through understanding 'the particular needs and ambitions of a particular polity, and can only indicate the means to procure wealth-as-conceived and wealth-as-desired by that nation, not wealth in general' (Cunningham quoted in Green 2002: 58, and see the discussion of historical economics contained therein, pages 56–64 and Green 1995: 162–183).

In his response, Marshall accepted that history was necessary for economic theory and argued that the *Principles* was 'indeed occupied mainly in showing how similar causes acting on people under dissimilar conditions produce more or less divergent results'. But he also claimed that theory was needed to understand causation in history, without which economic history would be reduced to 'a mere series of facts'. People in the past might be influenced by custom and have different habits of mind, with different means to attain their

⁸ See Cunningham (ibid.: 494) on Ricardo and more general attacks on pages 495–498. In 1889 Cunningham criticised Marshall's use of Ricardo instead of accepting descriptions of medieval or Indian economic forms as they actually were (see Cunningham quoted in Maloney 1976: 441).

ends—yet there might still be opportunity to pursue private gain. The point was to study the limits of custom and the opportunities for change, and to realise that the theory of rent imposed an upper limit on what a landowner could demand from the tenant. Negotiations between landlord and tenant over the amount of rent ‘offer unconscious illustrations of the law of rent; just as the expert cricketer fielding at slip, or the sailor bringing up his craft neatly to her buoy, does of the laws of mechanics’. The use of such concepts might assist the economic historian in knowing where to look in the past (Marshall to Foxwell, 27 March 1899, quoted in Whitaker 1996a: 251, and Marshall 1892: 508–511).⁹

Marshall’s conflict with Cunningham was less about hostility to the Historical School than to concerns about his extreme and naive empiricism (Hodgson 2005: 340–342). Indeed, Cunningham’s position was seen as exaggerated even by William Ashley, another leading historical economist, who realised that Marshall’s *Principles* ‘brings a message of conciliation to divergent schools, and it makes it possible for “deductive” and “historical,” “scientific” and “ethical” economists to work together in harmony’ (Ashley 1891: 489). The conflict was also about the nature of human personality: Marshall and Rogers were liberal rationalists who saw the slow working of rational calculation overcoming prejudice and custom; Cunningham was a romantic conservative who disliked individualism (Maloney 1976: 447). These points led to divergences over economy policy. Marshall was strongly committed to the gold standard and to free trade which could be seen as the triumph of the abstract principle of comparative advantage. By contrast, Cunningham argued that economics related to the particularities of time and place rather than abstract laws. Whether gold or silver, free trade or protection was desirable therefore depended on the assessment of precise circumstances, and not on the application of abstract reasoning.

In the nineteenth century, the ‘cosmopolitan ideal’ of free trade was in harmony with British national interest, because it seemed possible ‘to dump English manufactures on every other part of the globe for all time’. But circumstances changed with the rise of economic nationalism which meant that British adherence to cosmopolitan free trade was harmful (Cunningham quoted in Green 2002: 58). Tariff reform should be linked with regulation of the domestic economy. These international and domestic policies were

⁹Cunningham’s riposte to Marshall appeared in the form of letters in *The Pall Mall Gazette* on 29 September 1892, and the *Academy* on 2 October 1892. However, Marshall left the matter, as he explained to Neville Keynes, and ‘shall not even suggest that Cunningham has read his history almost as hastily as he has his Ricardo and my poor little self’ (Marshall to Neville Keynes, 10 October 1892, quoted in Whitaker 1996a: 82–83).

connected, for the state was not opposed to individual interests, and was more than an aggregate of individual interests. Rather, 'the State is the embodiment of what is common to the different persons in the nation, it expresses the spirit which each shares ... We cannot represent the State as antagonistic to the individual citizens. The State is concerned with the general interest—with what is common to all' (Cunningham quoted in *ibid.*: 59). The historical economists therefore had a different reading from liberal economists of the relationship between the State and the individual, with major consequences for the understanding of Britain's future and for economic policy. Neo-mercantilist tariff reform could solve both the problem of international economic rivalries and domestic social problems by creating employment and prosperity (Green 2002: 60–61). The stakes were high, and the conflict was not merely about induction versus deduction: it was about the very future of Britain and its empire.

The disagreement reappeared in the debate over the Economics Tripos in 1903 and the teaching of economic history in the History Tripos in 1909. Cunningham had been a candidate for the Professorship of Political Economy to which Marshall was appointed in 1884, and in 1888, he resigned from his Lectureship to avoid Marshall's authority. He was elected to a Fellowship of Trinity College in 1888, which he held alongside appointments as Vicar of the University Church and Archdeacon of Ely, and Professor of Economics at King's College, London, between 1891 and 1897. However, he continued to be a troublesome presence in Cambridge. In 1903, Cunningham challenged both the claims of the new Economics Tripos and free trade ideology by offering a series of lectures on 'The Rise and Decline of the Free Trade Movement' which were advertised as 'a dispassionate survey of the main issues involved in the present controversy' for those 'who have never given special attention to fiscal questions, and who have no time for systematic reading'. Not surprisingly, the Economics Board was concerned that Cunningham was Director of Studies in economics at Trinity: the Board complained that he was in opposition to their approach, and urged the College to rely on teachers who were 'in harmony with the spirit of the Tripos' (Kadish 1994: 217–218; Hodgson 2005: 341).

Conflict resumed in 1909 when the Special Board for History and Archaeology proposed revisions to the History Tripos. As the regulations stood, students had to choose between economic history and political economy in Part I; candidates could avoid the need to take a theoretical paper, with the 'effect of encouraging the habit of accumulating facts instead of interpreting them'. In order to cure this defect, the preamble to the Report proposing the revisions said that there should be 'greater stress on the requirement of an elementary knowledge of Economic Theory in the paper on Economic

History', and the separate paper on economic theory would be dropped in Part I. The proposed new regulation stated that 'In English Economic History candidates shall be required to shew [sic] some knowledge of elementary Economic Theory in relation to History'. Clapham was a signatory (*Cambridge University Reporter*, 4 May 1909: 820–827).

The Report provoked a discussion in Senate House which was printed in the *University Reporter*. Not surprisingly, Cunningham took exception to the Board's comments on the teaching of economic history with its imputation that the teachers were 'quite incapable of preventing men from merely cramming'. In his view, the Board's proposal to introduce an element of theory rested on 'an entirely false antithesis':

[T]here was no alleged fact that might not lead one furiously to think, and think to good purpose; to think first of all whether it was a fact, to think of its far reaching significance, and of the connexion [sic] of things physical and moral which might have brought about that occurrence. Every single fact in History could be treated in such a way as to give the opportunity for plenty of thinking, and the antithesis between fact and theory seemed to him [Cunningham] to be entirely mistaken ... There were two ways of treating Economic History, either as an attempt to follow the growth of the economic life of a people from the beginning, or by viewing it from the modern standpoint and picking out incidents in the history of the past that can be used for the illustration of modern economic theory. The Board appeared to prefer the second mode of treatment ... It really misled. Instead of the student's being encouraged when he came across an alleged fact to weigh the evidence and consider whether it was a fact or not, he felt that if the incidence was merely an illustration it did not matter whether it was a fact or not. It entirely changed the character of the study (*ibid.*: 26 May 1909: 973).

Clapham sought to reassure Cunningham that economic history was not to be studied merely to illustrate 'modern principles'. Rather, the aim of the reform 'was directed to bringing the theoretical element into close harmony with the History ... [W]hat they meant was that it should be studied as in Dr Cunningham's own works, where Theory was introduced at points where it became important' (*ibid.*: 974). The Board compromised, agreeing to drop the proposed regulation on economic history and to continue with the existing regulation that 'in the paper on English Economic History... questions involving some knowledge of Economic Theory shall be included' (*ibid.*: 1 June 1909: 1,011).

Marshall was happy to assign the understanding of historical specificity to Clapham who approached the task from the left foot of induction

without hostility to the right foot of deduction. The appointment of Pigou as Professor of Political Economy in 1908 marked a shift towards a much more theoretical approach to economics with less attention to historical specificity than Marshall—a change that was encouraged by the weakness of the Historical School in Britain compared with Germany. In the words of Geoffrey Hodgson,

None of the leading figures of the British school was able to build an alternative methodology or theory, and they remained largely entrapped by an empiricist methodology ... [T]he historical school failed to establish an enduring bridgehead in the British Isles. Gradually pushed aside in academic argument, several members of the British historical school made their way into the discipline of economic history, embraced empiricism, and abandoned economic theory to the theorists (Hodgson 2005: 343).

This interpretation applied to Clapham who moved towards a somewhat resigned coexistence with economics. He rejected the German Historical School, remarking in 1929 that ‘as economists, I believe that the German historical school have gone bankrupt’ as a result of their aim ‘to dissolve economics into history’. The problem with Schmoller’s *Principles* of 1900–1902 was that it proposed that ‘historical delineation can become economic theory’. Clapham wrote on the fly-leaf of the book: ‘He solves nothing’. He had similar doubts about Sombart’s *Modern Capitalism*, pointing out that he was ‘neither precisely historian nor precisely economist (he isolates too much for the first and narrates too much for the second)’. Sombart’s main success was historical, but he ‘has not as yet taught us very much about the contemporary functioning of capitalism’ (Clapham 1929: 26–27, 30–31). Both Schmoller and Sombart, it would seem, had forgotten the need to keep the right and left feet marching together in harmony.

Clapham’s most developed and explicit statement about economics was his article ‘Of Empty Economic Boxes’ in 1922 when he expressed doubts on the utility of modern economic theory, particularly as developed by Pigou who was more assertive about the power of deductive economics than Marshall. To Clapham, such concepts as diminishing returns and increasing returns to scale were ‘empty economic boxes’. He suggested that economists could be divided into those who studied things and those who studied concepts and had great difficulty in filling their empty boxes with complex reality. He argued that empirical research should not be controlled by theoretical categories, and implied that facts should be studied prior to theory. He complained that ‘a great deal of harm has been done through omission to make clear that the Laws of Return have never been attached to specific industries: that we do

not, for instance, this moment *know* under what conditions of returns coals or boots are being produced' (ibid.: 312; italics in original). He took hats as an example. Returns could not be defined just in terms of the industry's own output. What of inputs—coal, rabbit fur from the Australian outback, shellac, leather, or wood pulp for the hatbox? Did coal always experience diminishing returns? It might in Britain but did this apply in the USA? Rabbit fur had elusive internal and external economies; it was difficult to say what applied to wood pulp. Hence the returns in producing hats were very complex. He accepted that car production had increasing returns, but it was difficult to decide on locomotive manufacturing or—returning to his earlier work on Yorkshire—the production of combed woollen tops. Clapham's approach was through cautious, painstakingly detailed understanding of individual trades and their organisational structure, with complex and changing interconnections, all resting on uncertain statistical foundations. He saw 'grave danger to an essentially practical science such as Economics in the elaboration of hypothetical conclusions about...human welfare and taxes' (ibid.).¹⁰ In this sense, he was closer to Cunningham's deductive approach, but without the rancour and without feeling that the disagreement was a matter of deep importance for the future of the discipline or country.

Keynes felt that Clapham was 'barking up the wrong tree' (Keynes quoted in Deane 2008: 799), and Pigou responded by defending the use of 'empty boxes'. Both methods could be used and more scholars should be produced in the mould of Jevons with 'the qualities required for conducting a detailed intensive study of particular industries', in addition to being 'well versed either in the more intricate parts of economic analysis or in modern statistical technique'. Until such paragons were produced, it was better for economists and historians to 'work together in combination and not...waste time quarrelling' (Pigou 1922: 465; Kadish 1994: 239–241).¹¹ In fact, the two disciplines went their own ways in peaceful coexistence or even indifference rather than the combative hostility of Cunningham or the integration of historical economics. The battle of methods was over. Clapham's tone was one of resignation: peace rested on accepting the task assigned to him by Marshall of filling empty economic boxes with empirical fact, even if the new generation of economists was less interested:

¹⁰The debate with Pigou can be traced through Clapham (1922a), Pigou (1922), and Clapham (1922b). See also Kadish (1994: 229) and Groenewegen (2012: 77–79).

¹¹Piero Sraffa felt that Clapham gave up too soon and had, without realising it, found a fatal error in Marshall (Sraffa 1926).

Here in Cambridge...economist and economic historian are at peace. We know our limitations. We can sit happily side by side under Adam Smith's great umbrella labelled *An Inquiry into the Nature and Causes of the Wealth of Nations*. The Professor of Political Economy will not cry out because I do not read a mathematical article (which, for the rest, I might not understand) dealing with taxation 'in a purely competitive system with no foreign trade', though, for all I know, it may throw much light on the Nature of Wealth and its taxability. I shall not resent his indifference to what I take to be the final demonstration, just completed by archaeologists and air-photographers, that the now familiar strips of the medieval open-field were unknown in Roman or Celtic Britain; although the change to the strips—being connected with an improved plough—was no doubt in its time a Cause of a Nation's Wealth (Clapham 1929: 32).

Clapham read very little economics beyond the works of Malthus, Ricardo, and other classical economists as part of his understanding of economic policy in the early nineteenth century, and of Marshall. He never read *The General Theory*, for he concluded from discussions with Keynes that it would be too difficult (Clark 1946: 348). Equally, economists showed little interest in the open fields of medieval England. They were two different disciplines, with different concerns. What, then, was Clapham's approach to economic history?

3 Clapham as an Economic Historian

Clapham's preference was for a revival of comprehensive political economy, and he continued to refer to himself as 'a political economist and historian. I underline the word political' (Clapham 1937: 117). At the founding meeting of the Economic History Society, of which he was to become President, he urged his colleagues to 'beware of becoming a "craft guild"', and to retain their links with both history and economics (Barker 1977: 15). In reality, Clapham's links were closer to history, and he did not provide a clear statement of what a comprehensive political economy would look like. Clark commented that 'as an historian, after his sheer capacity for work, his best quality was a power of reducing large masses of detailed facts to systematic form'. He preferred to supply concrete, accurate, and well-chosen information which showed the complexity of the past, producing a 'well-regulated assemblage of facts'. As a result, he was open to criticism for failing to analyse the significance of his material and for producing books that were divided into separate compartments rather than forming a single argument. Clark felt that Clapham was capable of constructing a theoretical

argument, but that ‘a certain modesty’ limited his willingness to do so (Clark 1946: 348). As Postan remarked, Clapham was the master of the *mot juste* and the arresting sentence, but not of the fluent page or balanced volume. Postan’s general conclusion on the three volumes of the economic history of modern Britain was somewhat ambivalent but fair: ‘[Clapham] was a pioneer in the sense in which all men who colonise virgin lands are pioneers; there were beasts and even men in the field before him, but he was the first to live and to build in a civilised way’ (Postan 1946: 57).

Clapham was always a historian rather than an economist in his methodology. His obituary of Eileen Power could apply to his own approach:

[She] was not an economist. She was not trained as one. That is unimportant: Ricardo was not nor, I think, Jevons. Much more fundamental—she would have hated to spend her life with attention concentrated on one aspect of human activity, and could never have brought herself to neglect men and women for generalizations about them ... And from the other side, as she was the first to allow, even proclaim, she had not that combination of speculative and practical interest and sagacity which makes the ideal economist (Clapham 1940: 351).

He elaborated the point in 1930, when he said that economic history

is a branch of general institutional history, a study of the economic aspects of the social institutions of the past ... [T]he method of economic history differs in no way from that of history in general ... The central problems of economic theory, although they may be stated in terms of some particular historical phase, are in essence independent of history. In theoretical discussion it is necessary to isolate forces and factors in a way which history does not permit (Clapham quoted in Kadish 1994: 241).

The main methodological difference between history and economic history, in Clapham’s view, was a reliance on statistics—as he said in his Inaugural Lecture, ‘it is the obvious business of an economic historian to be a measurer above other historians’ (Clapham 1929: 34–35). Phyllis Deane captured his approach well:

What Clapham had learned from Marshall was that economics is the study of mutually interacting quantities and that it was the function of an economic historian to put the key quantitative questions to the historical record—for example, how large? how long? how often? how representative?—when spelling out the chains of cause and effect linking economic events (Deane 2008: 799).

His approach entailed producing quantitative measures to reject or moderate easy generalisations, whether it be Malthus's law of population or Marx's claims on immiseration.

Clapham's use of statistics went with an awareness of their limitations. There might be one set of figures (say, the amount of wool exported in 1273), but not another to make sense of them (the price schedule for wool). It was necessary to be aware of how representative are data, and how useful they are for determining specific questions (Clapham 1929: 34). Although Clapham used statistics to challenge the assumptions of literary evidence, his use of them was limited by scepticism about their accuracy and by his methodology. Quantification was designed 'to offer dimensions, in place of blurred masses of unspecified size' (Clapham quoted in Deane 2008: 799) rather than for formal statistical analysis to construct arguments. The American economic historian Abbott Usher complained that '[Clapham] was so conscientious in his efforts to achieve accuracy of statement that he refused to follow to their conclusions a number of important principles of empirical analysis'. Neither did he allow himself 'to be distracted from narrative by incidental efforts to persuade readers to accept his judgements' which were usually in the nature of *obiter dicta*. Usher feared that by allowing the correct record to speak for itself without argument, readers were not weaned from 'superficiality and error'. The focus on what happened meant that little attention was given to why and how it happened, and Clapham did not allow statistics to get in the way of narrative and description (Usher 1951: 149, 150, 152).

Clapham used statistics in a rather limited sense, and warned that the statistician's world was different from that of the historian. His aim was to balance the 'unreality of the generalised statistical statement' with 'scattered individual facts' to produce a sense of divergent social realities throughout Britain (Clapham 1926: viii). John Saltmarsh, a fellow economic historian at King's, pointed out that what mattered to Clapham was 'men and women, the things they made, the villages and the towns and the land in which they lived, came first, for their own sake. Explanations and theories came afterwards' (Saltmarsh quoted in King's College 1949: 8). He did not see statistics as the be-all and end-all. In his Inaugural Lecture, he pointed out:

If the economic historian has his modesties in presence of the pure economist he also has his pride. He is proud because, by definition as historian, he is one to whom the tangled variety of human life is attractive in itself; one who will study alterations in the tangle for the love of it, even when his information is such that he can never hope to pick out with assurance the forces at work, or measure exactly the changes brought about by the aggregate of them between

dates x and y. He cares for the beginnings of things as such. He likes to trace the growth of institutions which have been moulded by man's need to keep alive and man's desire for comfort and prosperity—village communities, trading companies, Christmas goose clubs—although he may not be able to number the community, or find the slate of the goose club. It pleases him to know that in such and such an age caravans took the golden road to Samarkand, and that in such another age they went no more, even if he cannot count the camels or prove—what he always suspects—that the total amount of the rose-candy spikenard and mastic conveyed was really trifling (Clapham 1929: 34–35).

In his *Concise Economic History*, Clapham set out his view of human personality that could be seen as a rejection of *homo economicus* and acceptance of the views of Cunningham: how people lived with their family, what songs they sang, what they thought looking at the sunset, what prayers they made, were more important than the nature of their tools or how they swapped with neighbours. 'Economic advance is not the same thing as human progress': a man with a motor car may have less imagination than a man at Stonehenge. But he then pulled back. Commercial and industrial life had its own morality and pleasures: '[E]conomic activity, with its tools, fields, trade, inventions and investment, is the basement of man's house'. Economic basements need not be dull, for 'A patch of earth dug level, a right stroke with a felling axe, a neat bit of welding, a locomotive brought smoothly to rest, even a tidy balance sheet or a quick calculation in forward exchange, all yield the craftman's, not to say the artist's, satisfaction' (Clapham 1949: Introduction).

In Clapham's world, there was no simple divide between *homo economicus* and a wider conception of human personality: an industrial and commercial society rested on values of integrity, hard work, and pride in a job well done. Much the same was true of Marshall who rejected the idea of economic man free of 'ethical influences' and altruistic motives. As Marshall said in the third edition of the *Principles* in 1895, his aim was to deal with a man of flesh and blood and not an abstract economic man,

a man who is largely influenced by egoistic motives in his business life to a great extent with reference to them; but who is also neither above vanity and recklessness, nor below delight in doing his work well for its own sake, or in sacrificing himself for the good of his family, his neighbours or his country; a man who is not below the love of a virtuous life for its own sake.

Similarly, the policy of free trade was complemented by the creation of an active associational life, such as goose clubs or friendly societies, liber-

ated from the monopolies of the past; it rested on small businesses that both competed with each other and developed ‘constructive cooperation’ in industrial districts.¹²

Clapham has been characterised as a ‘minor Marshallian’. He was cited in the notes to Marshall’s *Industry and Trade* (Marshall 1919: 62, fn. 1, 71, fn. 1, 232, fn. 2, 691, fn. 1) and in the fifth edition of the *Principles* (Marshall 1907, volume 1: 747, fn. 1, and volume 2: 732); in turn, Clapham cited *Industry and Trade* in the notes to his *The Economic Development of France and Germany, 1815–1914*, in respect of the aims of German cartels and banks (Clapham 1921: 310, fn. 1, 393, fn. 1). But there was, in the words of Peter Groenewegen, ‘only a few Marshallian flourishes’ (Groenewegen 2012: 70) in the book, such as changes in productive organisation; the role of cooperation in the development of European agriculture which was favoured by Marshall; and the increasing scale of manufacturing production with the emergence of cartels in Germany. Generally, the book was very cautious in coming to firm conclusions as a result of the low quality of available statistics. Like Clapham’s work on the woollen and worsted industry, the book relied on a detailed analysis of the two different experiences, without any generalisation from the case studies or reference to a general theory of economic development. The start and end points defined a ‘great age’ between wars. The victory at Waterloo marked the end of the European wars and the unusual position of the Continent in relation to England.

Clapham agreed with Schmoller that the great social question for Europe up to 1850 was the peasant question. The revolutionary land settlement made the peasant his own master, even if he continued to farm the land as before—and the policy ran from France to emancipation in Prussia and Russia, on to Irish land legislation. By contrast, Clapham felt that the revolutionary age was less decisive in industry. Lifelong wage earners were still a minority and revolutionary legislatures were more concerned to rid industry of medieval restrictions and guilds; the problem of wage contracts hardly interested them, and they did not have the same sympathy for wage earners as for the land. But revolutionary labour policy did clear the way for industrial growth, and its unsympathetic attitude to industrial workers was made permanent in the Napoleonic Codes, which made French urban workers hostile to the law. Commerce was less affected by the revolutionary and Napoleonic reforms, meaning that after the war it could revert to something like the conditions of the late eighteenth century so that the late

¹²On the ideology of free trade, see Trentmann (2008); on wider notions of personality, see Pearson (2004); on Marshall, see pp. 34–37 of Pearson (ibid.) quoting Marshall (1890: vi) and (1895: 26–27); on industrial districts, see Marshall (1919: 249, 324–325, 577–578, 582–584, 590, 605–608).

nineteenth-century merchant experienced less change than the manufacturer or peasant. Above all, the defeat of Napoleon led to a period of peace which meant that English mechanical knowledge became available. In Clapham's view, peace had never before been associated with the release of new economic forces on such a scale, and nations came together as good Europeans in economic matters more than at any time since the fall of Rome. The book on France and Germany ended in 1914, without any reflections on the closing of that 'great age' and why European nations went to war. The implication is that economic activity brought nations together, creating prosperity and peace, and war presumably had other causes (Clapham 1921: especially the Introduction and Epilogue).

In the *Economic History of Modern Britain*, Clapham paid some attention to the economists of the early nineteenth century, briefly mentioning the influence of Ricardo on debates over free trade and banking; McCulloch and Ricardo on post-war debt; and Malthus on the burden of the Poor Law and population. Even here, he paid little attention to the content of their theories, and he hardly mentioned later ideas. He certainly did not mount any critique of Ricardo. Usher justifiably complained that Clapham's account of the Bank Charter Act of 1844 in both *The Early Railway Age* and *The Bank of England* failed to grasp the competing theories of money and credit in the nineteenth century, and felt that Clapham drew too rigid a line between history and theory (Usher 1951: 151–152). A rare example is a reference to monopoly price theory in *The Early Railway Age*, merely to note that it was not used in debates over the railways in 1839–1840; Clapham passed on with the comment that the indifference to the theory of monopoly was excusable because at no time did a company 'secure monopoly revenues of even tolerable size, when reckoned in percentages'. Clapham did not provide any evidence for his claim about monopoly profits, and it is not possible to find any hypothesis that could be tested (see Groenewegen 2012: 74–75; Clapham 1926: 54, 56, 271, 312, 334–335, 349–350, 362, 497, 521; on monopoly prices, 416).

Clapham's economic history is now little read, and there is no obvious 'Clapham thesis' that has provoked and stimulated discussion. Clapham was not interested in the 'substantial analysis of historical process', and Usher caught his limitations and achievements well:

He was strategically placed to assume leadership in an empirical reaction against the mechanistic and idealistic systems of the Marxians and the ideal-type sociologists, but by temperament and background he became committed at an early date to a limited program, which he carried out with great skill and unusual literary distinction (Usher 1951: 153).

Nevertheless, Clapham did have a view of the economic development of Britain. He was a product of the free trade industrial society of the late nineteenth century, and he assumed that a market economy, with some concern for welfare, offered solutions to the problems of poverty. The final paragraph of his economic history of modern Britain up to 1914 ended in much the same tone as his economic history of France and Germany, combining literary finesse with a failure at explanation:

Thinkers and dreamers might well be discontented with the order of society or with the rate at which that order was being changed; but no honest man with a reasonably long and accurate memory, and some appropriate knowledge, could deny that it was a better order, if better only by a little, than at any time in the modern industrial age. Whether that age was itself in any profound sense good some doubted, as many have doubted since ... Of uprightness, wisdom and the clearness of the eye the economic historian as such may not profess to speak. He moves on the lower plane, the plane of commodities and comforts. Moving there, he does not hesitate to compare that time to its advantage—not only with other times in the industrial age, but with any time certainly known to him. And to those who lean towards quotation from the Book of Ecclesiastes he relies from that same discerning Book: ‘Say not thou, “What is the cause that the former days were better than these?” for thou dost not enquire wisely concerning this’ (Clapham 1938: 506–507).

Clapham’s methodology might have something in common with Cunningham but his interpretation was very different. He had a more sanguine view about *laissez-faire* or free trade economics, and the benefits of a commercial society in spreading both prosperity and stability. This turns us to a third point: Clapham’s views on politics and policy.

4 Clapham and Politics

Clapham accepted Marshall’s point that economic history could not be entirely separated from an interest in contemporary social problems, but wished to keep his distance from immediate political or policy aims. Clark remarked that ‘His purpose was scientific: he wanted to make available the information which economists, statesmen and general historians needed, and in the form which would be useful to them’ (Clark 1946: 348). Clapham was a free trade liberal and supporter of social reforms, but he did not use economic history for immediate political ends as did Cunningham

(in arguing for protection) or the Hammonds, Tawney, and Cole in arguing for social reform or socialism. Thus, Clapham took a balanced— or complacent—view of the economic policies of the early nineteenth century when judged by the standards of the time. There were faults of debt, taxation, commercial policy, unregulated town growth, and the Poor Law, but there was also a limit to the amount of creative legislation that could be implemented by any government, and Britain did better than other countries. He pointed out that George IV's London was insanitary but nothing like as bad as Charles X's Paris, and the French death rate in the 1820s was 50% higher. He concluded that 'the over-governed continentals of the early nineteenth century rightly gave credit to governments which knew when to hold their hand, to *laissez faire*—*laissez passer*, and to governments which had been able to preserve a good inheritance reasonably intact' (Clapham 1926: 317).

One of Clapham's few contributions to current policy debates was an article on 'Protection and the Wool Trade' in 1904, in which he criticised tariff reformers as 'amateurs in economic pathology' (Clapham 1904: 641), warning that retaliation would lead to tariff war, disorganisation of trade, and no prospect of a reduction of duties after the war (Kadish 1994: 227). But he did not adopt a dogmatic position. He was careful, rejecting overly assertive generalisations: policies should pay regard to the different contexts and circumstances of each country. In an article on the French economy in 1907, he remarked that tariffs would not create full employment—but that without them, French industries would have suffered more than they had. His book on the woollen trade took a balanced view, arguing that the case rested on comparative economic research rather than categorical assertion:

In Germany protection is associated with rising, in France with falling exports. If any fiscal moral were to be extracted from the facts it might run somewhat as follows: that at times of fiscal controversy there is a tendency to exaggerate the importance of government action, both positive and negative, and to underrate the effects of those deep working economic forces for which Acts of Parliament have but a limited and an indirect control. That this is a moral distasteful to the controversialists cannot be helped (Clapham 1907: 203–214).

The implication was that such topics should be removed from theoreticians and passed to political economists and their successors—economic historians (Kadish 1994: 228). It was not a challenge to which he was to turn.

Although the debates over tariff reform and free trade were fierce, and set Clapham's two mentors against each other, the third volume of his economic

history of modern Britain was largely silent on the issue. There were only two references: to a full-dress discussion of imperial and commercial policies in 1903–1905; and to the fact that free traders won the British general election with a ‘monstrous majority’ and ‘were content almost to complacency’ (Clapham 1938: 41, 51). He did not assess the case on each side, or set out their counterarguments, let alone subject them to any statistical scrutiny. His general conclusion was that Britain was as fully occupied as any country could have hoped to be in 1910–1913—a further example of his willingness to assert rather than analyse—but was not ‘stirring’ (ibid.: 71) as were Germany and the USA or as she herself had been in the past. Nevertheless, Clapham felt that ‘her conservatism had been shaken and that she was preparing, at her own pace, in those last years, to prove that she was not decadent, though both enemies and desponding friends often said that she was’ (ibid.).

He came to a similarly bland view on the return to the gold standard after the war, where Foxwell and Marshall took different views. He pointed out that Britain had gone back to gold after the Napoleonic Wars and did so again in 1925. He merely noted that the Committee on the Currency did not discuss other options of a devalued sovereign or managed currency as a way of avoiding the discomforts of returning to gold as a result of sterling prices being out of line. Clapham pointed out that everyone knew there would be discomforts, and critics anticipated many—a nod to his colleague Maynard Keynes’s critique of the decision. But his conclusion was even-handed. Although the case of the critics seemed to be proven when Britain came off gold in 1931, ‘it is far from certain that the ultimate distress was inherent in the original decision’. He left the matter there, without explaining why the return to gold might not have been the main reason for the economic difficulties of the late 1920s, or what share it might bear (ibid.: 538). As in the case of free trade, controversial areas of economic policy were treated briefly without engaging with the arguments on either side.

Unlike the work of many other economic historians in the late nineteenth and early twentieth centuries, Clapham’s interest in economic history did not emerge from the ‘pursuit of some external ideology’ (Kadish 1994: 223). However, there was a strong implicit ideology. His emphasis on economic forces as natural and superior to government action could lead to accusations that his work was an apologia for free-market capitalism. As Postan pointed out, Clapham had ‘an admiring appreciation of the self-adjusting action of the economic mechanism, and a horror of sudden and dramatic change’ (Postan 1946: 56). His account of the development of the British economy in the nineteenth century avoided the moral outrage at capitalism that had been expressed by Engels, Toynbee, or the Hammonds, and left-wing critics found him to

be complacent. Clark offered a defence, arguing that Clapham accepted the need for Liberal reforms designed to remove the imperfections in the market; he believed that ‘great inequalities of wealth are a danger and an evil’; he supported gradual reform to prevent casual employment and to make towns healthy, to control dangerous and unhealthy work, and to remove the worst inequalities in wealth so that ‘trade and classes will have learnt to work better together than they now do’ (Clark 1946: 347–348). These changes rested, in Clapham’s words, on the growth of Christian virtues of ‘self-restraint, self-denial, an honest attempt on the part of all classes to understand and help one another ... A Christian nation in the real sense of the word would certainly come very near to the socialist ideal’ (Clapham 1909: 101).

Clapham was aware of the criticism of his work. In the Preface to the reissue of *The Railway Age* in 1939, he noted the complaints of John Hammond that he gave a ‘happy impression’ (Clapham 1939: ix) of the period. He pointed out that he did not mean that everything was getting better, only that recent historians stressed ‘worsenings’ and ignored the ‘betterings’. He argued that excessive concentration on shadow led historians to miss patches of sunlight:

It is very easy to do this unawares. Thirty years ago I read and marked Arthur Young’s *Travels in France*, and taught from the marked passages. Five years ago I went through it again, to find that whenever Young spoke of a wretched Frenchman I had marked him, but that many of his references to happy or prosperous Frenchmen remained unmarked. Sympathy with wretchedness is the sign of a generous mind. Let us hope that the attempt to record other things, in their due proportion, does not denote an ageing heart hardened by statistics (ibid.: x).

Perhaps the most explicit expression of Clapham’s views came in the Epilogue to his economic history of modern Britain, when he reflected on the outbreak of the First World War in a way that he did not attempt in the book on France and Germany. He now asked whether industrial and capitalist civilisation, the private control of the means of production, led to war. He recognised that mercantilist belief that the volume of trade was limited could lead to tensions, but he argued that there was ‘no essential connection with capitalism ... The search for markets and the desire to retain them, with all the frictions which they may set up in a world of expanding national economies, were not vicious products of capitalist greed’ (Clapham 1938: 514). Sometimes trade might entail immorally pushing goods into reluctant markets and then demanding State support, but Clapham took a more benign view of trade that had more in common with Richard Cobden:

Merchants and manufacturers of the nineteenth century thought of themselves as lovers of Peace, which they certainly were, and of their work as a fosterer of Peace, which has not yet been proved false in spite of secondary trade wars. The charge most often brought against them then was that they loved Peace too well, for their pockets' sake, Peace without Honour ... At least, during the generations which these economic men had most power, the world for ninety-nine years was free from that 'general war' with which the statesmen of the eighteenth century had been thoroughly familiar (*ibid.*: 515).

This interpretation was close to the implications of Clapham's account of French and German economic history up to 1914. He accepted that industrialists produced 'terrible engines of war' (*ibid.*), yet he was not convinced that the arms trade had anything to do with the outbreak of war in 1914. For industrialists, war was likely to mean loss of trade, high taxes, and death in the family—that it brought high profits did not prove they wanted war, any more than the high wages secured by workers meant that they wanted war: 'Industrialists as a class were everywhere pacific; merchants and money-handlers even more so' (*ibid.*: 516). Clapham's conclusion was that the civilisation of the Victorian era was less warlike than any that came before: '[T]he individual selfishness to which it gave scope, in the ownership of property and other ways, was not more threatening to the peace of the world than that centralised, impersonal, property-controlling or property-owning state selfishness which shows signs of succeeding it for a time' (*ibid.*: 518).

It is easy to see Clapham as unquestioning of the values of the late nineteenth century in which he grew up. Yet the final words of the economic history of modern Britain showed some of the virtues of that culture. He reflected on the desire for greater equality in claims to resources—something that 'all men of good will and good sense were bound to share', with differences only of method and degree concerning what other values are to be surrendered, and what degree of equality is to be wished:

Hanging behind all thought and discussion of such matters were—or should have been—the reflections that almost the least propertied of their countrymen was already a privileged member of the human race; that the talk of a world of plenty which needed only to be organised, a way of speech then coming into fashion among social experimentalists, was not yet relevant to a world some two-thirds of whose inhabitants had not, by Western standards, decent clothing for their backs or plain food enough to eat; and that the privileged position of Britain, and indeed of the white races, though much less insecure than some pessimists maintained, was not quite certainly a part of the permanent divine order of things (*ibid.*: 554).

5 Conclusion

By the time of Clapham's death in 1946, economic history and economics at Cambridge had moved far beyond the methodological disputes of Cunningham and Marshall. The production of the first national income statistics by members of the Faculty of Economics eventually led to the compilation of long-run series that transformed the analysis of economic growth, using statistics for analytical purposes that had not been attempted by Clapham. By contrast, Maurice Dobb was applying Marxist theory to economic history in order to understand the transitions from feudalism to capitalism—a debate that created competing interpretations and explanations of long-run development that could not be teased out of Clapham's descriptions. Meanwhile, Clapham's successor, Michael Postan, was developing a non-Marxist approach to the medieval economy that owed more to Malthus and long-run trends in population and food, and rested on statistical analysis rather than description. The fate of most historical work is that it fades very soon, to be replaced by new approaches. Clapham's work faded even more quickly than most. But one influence has persisted: economic history has remained a compulsory element of both the History and Economics Triposes, alongside political and constitutional history, and theoretical economics.

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20

A.C. Pigou (1877–1959)

Karen Knight and Michael McLure

1 Introduction

David Collard (1981: 132) has described Arthur Cecil Pigou as elusive, partly because he was ‘caught between the shadow of Marshall and the pyrotechnics of Keynes’. Other factors that contributed to this elusive standing include a lifelong discipline of keeping his professional and personal life distinctly separate; personal traits that, as Saltmarsh and Wilkinson (1960: 16–17) have recalled, revealed a great shyness towards casual acquaintances (particularly with regard to women); and a tendency to be ‘brusque when privacy was invaded without warning’ (ibid.). But, notwithstanding these elusive qualities, Pigou’s scholarly achievements place him in the first rank of economists from the Cambridge School. His international reputation for major contributions to the economics of welfare, unemployment, money, business cycles and consumer demand was substantial by any standard, and, within

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the limits that his personality would provide for, ‘the Prof’, as Pigou was widely known, was conscientious in his efforts to foster the development of Cambridge economics during his tenure as the Cambridge Professor of Political Economy.

Pigou was born at Ryde on the Isle of Wight in 1877 and died at Cambridge in 1959. He was the eldest child of Clarence George Scott Pigou, an army officer in the 15th Regiment, and his wife Nora Frances Sophia Pigou (née Lees), the second eldest daughter of Sir John Lees, the 3rd Baronet of Blackrock.¹ A.C. Pigou’s brother, Gerard Clarence, was born the following year and his sister, Kathleen Marie, in 1881. Within the extended Pigou family, a tradition had developed from the 1820s whereby the eldest son of the family attended Harrow School and that tradition was followed in Arthur’s case. David Champernowne (1959: 264) has described Pigou as ‘a god among mortals’ at Harrow, where he excelled both academically and in athletics and won the respect of both teachers and peers. Pigou was elected a School Monitor in 1894 and in his final year he was made Head Boy. He left Harrow at the end of 1895 after having won the Clayton Scholarship for Modern Studies, which he used to attend Cambridge University and reside at King’s College.

Champernowne (ibid.) has also recalled Pigou’s ‘refusal to be diverted by any other ambitions’ enabling him ‘to live a completely uncomplicated life’. This uncomplicated life encompassed two lifelong endeavours: his intellectual and professional activities at Cambridge, and mountaineering, an activity that was encouraged by his King’s colleague, the economic historian John Clapham, during the first decade of the twentieth century (Austin Robinson 1968). Pigou spent the majority of his life at King’s. When not at King’s, he became a proficient climber, scaling peaks in the Alps and the Lake District where he had a cottage built at Lower Gatesgarth, Buttermere, in 1911.

2 Early Years at Cambridge

When admitted to King’s College in October 1896, Pigou fully exploited the prospects of academic and co-curricular activities open to students residing in Cambridge. In 1899 he received the Chancellor’s Gold Medal for English verse, which was awarded in recognition of his ode to *Alfred the*

¹ The Baronetcy of Blackrock in the County of Dublin was created for John Lees, soldier, politician and administrator, on 30 June 1804, in the Baronetage of the United Kingdom (Lundy 2012).

Great, a literary treatment that refers to ‘truth’s bright star’ and ‘Reason’s light’. Later that year he obtained a First in the undivided Historical Tripos but, given his growing interest in economics and ethics, he then commenced Part II of the Moral Sciences Tripos, which he also completed with a First, in 1900.

In 1900, while studying for Part II of moral sciences, Pigou entered his essay on *Robert Browning as a Religious Teacher* into competition for the Burney Prize—awarded to the best essay submitted dealing with the philosophy of religion—which he won, with the essay subsequently published (Pigou 1901a). In the following year, Pigou’s dissertation on *The Causes and Effects of Change in the Relative Values of Agricultural Produce in the United Kingdom during the Last Fifty Years* (Pigou 1901b) was awarded the Cobden Prize.

As Pigou turned his mind to his future career, he decided to enter the Fellowship competition at King’s, which requires submission of a Fellowship thesis that is assessed by referees associated with the College.² In 1901, he submitted his Burney Prize-winning essay on Browning as his Fellowship thesis. While Marshall was not a referee for this Fellowship dissertation, he was nevertheless asked by the Provost of the College for his views on Pigou and wrote that ‘With perhaps one exception, I have never wished so strongly to see any student retained at Cambridge, as Pigou’ (Marshall to Leigh, 8 March 1901, quoted in McLure 2013a: 278). But notwithstanding this strong support from Marshall, the dissertation on Browning was not successful in obtaining a Fellowship for Pigou at King’s in 1901. In the following year, he submitted his Cobden Prize-winning dissertation to King’s as part of the Fellowship round and was successful.

Between being elected as a Fellow of King’s in 1902 and being appointed as the Professor of Political Economy at Cambridge in 1908, Pigou acquired new teaching duties that befitted a Fellow, initially by relieving Marshall from his general course in economics, then, more formally, as the Girdlers’ Lecturer in Economics at Cambridge from 1904 until 1907. During this time, he first emerged in debates on trade policy undertaken within the Cambridge Union. Notable in that regard was Pigou’s confrontation with the protectionist Sir Howard Vincent (Saltmarsh and Wilkinson 1960: 7), who had founded the United Empire Trade League in 1891.

² Pigou’s attempts to obtain a Fellowship, and the referees’ assessments of his Fellowship dissertations, are discussed in McLure (2013a).

Pigou's 1908 appointment to a professorship was controversial. He was only 30 years of age upon succeeding Marshall and, although Pigou had shown great promise in the application of the theoretical tools that Marshall had developed, the extent of his experience and maturity of thought was limited by his young age. The other candidates—which included Herbert Foxwell at Cambridge, William Ashley at Birmingham and Edwin Cannan at the London School of Economics (LSE)—all had greater experience. The voting pattern of the electors is not known, although it is certain that Marshall was active in lobbying in Pigou's favour and that some of the electors found this to be inappropriate, unwarranted and distressing.³

The period at Cambridge immediately before the First World War has been described as Pigou's heyday (Saltmarsh and Wilkinson 1960: 19), but during the Great War his position attracted a considerable amount of ill will as Cambridge University made a special case to have Pigou exempted from military service on the grounds that his duties as Professor of Political Economy were indispensable. This decision was unsuccessfully contested at tribunal by others, including Foxwell (Aslanbeigui 1992). During the war, Pigou was a volunteer ambulance driver in France and Italy during breaks in the academic year.

3 Scholarship

Pigou wrote more than a dozen books and over 100 articles. After he transitioned from being a Cambridge student to a Fellow, he won the Adam Smith Prize at Cambridge in 1903 for an essay which was revised and published in 1905 as the *Principles and Methods of Industrial Peace* (Pigou 1905). This essay, which also formed the basis of his eight lectures for the 1903–1904 Jevons Memorial Lectures at University College London on *Associations of Employers and Employed, Arbitration and Conciliation*, is of interest to intellectual historians for the framing of labour market problems with reference to ethics: 'The problem of this book is ethical—to determine what principles and methods ought to be employed in the settlement of industrial differences ... But the solution of the ethical problem can only be reached with the help of an investigation of actual and recent experience' (Pigou 1905: ix).

³For further discussion on Pigou's appointment, see Coase (1972), Coats (1967) and Jones (1978).

While he recognised that the issue of industrial peace ‘is not confined to the narrow circle of economists’ (ibid.: vi), the analysis is still rich in economic ideas, with the explicit and formal use of economic analysis reserved for the appendices. At its most general level, the *Principles and Methods of Industrial Peace* reflects Pigou’s view that social problems are themselves ethical in character. The particular role of economic analysis—in the case of this book—was generally derived from Marshall’s *Principles*, but Edgeworth’s *Mathematical Psychics* was influential too. As such, Pigou was consciously aligning his views on the relevance and purpose of economics with those of his teacher, Marshall. Indeed, even the subject of this book was suggested to him by Marshall (ibid.: vii). The book is also of historical interest for its focus on an issue that subsequently featured prominently in Pigou’s formal definition of economic welfare, namely improving the living standards of the working poor and their families.⁴ It also features an important theme of his subsequent work on welfare economics, notably the complexities associated with balancing efficiency, in this case, efficiency in the setting of wage rates, with broader redistributive goals.

Pigou critically assessed the protectionist policies of the British Colonial Secretary, Joseph Chamberlain, in his booklets, *The Riddle of the Tariff* (Pigou 1903a) and *Protective and Preferential Import Duties* (Pigou 1906a), and in his subsequent essays in the *Fortnightly Review*, ‘The Known and the Unknown in Mr Chamberlain’s Policy’ (Pigou 1904a), and the *Edinburgh Review*, ‘Mr Chamberlain’s Proposals’ (Pigou 1904b) and ‘Protection and the Working Classes’ (Pigou 1906b). All of these essays are critical of protective tariffs and imperial preference, with Pigou estimating the loss of national income due to tariffs and noting that redistribution in response to preferential tariffs tended to favour landlords (Pigou 1904a [2002], (1904b) [2002]), and rejecting the proposition that protection increases economic stability (Pigou 1906a). Collard (2002: xii–xiii) astutely pointed out that while the polemical tone of these papers is readily evident to the reader, the level of analysis with which they are supported would not be out of place in the *Economic Journal*. The issue was important to Pigou, who joined Edgeworth, Marshall and 11 other prominent economists in signing

⁴‘It is prima facie desirable that arbitrators should seek somewhat to modify the general distribution of wealth awarding to poor workpeople higher wages than the trend of economic forces would naturally bring about, provided that these wages seem likely to come from the pockets of relatively wealthy persons’ (Pigou 1905: xi). Takami (2014) provides an interesting reconstruction of the intellectual environment that Pigou faced at Cambridge, including discussion of the potential influence of the Fabian Society and socialism more generally on Pigou’s thinking up to 1912.

a joint letter that was published in *The Times* on 15 August 1903, under the heading ‘Economics Professors and the Tariff Question’, which questioned the logic of protectionism.⁵

During this early period of his career, Pigou also devoted time to core issues in equilibrium economics that laid the formal foundation for his subsequent work on welfare theory. In ‘Some Remarks on Utility’, (Pigou 1903b) Pigou considered the relationship between demand and utility when an individual’s assessment of the marginal value of their consumption changes with variations in the consumption of others. In modern Pigouvian language, it investigated whether demand-related externalities exist when goods are not common across society as a whole, but are common amongst a subclass of society that one may aspire to, such as diamonds and tuxedos among the wealthy elite. His conclusion was that such externalities do indeed exist but that they represent a relatively small influence on an individual’s consumption and can be ignored in a first approximation study of individual demand. He returned to the subject again at the end of the decade, demonstrating that, instead of simply ignoring this influence because it is ‘small’, it can be considered analytically as a demand externality.

Finally, and importantly, Pigou continued his interest in ethics and philosophy. This was no passive interest either. Rather, it was an area of study that he took seriously. In 1908, three of Pigou’s earlier articles from the *International Journal of Ethics*, namely ‘The Problem of Good’, ‘The Ethics of the Gospels’ and ‘The Ethics of Nietzsche’, plus one article from the *Independent Review*, namely ‘The Optimism of Browning and Meredith’, were republished in his book *The Problem of Theism and Other Essays*, which also included three new chapters: ‘The General Nature of Reality’, ‘The Problem of Theism’ and ‘Free Will’. It should also be noted that the chapter entitled ‘The General Nature of Reality’ is particularly notable for drawing out the methodological issues of social enquiry that derive from philosophical thought and which pertain to Pigou’s subsequent method of science, in that it recognises that at least part of the independent reality that social scientists deal with concerns the spirits of living men. Consequently, in the lead-up to Pigou being appointed a professor, his preparations for serious reflection on economics were preceded by philosophic reflection on the nature of what is good, the nature of science and the tools of economic analysis. In other words, he was

⁵The letter is reprinted in Coats (1992: 314–315).

laying the philosophical grounding for his seminal works in the field of welfare economics.

Less than two years after succeeding Marshall, Pigou contributed two papers to the *Economic Journal* that had notable legacies. First, ‘A Method of Determining the Numerical Value of Elasticities of Demand’ (Pigou 1910) contained the proposition that the own price elasticity of demand for a specific good is related proportionally to the income elasticity of demand for that good, which subsequently became known as ‘Pigou’s Law’ (Deaton 1974). Once this proportionate relationship is derived, own price elasticity of demand can then be estimated indirectly from empirical estimates on income elasticity of demand. Pigou started by assuming that individuals on a similar income have a similar utility for the good in question and, using an additive representation of utility, he investigated the consequences of the law of one price for the total utility and marginal utility as income changes. Second, in ‘Producers’ and Consumers’ Surplus’ (Pigou 1910 [2002]) Pigou developed his analytical apparatus to systematically account for externalities (although without using the term): both demand-related externalities, where an individual’s demand is influenced by others’ consumption of consumer goods, and supply-related externalities, where the supply of a good affects the well-being of people who are not a direct party to the exchange of the good in question. The efficiency consequences of these factors are discussed in that paper, as are corrective measures.⁶

Pigou’s first truly great work, *Wealth and Welfare* (Pigou 1912), investigates the relationship between wealth, which he characterises as the national dividend, and welfare, which he considers with reference to two general propositions. First, welfare concerns states of consciousness only, but, as consciousness is too difficult a concept to work with analytically, Pigou presents ‘economic welfare’ as a part of a broader conception of welfare, one that can be considered with reference to the ‘national dividend’. Second, that welfare can be considered by categories such as ‘greater or lesser’. While welfare and economic welfare are broadly correlated, they are not synchronised to the point where a change in economic welfare necessarily yields an equal change in welfare, but in a probabilistic sense, and without information to the contrary, Pigou’s starting point is that increases or decreases in economic welfare can be considered with respect to changes in the national dividend.

⁶The relationship between ‘Some Remarks on Utility’ (Pigou 1903b) and ‘Producers’ and Consumers’ Surplus’ (Pigou 1910 [2002]) is considered in McLure (2010).

Within this context, Pigou (1912: 66) contends that an increase in economic welfare is associated with three conditions: (1) 'an increase in the size of the national dividend', which we may call the 'wealth-efficiency' condition; (2) an 'increase in the absolute share of the national dividend accruing to the poor', which we may call the 'distributive fairness' condition; and (3) a 'diminution in the variability of the national income, especially of the part accruing to the poor', which we may call the 'macroeconomic stability' condition. For welfare in general the key question for Pigou was the relative 'harmony' and 'disharmony' between each of these three conditions. Economic welfare is enhanced, at least in a probabilistic sense, when an outcome meets one or more of these three criteria and there is no disharmony with the remaining conditions. But Pigou does not rule out the possibility of economic welfare improving even in the face of disharmony, such as when disharmony is only evident in the short run. On the issue of distributive fairness, the utilitarian foundation to his thinking on interpersonal comparison is clear, as he accepts that the marginal utility of money to a poor man is greater than that of a rich man, but he is extremely cautious in the use of that principle. On the question of macroeconomic stability, Pigou is concerned that lower-income workers are disproportionately affected by economic downturns, so policies that diminish fluctuations are welfare enhancing. Once such downturns occur, however, he advocates 'equalising actions' in the form of public works to offset the welfare losses from recessions and depressions. When people who would otherwise have been unemployed,

can be turned to some task of 'actual and substantial utility,' it [the cost of equalising action] may be comparatively small. This class of consideration helps to determine from how large a quantity of equalising action economic welfare would benefit. In all circumstances, however, advantage can be obtained from some quantity of equalising action (Pigou 1912: 476–477).

It is while addressing the issues related to the wealth-efficiency condition that the theoretical apparatus developed provisionally in 'Producers' and Consumers' Surplus' found full expression in an analysis of market failure, predicated on the view that money is the measuring rod of utility (i.e. a cardinal conception of utility). The resulting analysis revolutionised thinking about the potential for government to correct market failures through what we now call Pigouvian taxes, when negative externalities from a particular activity result in overinvestment in that activity.⁷ The significance of Pigou's

⁷ Indeed, today the manifesto of the so-called Pigou Club, established by Gregory Mankiw (2006), advocates the imposition of a Pigouvian tax on gasoline to combat, amongst other things, global warming and road congestion.

work here is readily evident from the force and quality of the counter-theses that emerged in response, with the old (cardinal) welfare economics of Pigou being contrasted with the new (ordinal or Paretian) welfare economics of Hicks and the Coase (1960) critique of Pigou's analysis of market failure and social costs through the development of what became known as the Coase Theorem, that is, clearly defined, and legally enforceable, property rights have the potential, in many instances, to eliminate the need for correction of market failure by public authorities.

In response to the second edition of Irving Fisher's *The Purchasing Power of Money* (Fisher 1913), Pigou wrote 'The Value of Money' (Pigou 1917) for the *Quarterly Journal of Economics*. This article is significant because it was the first time that the Cambridge equation version of the quantity theory of money, in which the real value of a unit of legal tender is given by the proportion of real economic activity held in real cash balances, was published in print. Pigou does not claim too much by way of originality for the development of this equation, as it is implicit in Marshall's reflections and testimony on the subject, but it does reveal a difference in approach to theory between the master and pupil. Specifically, Marshall was not inclined to introduce mathematical formulation into economic theory, whereas Pigou was more willing to do so, but by complementing, in the case under consideration, the equation with textual discussion that canvasses the circumstances when the price–quantity relationship typically associated with the quantity theory will fail to hold (McLure 2013b).

Pigou then set about building upon the framework developed in *Wealth and Welfare* to produce *The Economics of Welfare* (Pigou 1920), which was published in four editions and became an integral part of the Cambridge tradition in economics. Pigou's considerations on the variability of the national dividend were published as a new book *Industrial Fluctuations* (Pigou 1927a) which, amongst other things, stressed the correlation between pessimistic judgement and the phase of the business cycle. His discussion of public finance would be reproduced in *The Political Economy of the War* (Pigou 1921) and later extended into *A Study in Public Finance* (Pigou 1928a).

In the 1920s, Pigou also contributed substantively to two related debates, mainly between colleagues at Cambridge University, which were published in the *Economic Journal*.⁸ The first may be called the 'empty boxes' controversy, which commenced with John Clapham's famous article 'Of Empty Economic Boxes' (Clapham 1922) in which it is contended that Marshall's treatment of the concept of increasing and decreasing returns and Pigou's treatment of the same in *The Economics of Welfare* were empty theoretical constructs because

⁸These episodes are discussed in some detail by Aslanbeigui (1996).

they were devoid of any clear linkage with real-world phenomena. In that debate, Pigou rejected Clapham's point, suggesting that these ideas are not boxes per se, but part of a larger system of economic thought that guides practical enquiry. However, Dennis Robertson, Pigou's Cambridge colleague, was not satisfied with Pigou's defence (Boianovsky 2014), as he concluded that Pigou had overestimated the extent of efficient output by treating fixed costs as if they could be broken down into variable costs. This criticism is reminiscent of Allyn Young's (1913) suggestion that Pigou's industry-wide cost analysis in *Wealth and Welfare* was flawed because rents that firms realise from increasing returns are incorrectly attributed to the marginal net product of resource inputs.

The second debate may be called the 'costs' controversy, which was sparked by the publication of Piero Sraffa's landmark paper on 'The Laws of Returns under Competitive Conditions' (Sraffa 1926). It pointed out that Marshall's treatment of increasing and decreasing costs were inconsistent with competition because it ignored interdependencies between the economic output of firms and industries that impacts on the costs of all firms. Pigou took Sraffa's assessment very seriously by reflecting on when partial analysis, which sets aside general interdependencies, can provide a reasonable approximation (in the case of internal economies) and when it cannot be set aside (in the case of external economies). His mature solution to the issue, provided in 'An Analysis of Supply' (Pigou 1928b), was to replace Marshall's notion of a representative firm for an industry, which recognised that firms are distributed around the representative firm with different costs structure (as a result of the different age and evolutionary state of each firm within an industry), with the idea of an 'equilibrium firm' given under competitive conditions, which, as Newman has accurately observed, 'is Marshall's representative firm with the representativeness with respect to size left out' (Newman 1960: 591). Using comparative statics, Pigou argued that when industry output is in an initial equilibrium there is a tendency for individual firms to expand and contract, with no significant net effect on size because expanding firms offset contracting firms. Similarly, when there is a change in the conditions of demand that leads the industry to adjust to a new equilibrium, new output and supply price will also have adjusted in the same way, with any tendency for individual firms to expand and contract again offsetting each other. In that way, the size of the firm is effectively set aside in equilibrium theory. All that is required to permit increasing, constant or decreasing returns that are internal to the firm is that one firm, which need not be representative in terms of size, is representative in the sense that it, like the industry, is in an equilibrium state. However,

this abstraction was not welcomed by all, with Robertson suggesting that ‘Pigou’s equilibrium firm is, it seems to me, even more “ghostly”...than Marshall’s representative firm’ (Robertson 1960: 601).⁹

In the 1930s, Pigou’s enduring interest in the problem of unemployment came to the fore with *The Theory of Unemployment* (Pigou 1933a), which Keynes argued seemed ‘to get out of the Classical Theory all that can be got out of it’ (Keynes 1936 [1973]: 260). In this work Pigou developed his analysis of unemployment by modelling a sequence of real interdependencies in the microeconomy associated with the demand for the provision of raw materials, the short-run demand for wage goods, and the aggregate demand for labour. Pigou then extended his analysis to consider the interdependence between real and monetary influences on labour markets. The central analytical mechanism for dealing with these interdependencies throughout the book was the notion of the elasticity of demand for labour. *The Theory of Unemployment* was drafted during the Great Depression, with one of Pigou’s goals being to establish the relationship between the nominal and real elasticities of demand for labour in the macroeconomy. His analysis, which incorporated broad statistical observations and intuitive reasoning, led him to provisionally conclude that the elasticity of demand for labour with regard to the money wage would be ‘not less numerically than -1.5 ’, from which he concluded that a 10% cut in money wages would, *other things being equal*, result in more than a 10% expansion in the aggregate volume of labour demanded (Pigou 1933a: 106). Pigou, however, emphasised the importance of *other things being equal* and cautioned that other influences may tend to deepen depressed conditions and that, as a result, the ‘expansive effect of the reduction would be partly or wholly masked’ (ibid.). In addition to questions related to the elasticities of demand for labour, Pigou also examined shifts in the demand for labour in *The Theory of Unemployment*.

The consequent theoretical controversies with Keynes during the 1930s led to Pigou’s continued analysis of the issues raised by Keynes in *The General Theory*.¹⁰ Pigou’s immediate response was to produce ‘Real and Money Wage Rates in Relation to Unemployment’ in 1937, a controversial article which Keynes viewed as ‘the work of a sick man’ (Keynes quoted in Collard 2011: 27), a comment due in part to Pigou’s ill health at the time, but also because of a disagreement with Pigou’s assumptions concerning the monetary system

⁹ Modern historians of Marshallian economics, such as Neil Hart (2013), are even more critical of Pigou, suggesting that his work on costs and the equilibrium firm closed off the evolutionary dimension of Marshall’s work to the economics profession, with the result being that important evolutionary themes in Marshall were not to be developed further.

¹⁰ The definitive ‘rational’ treatment of this historical issue was undertaken by Ambrosi (2003).

and the flexibility of real wages. However, Pigou continued to develop his analysis concerning employment in the wake of *The General Theory*, culminating in *Employment and Equilibrium: A Theoretical Discussion* (Pigou 1941)—which has been beautifully characterised by David Collard as a ‘late flowering’ (Collard 2013: 14), perhaps because Pigou had developed a small macro-economic model that, among other things, included an employment multiplier which he regarded as probably positive (a nod to Keynes) and a money wage multiplier which he regarded as probably negative (a nod to his earlier *The Theory of Unemployment*).

The article ‘The Classical Stationary State’ (Pigou 1943), from which emerged Pigou’s real balance effect (now popularly known as the ‘Pigou effect’), followed, as did *Lapses from Full Employment* (Pigou 1945), and, five years later, the pamphlet Keynes’ ‘General Theory’: *A Retrospective View* (Pigou 1950), in which Pigou accepted certain elements of Keynes’s work but, as Collard (1981:127) has noted, he also maintained several reservations as well. Finally, it should be noted that Pigou continued to work long after his retirement at the age of 65. The work produced after this time included studies on income (*Income: An Introduction to Economics* (Pigou 1946) and *Income Revisited* (Pigou 1955)), British economic history (*Aspects of British Economic History 1918–1925* (Pigou 1947)) and money (*The Veil of Money* (Pigou 1949)).

4 Leadership

From 1908 to 1943, Pigou was Professor of Political Economy at Cambridge. During that time he worked in a particularly hard and focused way. From early in his Professorship, Pigou was generally referred to as the ‘Prof’, especially by the few who were privileged to enter into his private life. One such person, Donald W. Corrie, who read and commented on the manuscript of *Wealth and Welfare* (Pigou 1912), recalled ‘a rapidity with which he [Pigou] could relax after serious work and plunge with boyish enjoyment into any sort of hair brained spree, and at [illegible] notice assume the gravity of the Professor of Economics in discussion with Layton, Fay or Keynes’ (Corrie to Saltmarsh, 19 February 1960, King’s College Archive Centre, Cambridge University: ACP1/Corrie).

However, to the general student body, the recollection of Pigou is perhaps slightly different from that of Corrie. Austin Robinson (1968), an undergraduate in Pigou’s classes, recalls Pigou’s eloquence and clarity of exposition, but also notes a degree of reserve: ‘We admired Pigou; after a lecture we would

sometimes shyly ask him a question, and he would answer, either jocularly or even more shyly. But most of us as undergraduates hardly knew him outside a lecture room'. Perhaps the distance between the shy Pigou and his students was accentuated by his unusual mode of dress. According to Howarth (1978: 139), it was a moot point as to whether J.J. Thomson (the Nobel Prize-winning inventor of the mass spectrometer and discoverer of the electron from Trinity College) or Pigou (who would wear unusual combinations such as white gym shoes with black laces) was the worst dressed man in Cambridge. Indeed, Pigou's dress sense during the first year of his Professorship was such a serious concern to Marshall that he wrote to Charles Fay on the matter: 'Fay, I do wish you'd speak to Pigou on a personal matter—a rather delicate matter. I saw him coming out of Bowes' shop in a Norfolk jacket with holes in both elbows. So bad for the Economics Tripos!' (Marshall quoted in Saltmarsh and Wilkinson 1960: 18).

In regard to the dissemination of his own work, Pigou attempted to develop or refine his contributions without generally attending conferences. Rather, he would read published materials and respond formally, either via articles or noting corrections to errors made in subsequent editions of his books. He remained essentially a theoretical economist, developing his line of thought with little collaboration, although he did seek assistance in mathematical and other analysis (Austin Robinson 1968). For example, Pigou sought out the help of Keynes in the revising of *Wealth and Welfare*, and called upon the strong mathematical abilities of the young Frank Ramsey during the late 1920s (Duarte 2009). Ramsey provided mathematical assistance in relation to two propositions Pigou was developing, one being on credit and the other on unemployment, both of which were subsequently published as articles in the *Economic Journal* (Pigou 1926, 1927b). Ramsey also assisted Pigou with revisions and modifications that would appear in the third edition of *The Economics of Welfare* and in the development of Pigou's treatise on public finance (Pigou 1928a). It has been argued by Collard (1996: 588, 2011: 245) and Duarte (2009: 461) that Ramsey's two major economic articles, 'A Contribution to the Theory of Taxation' (Ramsey 1927) and 'A Mathematical Theory of Saving' (Ramsey 1928) directly arose from considerations resulting from his collaboration with Pigou during this time. This is an argument substantiated by correspondence from Pigou to Keynes: 'Ramsey is writing out a paper on some results he got in the course of doing sums for me—with a marvelously simple generalised formula about taxes. Don't let him be too modest to produce it for the Journal' (Pigou to Keynes, before March 1927, quoted in Bridel and Ingrao 2005: 160).

During the late 1930s, Pigou would also seek the mathematical assistance of David Champernowne, who was appointed Lecturer in Statistics in 1938.¹¹ His method of supporting colleagues and potential colleagues also tended to be private in character, relying on deep reflection on text, and quite private discussion and correspondence. His mentoring and support for Joan Robinson is perhaps the best example of this, although it is not the only one.¹² Aslanbeigui and Oakes (2009) outline the considerable support Joan Robinson received from Pigou when she was working on *The Economics of Imperfect Competition* (Joan Robinson 1933), with Pigou even providing algebraic input for Robinson's treatment of some issues. This work by Pigou was subsequently published in the *Economic Journal* as a note in the 1933 March issue (Pigou 1933b), although in the same note Pigou paid generous tribute to Robinson for her original insights. In the summer of 1939, Pigou wrote a letter taking the opportunity to advise her on her teaching. In regard to mentorship, the letter reveals that Pigou was not willing to constrain original scholarship or the dissemination of new ideas but, in the interests of recognising the depth and scope of past writers in the history of economics, he urged caution against extreme polemics other than in formative debate and discussion. With regard to his own writing, a key point is that he typically did not seek to emphasise differences between scholars.

At King's College, Pigou also made numerous contributions to the Elector's Committee for Fellowships, with his judgement on the intellectual talents of applicants held in very high regard. As Saltmarsh and Wilkinson (1960: 11) have highlighted: 'His colleagues came to listen for his verdict, as foxhounds listen for the tongue of the oldest and sagest hound in the pack'. Notwithstanding Pigou's worsening health problems as his period of tenure went on, his 'sense of justice' was recalled as an outstanding feature in handling the problems of running the Faculty during his Professorship. As Austin Robinson (1968) recalls, 'If you were working with him, you had to satisfy him that what you proposed was a completely just solution of the problem at hand'. For example, notwithstanding his reputation for misogyny (Aslanbeigui 1997), Pigou was still regarded as a supporter of the women's

¹¹ Pigou freely advised others to do the same. As Harcourt (2012) notes: 'Paul Samuelson gave Pigou one of his articles—it may have been factor price equalisation in the 1940s or 1950s and Pigou asked had the maths been checked. Samuelson said he did maths. Pigou said, "No, I mean by a Cambridge mathematician"'.

¹² Pigou also provided considerable support to Richard Kahn, particularly in the consideration and endorsement of Kahn's Fellowship dissertation, providing advice to Kahn to publish the work without delay, advice Kahn did not in the end act upon (see Aslanbeigui and Oakes 2010 and Harcourt 1991).

cause at Cambridge in the immediate post-First World War period (Howarth 1978: 36; Wilkinson 1980: 151).

However, the ‘Prof’ did not stamp his authority on the future direction of the economics programme at Cambridge in that he never attempted to shape the discipline in his own image, and he did not like formal meetings. The manner in which he worked—his quiet isolation and his innate shyness—did not lend itself to the qualities of leadership that Keynes would later wield especially among the younger generation of Cambridge economists who had collaborated with him during the development of *The General Theory*. Nevertheless, it was under the leadership of Pigou that a good number of prominent Cambridge economists were trained, including many of the younger Cambridge economists who gravitated into Keynes’s camp.

None of this means that Pigou was indifferent to Cambridge appointments. Rather, he was more concerned with the intellectual qualities of the appointee, and on that score he had more confidence in the judgement of Keynes than with appointing protégées who followed in his exact footsteps. Indeed, in 1935, he invited John Hicks to apply for the post of Lecturer at Cambridge. Hamouda (1993: 290) has speculated that Pigou’s job offer to Hicks was motivated by a desire to balance the authority of Keynes and his younger followers, on the one hand, with Pigou, Robertson and Hicks on the other. But another interpretation is also evident that is simpler, less Machiavellian and more consistent with Pigou’s personality. That is, notwithstanding the divide between the ‘old’ and the ‘new’ welfare economics, with Pigou and Hicks on different sides of that divide, Pigou simply recognised Hicks’s great intellectual qualities and he sought to employ him on that basis. Interestingly, Hicks appeared to like Pigou and was happy to engage him in discussion on issues in economics, and he found a subtle way of achieving that end: ‘The thing to do is never to press him [Pigou], or argue with him; just throw out a remark to see if it tempts him’ (Hicks to Webb, 12 November 1935, quoted in Marcuzzo and Sanfilippo 2008: 86).

It is perhaps true that, from the mid-1930s, Pigou was regarded as somewhat ‘old school’, with the intellectual distance between him and the younger economists, including Kahn, James Meade and the Robinsons, growing over time. But for a largely singular scholar like Pigou, that distance may well have been more incidental than deliberate, and appeared more deliberate and cultivated than it actually was. Moreover, the distance was implicitly reciprocated by the younger economists, who were intent on extending economics and taking it in directions inspired by Keynes.¹³

¹³The relationship between Pigou and his younger colleagues is discussed in Naldi (2005).

Pigou's method of leadership is also revealed in his actions to smooth over the tensions which had developed at Cambridge during the 1930s. Dennis Robertson, who succeeded Pigou to the Professorship of Political Economy in 1944, had, unlike Pigou, continued to harbour some bitterness towards Keynes after the 1937 debates, leading to estrangement between the two scholars as the younger economists were gravitating towards Keynes's circle. In a study of the correspondence between Pigou and Keynes, Bridel and Ingrao (2005) outline how the deterioration of Keynes's and Robertson's working relations came to a head with the formation of a research project investigating depression and recovery in Britain. Pigou is shown to have been sensitive to Robertson's resentment at being excluded from the project and attempted to diffuse the situation by suggesting the formation of an advisory committee which would include Robertson, Keynes and himself. The members of the committee that proposed the original project, which had included Austin Robinson, Sraffa and Champenowne, bitterly fought Robertson's inclusion on that advisory committee. Pigou's attempt to smooth things over failed, with Robertson resigning from his position at Cambridge and taking an appointment at the LSE. However, the episode demonstrates Pigou's sensitivity towards his colleagues. The sensitivity shown in this case was moreover not an isolated incident. For example, in his correspondence with Keynes during the Second World War, Pigou expresses general concerns for Keynes's health and worries over Sraffa's possible internment as an enemy, war having been declared between Britain and Italy. Pigou wrote to Keynes advising him that he had suggested to Sraffa to write to Keynes's mother to ask her 'to keep a friendly eye on his [Sraffa's] mother when he gets pinched' (Pigou to Keynes, 12 June 1940, quoted in Bridel and Ingrao 2005: 159).

5 Public Service

After the First World War, Pigou still performed some public service, although perhaps not as much as he would have if his health had not deteriorated in the middle of his professorial tenure. Thus, he served on the Cunliffe Committee on Foreign Exchange (1918–1919) and the Royal Commission for Income Tax (1919–1920). He also sat on the Chamberlain-Bradbury Committee on the Currency and the Bank of England Note Issue (1924–1925), the final report of which recommended the urgent restoration of the gold standard at the old parity of exchange. In 1930, Pigou joined the Committee of Economists, chaired by Keynes (other members of the Committee included Lionel Robbins, Sir Josiah Stamp and Hubert Henderson) to review current

economic conditions in Great Britain and the causes of these conditions, and to advise on how to achieve recovery. However, at Cambridge, Pigou avoided committee work, although a major exception to this was his 40-year-long commitment to the Fellowship Electors.

6 Conclusion

A.C. Pigou found an avenue to address his intellectual concerns for the general interest of people, especially working people, by drawing upon his philosophical reflections of what was ‘good’ for the individual and society and utilising the analytical tools of economic theorising developed by Marshall in order to provide the means by which society might benefit from the ‘fruits’ of economic knowledge. The discipline of economics provided a means to develop measures that would ameliorate emerging and ongoing social problems. In Pigou’s (1920: 5) words: ‘[I]t is...the social enthusiasm which revolts from the sordidness of mean streets and the joylessness of withered lives, that is the beginning of economic science’. He tended to eschew dramatic rhetorical forms that culminated in polemics, preferring to focus on the detail of what is fundamentally important and work through those issues carefully and patiently. To that end, he noted in the Preface to *The Economics of Welfare* (ibid.: xix): ‘The complicated analyses which economists endeavour to carry through are not mere gymnastic. They are instruments for the bettering of human life’. The end result is that Pigou produced a body of economic work that, in the best tradition of Cambridge economics, informed policy deliberations that have indeed contributed to the betterment of human life.

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21

Ralph George Hawtrey (1879–1975)

Patrick Deutscher

1 Introduction

Ralph Hawtrey published his first work in economics, *Good and Bad Trade: An Enquiry into the Causes of Trade Fluctuations*, in 1913, and his last, *Incomes and Money*, in 1967. His contributions to monetary economics and to the analysis of economic fluctuations made him an early leading light of what was to become macroeconomics, particularly in the interwar period up to the publication of Keynes's *General Theory*.

After graduating from Cambridge and preparing for civil service entry examinations, Hawtrey entered the service and worked throughout most of his career in the British Treasury. He spent one academic year in Cambridge, Massachusetts, as a visiting professor at Harvard's economics department. He retired from the Treasury in 1947 after 45 years in the public service to become Price Professor of International Economics at the Royal Institute of International Affairs. He continued to contribute to discussions in economics and economic policy through the subsequent decades into the 1970s.

It may be asked whether Hawtrey should be considered a Cambridge economist. He studied mathematics, rather than economics, at Cambridge. He graduated before the Economics Tripos was established but, unlike Keynes, another mathematics graduate, he had not taken any economics courses before

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being tutored by the economic historian, John Clapham, for his civil service exams. Moreover, he disavowed any influence from Marshall or the Marshallian tradition more broadly on his own analysis. His analysis went back to Bagheot rather than Marshall and drew on the practical wisdom of the City.

Arthur Pigou reviewed *Good and Bad Trade* in 1913. He discerned an approach to the analysis of money and its workings that was consistent with Marshall's analysis, an analysis which was, however, limited to an oral tradition. Pigou applauded Hawtrey's analysis of the value of money, writing that 'The way of stating the matter which Mr. Hawtrey has adopted is, of course, familiar to pupils of Dr. Marshall; but it has not, so far as I am aware, hitherto found its way into ordinary books' (Pigou 1913: 580). However, Hawtrey's conclusion that he had uncovered the source of the cycle in monetary dynamics was, according to Pigou, mistaken. The chain of cause and effect that Hawtrey skilfully traced in a deductive exercise from a monetary shock might look like a business cycle, but it was incorrect to conclude that the source of fluctuations had been discovered in those monetary shocks. As Pigou further noted, 'Such a point of view is exceedingly superficial' (ibid.: 582).

Whether or not Hawtrey can properly be considered a Cambridge economist, he was certainly a Cambridge scholar. He was admitted to the Apostles in 1900 and reportedly attended every annual dinner until prevented by illness in 1954. He was devoted to G.E. Moore, corresponded with Bertrand Russell on the foundations of mathematics and maintained a lifelong interest in ethics and moral philosophy. His 1946 Presidential Address to the Royal Economic Society concerned *The Need for Faith*. Economists needed justified confidence and that required 'right ends'. Economists needed to proceed from a 'common ground' that 'must in ultimate analysis be a common standard of what is good or bad. Only what is good or bad is important, and important in proportion as it is either good or bad, whether as end or as means' (Hawtrey 1946a).¹

His membership of the Apostles may well have aided Hawtrey's entry into the Treasury. Lubenow (1998: 163) writes how 'Apostle recruited Apostle into the Treasury'. Robert Trevelyan writing to his father (both Apostles) compared Hawtrey with Theodore Llewelyn Davies, another Apostle in the

¹ *The Economic Problem* published in 1926 differed from most of Hawtrey's other writings and displayed his deep interest in applying moral philosophy to economics. Aspects of his criticism of the abstractions of economic theory and the assumption of rational economic behaviour anticipate in some respect the rise of behavioural economics. The discussions of the distribution of income and of the 'false ends' that can lead to harmful spending decisions show that Hawtrey's economics would clash with the positive economics of Lionel Robbins.

Treasury: ‘Hawtrey is a man quite of Theodore’s type of mind, with less of his power of influencing and directing others no doubt, but with the same fine common sense and intellectual power, and the same good political tradition—being, like Theodore, a Home Ruler as well as a liberal’ (Trevelyan quoted in *ibid.*: 165).

Hawtrey’s first civil service post in 1903 was with the Admiralty. In 1904, the Treasury was allotted an additional position, and after a round of promotions of incumbents, Hawtrey was brought in from the Admiralty as a Second Class Clerk. While working as an acting First Class Clerk in 1910, he served as Principal Private Secretary to the Chancellor of the Exchequer, Lloyd George. In 1911, ‘he became an established First Class Clerk’ (Black 1977: 367). However, he was seen as a poor administrator, and after his last promotion in 1919, he served as the Director of Financial Enquiries, essentially a one-man operation. In that role he provided advice on economic and financial policy and the implications of global developments for the British economy and public finances. In 1936, Warren Fisher, the Permanent Secretary of the Treasury, told parliament’s Public Accounts Committee that Hawtrey ‘works away on metaphysics and writes learned books’ (Grigg 1948: 81–82), words that would send a chill down the spine of a twenty-first-century civil servant!

Hawtrey’s position in the civil service was not a barrier to writing and publishing an impressive number of books and articles, often on the very topics on which he was providing advice to the government of the day. There is little evidence that his superiors interfered with or gave direction to his work, a forbearance that might not have been forthcoming had his views been at odds with the culture and policy of the British Treasury. He also was able to carry out extensive correspondence with other leading economists on questions of economic theory and policy.

2 Hawtrey’s Economics

Macroeconomic cycles were the focus of Hawtrey’s analysis throughout his career: what caused them and how to prevent them or how to mitigate their consequences. One of the key conclusions was that ‘the trade cycle is a purely monetary phenomenon’ (Hawtrey 1923: 141). *Good and Bad Trade* dealt with the trade cycle and advanced a perspective that, while it evolved and was elaborated and applied to historical and contemporary issues, remained at the core of Hawtrey’s subsequent work. In 1937, he told Keynes,

I have adhered consistently to my fundamental ideas since 1913 and in so far as they have developed and grown the process has been continuous since then ... I do not think this conservatism is a merit; indeed I should rather like to go in for something novel and extravagant if I could be convinced of it (Hawtrey quoted in Keynes 1973a: 55).

Looking back from the early 1960s, Hawtrey reported that he had started working on *Good and Bad Trade* in March 1909. He had become interested in the trade cycle and cited official reports and his own observations as the genesis of his thinking:

It struck me that the extinction of profit could be explained if the price level fell in the interval between the incurring of costs and the sale of the product. And if traders borrowed at interest to finance profit-making business, they could pay a higher rate of interest when prices were rising and a lower rate when prices were falling. A falling price level makes a given market rate of interest more onerous, and a rising price level less so. Here, I thought, was a discovery, but I was disillusioned when I learnt from an economist friend that the principle was one already recognized, and had been expounded in Irving Fisher's work (Hawtrey 1962a: vii).

The 'economist friend' was, in all likelihood, Keynes.

The most complete development of Hawtrey's model can be found in *Currency and Credit*, which went through four editions and was widely used as a text in money and banking courses in the 1920s and 1930s.

An understanding of the dynamics of an economy required starting from a model of a monetary economy with flows of income and output based upon credit and expressed in monetary terms. The first chapter of *Currency and Credit* began with consideration of an economy based on 'credit without money' (Hawtrey 1919 [1928]: 1–16). Transactions of all sorts result in the creation of debts and offsetting credits for the buyers and the sellers. Institutions such as banks can arise to provide mechanisms to clear debts. A unit of account must arise so that different sorts of transactions and the debts to which they give rise can be set off against each other. That is one of the services that money can provide.

The demand for a stock of the means of settling debts depends primarily on the level of incomes. Each person 'would keep some unexhausted credit in hand to meet the payments, foreseen or unforeseen, which will have to be made in the near future' (ibid.: 7). In Hawtrey's unique terminology, these balances were sometimes labelled the 'unspent margin' because they increased or decreased depending on the difference between *consumers' income* and *consumers' outlay*.

Consumers' income and outlay were the categories that Hawtrey developed to analyse the circular flow of income, expenditure, and production:

An increase or decrease in people's (nominal) incomes will lead to an increase or decrease in their (nominal) expenditures ... Only true income and final expenditure are to be counted; expenditure on the production or purchase of goods for subsequent sale is to be excluded. The income is to be what a man has available to spend on his own needs; the expenditure is to be what he so spends. They may conveniently be called the 'consumers' income' and the 'consumers' outlay,' though it must be understood that 'consumer' includes 'investor,' for investment is one of the purposes on which income may be spent.

The receipts and disbursements of the trader, who buys or produces with a view to sale, may be called the 'trader's turnover' ... The trader...gets his true income out of the profits of his business, and this figures in the total of the consumers' income (ibid.: 45–46).

Economic activity in Hawtrey's analysis pivots around *dealers*. Dealers hold stocks of goods that they order from primary producers, manufacturers, or other dealers. They sell from these stocks to retailers or other dealers. In Hawtrey's model, dealers possess a uniquely strong ability to judge the state of markets and make the decisions that, in the aggregate, speed or slow the pace of economic activity.

Dealers are also particularly sensitive to changes in short-term interest rates. A mutually beneficial relationship arises between dealers and bankers. Hawtrey characterized bankers as 'dealers in debt' (ibid.: 4). Inventories are excellent collateral for loans from banks. As a result, dealers are able to finance their inventories with short-term loans on comparatively favourable terms, that is, in comparison to financing with their own capital or with longer-term borrowing.

For many of Hawtrey's contemporaries, it was fixed capital formation which played the central role in the trade cycle. Cycles in capital goods-producing industries were observed to be especially pronounced. Hawtrey acknowledged this and even in *Good and Bad Trade* developed an accelerator mechanism to account for it. He started from an assumption of an industry growing at a steady pace of 1% a year and replacing 5% of its capital stock each year. Gross investment is equal to 6% of the initial stock in the steady state. If the capital-using industry's growth rate increased from 1% to 4%, then the output of the capital-producing industry would need to rise by 50%, from 6% to 9% of the initial stock. This demonstrated that the greater cyclicality of capital-producing industries could be a consequence rather than a cause of fluctuations in consumers' income and outlay. Hawtrey acknowledged that

exogenous shocks to fixed investment could be the source of macro fluctuations, but he argued, first, that these could not account for the regularity of trade cycles before the First World War and, second, that these shocks could still be countered by monetary policy, operating primarily through short-term interest rates.

A central bank is able to determine short-term interest rates in the economy because of its power to lend funds or buy assets to an unlimited extent. To raise interest rates generally the central bank raises the terms on which it makes loans available. Usually the change in Bank rate alone would be sufficient to affect the demand for loans and economic conditions. If more were needed, Bank rate could be reinforced by buying or selling other financial instruments. By selling government bonds or other debt instruments, the central bank lowers their prices and raises their yield while drawing liquidity from the market. To lower interest rates it reverses the process, offering to lend at a lower rate than previously and buying bonds and other debt instruments, raising their price and lowering the yield while adding liquidity. The decisions made by the central bank depend on the monetary regime in place and on policy goals.

In the 1930s, Hawtrey developed the concepts of *widening* and *deepening* for the analysis of fixed investment. Widening is investment that takes the form of expansion of a business with no change in the amount of capital per unit of output. It is motivated by opportunities for profit. Deepening is investment that increases the amount of capital per unit of output. The long-term rate of interest would determine how far to push investment in capital per unit of output. However, Hawtrey expected the relationship to be loose because the response of fixed investment would be constrained by uncertainty about future demand and factor prices and by discontinuities in the marginal yield of capital.

The central bank's direct power is over short-term nominal interest rates. From the outset Hawtrey distinguished between nominal and real interest rates. In *Good and Bad Trade*, he defined the profit rate of interest as the natural rate of interest plus an allowance for changes in the price level: 'Competition always tends to make the rate of interest prevailing in the market equal to the actual yield expected from the use of money in business' (Hawtrey 1913: 45–46). The long end of the term structure is less amenable to central bank power and its impact on fixed investment decisions, to which many of Hawtrey's prominent contemporaries gave primary importance, was uncertain and highly variable in comparison to the responsiveness of dealers' demand for stocks of goods to changes in the short-term rate at which they could borrow.

Under a gold standard, with a country's money legally convertible to gold at a fixed rate, the maintenance of a gold reserve adequate to meet any conceivable demand was the primary driver of central bank policy. Throughout much of Hawtrey's career the tribulations related to gold—the piecemeal return to gold after the First World War, maintaining the gold standard under conditions of high unemployment after the return, and, abandoning gold during the Great Depression—were dominant issues.

In Hawtrey's analysis, the monetary economy does not settle into a stable equilibrium or growth path. One of his key themes was 'the inherent instability of credit' (Hawtrey 1932: 166–174). Left to itself, credit would tend to expand without limit once an inflationary path was in motion. Similarly, there was no built-in limit to deflation. Nor is adjustment to lower prices or a path of falling prices smooth. Prices, including wages, are lowered only under sustained pressure from falling sales, reduced orders and production, and rising unemployment. A gold standard provides an anchor, though a very imperfect anchor, because bankers respond to the loss of reserves that comes about from an overly expansionary policy by raising lending rates and to an increase of reserves by lowering rates and otherwise making credit more readily available. Because the adjustments take time this tended to create a credit cycle. The cycle was fairly predictable when it was not punctuated by major events such as the discovery of new mines or periods of war when combatants abandoned gold.

Gold itself offered only a flawed foundation for money because the 'wealth value' of gold itself varied. For example, growing monetary demand for gold that outpaced increased supply could lead to a tendency to deflation on the gold-using world. This was precisely the situation expected following the First World War.

Hawtrey had great confidence in the ability of monetary authorities to mitigate the instability of credit through skilful and timely adjustment of Bank rate. However, in practice central bankers were apt to make mistakes and did not themselves either recognize or acknowledge the power that he ascribed to them. Failure to move promptly to counter a deflationary tendency could lead to 'trade deadlock' (Hawtrey 1933: 29), a situation in which interest rate cuts would fail to stimulate the new borrowing that would turn the situation around. Under these circumstances, other tools to increase the flow of income and outlay, including debt-financed public works, were called for.

Hawtrey is identified with the *Treasury view*, the position that public spending simply displaces private spending and would have no impact on aggregate economic activity. He developed this case in the most complete

way in his 1925 paper 'Public Expenditure and the Demand for Labour'. Public spending was only effective if it was a vehicle for credit expansion and, considered that way, it was an inefficient means to the end of increasing employment and economic activity: 'The public works are merely a piece of ritual, convenient to people who want to be able to say that they are doing something, but otherwise irrelevant' (Hawtrey 1925: 44).

He had argued against public works in *Good and Bad Trade*. There he wrote in response to the 1909 Minority Report of the Poor Law Commission proposal that a portion of public expenditures on capital formation be triggered when the level of unemployment breached a target level. The funds required would be deficit-financed and Hawtrey's view was that 'the writers of the Minority Report appear to have overlooked the fact that the Government by the very fact of borrowing for this expenditure is withdrawing from the investment market savings which would otherwise be applied to the creation of capital' (Hawtrey 1913: 260). In the Foreword that he wrote in 1961 to a reprint of the book he acknowledged that this 'failed to take account of the possibility of the expenditure having a deliberately inflationary character' (Hawtrey 1962a: ix).

It seems likely that Hawtrey's scepticism of public works emerged in part at least from the native hostility of a Treasury official to spending public money and adding to public debt when the net benefits of the spending are doubtful. In later years, he was happy to take the credit or the blame for authorship of the *Treasury View* (Deutscher 1990a).

A significant part of Hawtrey's theoretical and practical work was concerned with foreign exchange markets. Markets for foreign exchange, like all markets in Hawtrey's analysis, are operated by dealers. The economy itself is divided between sheltered and unsheltered industries, sometimes using the terminology of home-trade products and foreign-trade products. Sheltered industries produce only for the home market and are protected by natural barriers from foreign competition. Unsheltered industries compete in global markets whether their products are bought domestically or are exported. Their products are effectively priced in world markets. Hawtrey in various writings described the mechanisms leading towards equilibrium between the external and internal price level and the distribution of resources between sheltered and unsheltered industries.²

² See, for example, *Trade and Credit*, pages 23–24, or *Bretton Woods for Better or Worse*, pages 86–88, or Chapter 5 of *Currency and Credit*.

3 Hawtrey and Some Economic Policy Issues

Hawtrey's appointment as Director of Financial Enquiries in 1919 came as double-digit wartime and post-war inflation continued. The effective suspension of the gold standard and the issuance of Treasury notes to finance the war had contributed to a steep rise in the price level. The Interim Report of the Cunliffe Committee, delivered in August 1918, recommended the restoration of pre-war arrangements. These included a return to an effective gold standard, an end to government borrowing, the use of the Bank of England's discount rate tool to control the demand for credit and limits on note issue. The final report in 1920 largely reiterated these recommendations. Hawtrey approved of the Committee's recommendations. He did not challenge the gold standard as the basis for money and exchange markets and his analysis led him to support measures to stabilize prices.

Stopping inflation required an end to inflationary deficit finance. The government in mid-1919 was borrowing from the Bank of England at a three month rate (3.5%) lower than the Bank's discount rate (5%). To make Bank rate 'effective' the government had to accept a higher rate. That move was made in October, but it was only the first step and further increases were required to stop inflation and the fall in the value of sterling in terms of gold. Hawtrey supported higher rates but believed that only a short period of dear money would be needed to break inflationary expectations after which rates could be brought down quickly. Bank rate was raised to 7% in April 1920 and over the next year inflation was reversed. Reviewing the results, Hawtrey saw confirmation of his analysis of credit policy (Howson 1975: 23–24). A similar deflationary policy was being pursued in the USA and the combined effect was simultaneously to exacerbate global deflation and prevent recovery in sterling's exchange rate. Hawtrey, with a perhaps exaggerated belief in the financial power of London, advocated various mechanisms to send gold to the USA to trigger interest rate cuts there. He also began to strongly criticize the Bank of England for being too slow to cut interest rates as unemployment rose and deflation took hold in Britain.

Hawtrey supported the return to an international gold standard though he recognized it as a flawed standard, subject to variation in the 'wealth value' of gold. He also pointed to the danger of a sharp increase in the demand for gold from many countries seeking concurrently to return to the standard. To economize on gold he proposed a gold exchange standard and some form of international cooperation, primarily involving America and Britain.

Hawtrey was the main author of proposals for international monetary cooperation that were adopted at the Genoa Conference of 1922. The proposals aimed at facilitating a return to the gold standard while avoiding a deflationary rush to accumulate gold. Countries were to aim for price stability and to eliminate internal circulation of gold as domestic currency. Reserves for most countries were to be holdings of the monies of financial centres who would maintain the stock of gold reserves. Fluctuations in the value of gold, which Hawtrey diagnosed as the key cause of the pre-war trade cycle, would be mitigated with central bank management. The Conference approved the proposal and directed central banks to meet and develop the means to implement them, but the central banks did not follow through. Hawtrey frequently lamented the failure to act on the Genoa resolutions as the source of subsequent global economic weakness.

Hawtrey subsequently accepted the British goal of returning sterling to the pre-war parity in terms of gold (or, equivalently, the US dollar) on the grounds that it would make the standard more credible. One of his key concerns was sustaining London's position as a financial centre. Stable exchanges, which at the time implied a gold standard, would strengthen London's role in financing international trade. However, this was not a goal to be achieved at any price. In fact, he expected the pound to move to parity and, in his memorandum on the gold standard for Churchill, wrote that 'no active measures need be taken' (Moggridge 1972: 72–73). Hawtrey expected rising prices in the USA because of the abundance of reserves there, reducing the amount of sacrifice required of Britain. He also held that if the economic burden became too high, then the parity should be abandoned, a perspective that might seem at odds with his approval of returning to the pre-war parity because it enhanced credibility.

Following the return to gold in 1925 British unemployment persisted at a high level and Bank rate remained, in Hawtrey's view, too high. He criticized the Bank of England for keeping Bank rate high and for holding on to gold reserves that they ought to have let go, a mechanism which would have promoted credit expansion in other countries as well. Looking back in *A Century of Bank Rate* published in 1938 he wrote: 'It is, I think, not unreasonable to hold that a policy of cheap money and credit relaxation from the beginning would have had a favourable effect on economic activity throughout the world in 1925, and would have made the task of retaining the Bank of England's gold quite easy' (Hawtrey 1938: 141).

The October 1929 crisis in US financial markets came for Britain at a time of high unemployment. The financial turmoil culminated in Britain abandoning the gold standard in September 1931. Bank rate was raised to 6%

to defend sterling and prevent an unrestrained fall in the exchange rate. It remained at 6% for five months and was then cut to 2% between February and June 1932. Hawtrey approved of the speed of the cut. With the advantage of hindsight, however, he concluded that ‘it was too late; cheap money by itself was insufficient to start revival. The vicious circle of deflation, broken for a moment by the suspension of the gold standard, had been joined again’ (ibid.: 145).

Based on the change in prices subsequent to the 1925 return to gold, Hawtrey argued for a depreciation of sterling of 30% and using Bank rate subsequently to stabilize it at that level. That would restore rough equilibrium between British costs and world prices. He warned against going further, the danger being an upward pressure on wages (Peden 2000: 255–260).

When economic activity did not recovery fully in response to low Bank rate, Hawtrey developed an analysis of a trade or credit deadlock: ‘[I]f the depression is very severe, enterprise will be killed. It is possible that no rate of interest, however low, will tempt dealers to buy goods’ (Hawtrey 1933: 29). An economy would not remain stuck below full employment naturally but only if the central banks made severe mistakes. Even if bringing interest rates to the lowest possible level proved ineffective, Hawtrey held that monetary policy would eventually be made effective if continued asset purchases by central banks increased the money supply sufficiently. This was a policy that could be pursued without limit.

Hawtrey was critical of the reconstruction of the international monetary order enshrined in the Bretton Woods agreement. He was sceptical about the fixed exchange rate regime, with adjustments allowed only with the approval of the International Monetary Fund (IMF). He also viewed it as imparting an inflationary bias in monetary policy and for failing to enshrine any commitment to monetary cooperation to stabilize the price level (Hawtrey 1946b).³

In 1949, Britain used the IMF mechanism to devalue the pound from \$4.03 to \$2.80, a level that Hawtrey saw as far too low. Despite subsequent inflation of both prices and costs, the exchange rate remained too low, leading to overemployment and upward pressure on prices. With monetary policy restricted to defending the exchange rate, the value of money was left to erode

³ Hawtrey voiced his concerns while the Bretton Woods negotiations were underway. Keynes, who led the British team, had some sympathy but in the context of the day, Hawtrey’s concerns would impede moving forward. Lionel Robbins, who was also part of the British delegation, later wrote: ‘I remember getting up one morning at Bretton Woods to find among my papers a minute by Sir Ralph (then Mr.) Hawtrey which had been passed on to me by Keynes. I forget the exact words ... “What’s all this talk about the dangers of deflation?” said the writer; “Inflation, not deflation, is the postwar problem”. ‘Dear Lionel’, ran Keynes’s covering note, ‘I thought you would like to see what the old thing is saying. After all, he may be right’ (quoted in Howson 2011: 980).

and rather than being a fixed source of stability, the exchange rate was repeatedly tested in financial markets. This was a recurrent theme of Hawtrey's writings in the 1950s and 1960s (see, for example, Hawtrey 1954).

Hawtrey's analysis, throughout his long career, led him to the conclusion that price stability was the appropriate target for monetary policy. In a new Foreword to *A Century of Bank Rate* in 1962 he wrote: '[W]hen we look back on the monetary experience we have had since 1932, surely the moral to be drawn from it is above all the vital importance of maintaining stability of the value of the money unit' (Hawtrey 1962b: xxi).

4 Contemporary Influence

Hawtrey's work was widely read and cited. *Currency and Credit* was a popular textbook on both sides of the Atlantic. He also carried out extensive debates through correspondence with other leading economists. In contrast, spending his career as an advisor in the British Treasury meant that he did not have continuous dialogue with students, the next generation of economists. Determinedly self-taught and insistent on using his own terminology, he was somewhat outside the mainstream. Differences of language or of using the same word (e.g. *investment*) to mean something different, sometimes stood in the way of understanding.

Hawtrey was the fifth most cited 'macroeconomist' during the interwar period, 1920–1939 (based primarily on articles listed in the relevant sections of the *Index of Economic Journals*) (Deutscher 1990b: 188–194). Table 21.1 lists the top ten along with an index of the number of citations.

Table 21.1 Most cited macroeconomists of the interwar period

Rank	Economist	Citation index (Keynes = 100)
1	J.M. Keynes	100
2	D.H. Robertson	52
3	Irving Fisher	36.5
4	Arthur Pigou	36
5	R.G. Hawtrey	33
6	F. von Hayek	29
7	Alfred Marshall	21.5
8	W.C. Mitchell	21
9	G. Cassel	20
10	John Hicks	17.5
10	Roy Harrod	17.5

Hawtrey's place among the leading macroeconomists was sustained throughout the period. He ranked sixth between 1920 and 1930, fourth between 1931 and 1935, and sixth from 1936 to 1939. However, his placing fell away following the Second World War.

Schumpeter attested to Hawtrey's stature in the interwar years: 'Throughout the twenties, Hawtrey's theory enjoyed a considerable vogue. In the United States, especially, it was the outstanding rationalization of the uncritical belief in the unlimited efficacy of the open-market operations of the Federal Reserve System that prevailed then' (Schumpeter 1954: 1,121). The receptive hearing that Hawtrey received from US economists is shown by his connection to the work of Allan A. Young. Young was an important figure more through his personal influence as a teacher in the Harvard economics department than his publications. Early on he commented on the originality and value of Hawtrey's work and he used *Currency and Credit* as the textbook for his courses in money and banking in the 1920s. He took a temporary appointment at the London School of Economics (LSE) from 1927 to 1929 and arranged that Hawtrey replace him at Harvard in 1928–1929. Young passed away in 1929 at the age of 52 before completing his LSE appointment.⁴ Laidler has made the case for a chain of influence from Hawtrey through Young and Lauchlin Currie (who as a student had worked with Young and had also attended Hawtrey's Harvard classes), to the Chicago monetary tradition (Blicht 1983; Laidler 1993).

In the 1930s the persistence of the downturn and the ascendance of Keynes's work combined to raise doubts about Hawtrey's analysis, though he remained a key participant in contemporary debates. Hawtrey had a long-lasting friendship and professional interaction with Keynes. Keynes reviewed the first edition of *Currency and Credit* warmly: 'This is one of the most original and profound treatises on the Theory of Money which has appeared for many years' (Keynes 1920). Through the 1920s they found themselves in broad agreement on the need for managed money and on there being some role for a monetary transmission mechanism for economic fluctuations. However, they disagreed over many important points, including the approach to the return to the gold standard, the practical power of Bank rate policy, and the potential role of public spending to boost employment. As a discussant of a 1929 paper presented to the Royal Statistical Society by Hawtrey on 'Money and Index Numbers', Keynes remarked:

⁴Hawtrey was considered as Cannan's successor at LSE and again after Young's death. I do not know whether he actively pursued the post. (See Howson 2011: 142, 163–164.)

There are very few writers on monetary subjects from whom one receives more stimulus and useful suggestion...and I think there are few writers on these subjects with whom I feel in more fundamental sympathy and agreement. The paradox is that in spite of that, I nearly always disagree in detail with what he says! (Keynes 1973b: 127).

Hawtrey and Keynes corresponded at length over *A Treatise on Money* and *The General Theory*. Hawtrey's critique of the 'Fundamental Equations' as tautologies likely nudged Keynes in the direction of *The General Theory*. The dynamics of the *Treatise* operated through relative price adjustments and Hawtrey proposed a model that called for quantity adjustments in advance of price movements. In the *Treatise*, Keynes placed more emphasis on non-monetary causes of fluctuations and Hawtrey teasingly suggested he was advancing 'a non-monetary theory of money', an approach 'in the tradition of the Cambridge school of economists' (Hawtrey 1932: 359). Hawtrey's critique of *The General Theory* was reprised in *Capital and Employment* (Hawtrey 1937).

In the course of his commentary, Hawtrey developed examples that contained at least an embryonic multiplier analysis. The variant that he published in *The Art of Central Banking* proposed an assumption that a change 'in income is divided in a fixed proportion ($k:1-k$) between consumption and investment', observing that in this context investment would usually be termed 'savings'. He went on to show that 'provisional equilibrium' after some initial shock to consumer expenditure, ΔC , was a change in the value of output equal to $\Delta C/(1-k)$ (Hawtrey 1932: 351). This and similar exercises have led a number of authors to applaud Hawtrey as at least one of the discoverers of the multiplier (Davis 1980).

Hawtrey's work also intersected frequently with that of Dennis Robertson, the second most cited macroeconomist of the interwar period. Robertson developed non-monetary (*real*) models of the business cycle, an approach in stark contrast with that of Hawtrey. Robertson found the origins of fluctuations especially in the behaviour of investment: the discontinuous flow of innovations, the time required to carry out capital projects and the durability of capital. Robertson was generally sceptical about the power of interest rate policy to mitigate fluctuations and to the extent that monetary policy could be effective; he argued against price stability as the right target because that could discourage the flow of innovations. Robertson's contributions to economics were wide-ranging, but in his 1963 *Lectures on Economic Principles* he introduced the final section on money and economic fluctuations with the personal observation that 'this has always been to me the most interesting part of economics—the only part to which I can hope to be remembered as

having made any personal contribution' (Robertson 1963: 325). Robertson, even more than Hawtrey, collaborated with Keynes and influenced him. Of his 1926 work *Banking Policy and the Price Level* he wrote that he had had 'so many discussions with Mr. J.M. Keynes' regarding the nature of saving and its role in the economic cycle 'that I think neither of us now knows how much of the ideas therein contained is his and how much is mine' (Robertson 1926: 5).

Robertson's earliest work also dealt with the economic cycle. *A Study of Industrial Fluctuation* published in 1915 was developed from a 1913 essay. He also reviewed Hawtrey's *Good and Bad Trade* for the *Cambridge Review* in 1913. The review took Hawtrey to task for overemphasizing monetary forces and for its lack of empirical evidence: 'Mr. Hawtrey's work is innocent of an appeal to facts' (Robertson 1913: 163). Robertson also dismissed Hawtrey's opposition to public works as a remedy for unemployment, saying that it 'scarcely deserves formal refutation' (Deutscher 1990a: 82–84).

Robertson's work in the 1920s paid more attention to monetary forces and gave a more receptive account of Hawtrey's analysis. Fundamentally, however, Robertson saw the origins of fluctuations in 'real' factors and suggested only a limited role for monetary policy or a policy aimed at price stability.

Hawtrey's courteous debates with other contemporaries were an important part of economic discourse in the 1930s. His 1937 work, *Capital and Employment*, included essays that critically examined the work of Hayek, Pigou, Harrod, and Keynes, along with a gentle debunking of Major Douglas's theory of social credit.

5 Conclusion

Ralph Hawtrey played a leading part in the development of macroeconomic thought and monetary theory, especially in the 1920s and 1930s. He provided the pre-Keynesian world with the textbook foundation of an approach to the analysis of aggregate economic performance and the place of money in the economy.

In crucial respects, Hawtrey developed a model of economic performance that pointed the way for Keynes and subsequent economists. His analysis of the inherent instability of credit and his diagnosis of the need for active stabilization policy was a key contribution and at odds with orthodox economic teaching. His emphasis on price stability as the optimal target for economic policy and his advocacy of the short-term interest rate instrument for monetary policy, backed up if needs be by quantitative easing (or tightening), comes across, despite differences in language and style, as a perspective that would be well understood by policy makers in the twenty-first century.

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Frederick Lavington (1881–1927)

Peter D. Groenewegen

1 Introduction

Frederick Lavington was born in 1881. From 1897 to 1908 he worked in the Capital and Counties Bank, a sign of things to come. In 1908 (the year Marshall retired from his Cambridge Chair), Lavington entered Cambridge as a mature student. In 1909, he obtained a Fellowship from Emmanuel College. As a student in his late twenties, Lavington was considerably older than his fellow students and, by two years, than his lecturer, Maynard Keynes. Charles Fay (1927: 504–505) later recalled that Lavington came up from London with ‘two passions, one for the academic life of discussion and analysis, the other for the pulsating romance of the London money market. His essay on the latter in Part I of the Economics Tripos extracted from us the unusual mark of 90 per cent’. Two problems in Marshallian economics particularly intrigued him: the nature of ‘the particular expenses curve’ and ‘the validity of consumer surplus’. Neither was explained to his satisfaction by Fay until Fay got the definitive answer from Marshall himself.

Lavington was placed in the First Class of Part I of the Economics Tripos in 1910, followed by another First when he completed Part II in 1911.

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Lavington was part of the brilliant student cohort of pre-1914 graduates trained by Keynes and A.C. Pigou. Lavington also won the Adam Smith Prize for his essay, 'The Agencies by which Capital is associated with Business Power'. After a short spell of lecturing at Cambridge on the capital market, Lavington found employment in 1912 in the new Labour Exchanges (Collard 1990: 197). In 1918, Lavington returned to Emmanuel College as a College Lecturer in Economics; in 1920, he obtained the Girdlers' Lectureship in Economics as successor to Keynes, a position held until his death in 1927.

Lavington became a monetary economist concentrating on topics associated with the rate of interest, uncertainty, and speculative activity. On these he published three articles in the *Economic Journal* (Lavington 1911, 1912, 1913). His superb 1912 article 'Uncertainty in its Relation to the Net Rate of Interest' established him as a gifted Cambridge monetary theorist. During the 1920s he published two books: *The English Capital Market* (1921, with a second posthumous edition in 1929) and the far shorter and quite unoriginal *The Trade Cycle* (1922). Both were reviewed in the *Economic Journal* (Lemberger 1921; Cannan 1922). An article on the topic of 'Business Risks' appeared in two parts in the *Economic Journal* (Lavington 1925, 1926a), as did a lengthy book review (Lavington 1923). He published three articles in *Economica* (Lavington 1924, 1926b, 1927), the last two devoted to topics in the theory of the firm.

These publications established his reputation as a sound Marshallian economist and made him part of the Cambridge Monetary School (then led by Keynes, Pigou, and Dennis Robertson) which contributed to money and banking, economic fluctuations, and the business cycle. Lavington was never greatly involved with Keynes. A single letter (from 1911) between the two economists survives, while Keynes's appointment books mention Lavington only once (Moggridge 1992: 433–434). Lavington's initial lectures were devoted to topics in finance and the capital market; from 1920, he also lectured on the structure and organization of industry. Both his publications and lectures reveal Lavington as a very orthodox Marshallian economist, a staunch believer in the literal inspiration of Marshall's *Principles of Economics*, and credited with the saying, 'It's all in Marshall' (Wright 1927: 504).

The 1920s were a period for Lavington of ill health, combined with a tendency to overwork. Apart from his teaching and research, Lavington played an active role in college life and in wider university activities. Fay (1927: 505) mentioned he was 'an excellent supervisor', and both he and Wright (1927) argued that Lavington was far more devoted to Marshall and his econom-

ics than members of the Faculty of Economics and Politics generally were. With this background, Lavington's various economic contributions can now be chronologically examined.

2 The 1911, 1912, and 1913 *Economic Journal* Articles on Banking, Interest, and Speculation

Following his completion of Part II of the Economics Tripos, Lavington published three articles in the *Economic Journal* in 1911, 1912, and 1913. These laid part of the theoretical foundations for his 1921 study on the English capital market and developed from his grasp of English and German monetary literature which had been part of the reading for his economic studies. They may also have reflected his preliminary banking experience with the Capital and Counties Bank. These articles in turn dealt with 'The Social Importance of Banking' (Lavington 1911), 'Uncertainty and Its Relation to the Rate of Interest' (Lavington 1912), and 'The Social Interest in Speculation on the Stock Exchange' (Lavington 1913). The 1912 paper earned him a considerable reputation as a new monetary economist, and was quite influential. His 1912 and 1913 articles were cited by Frank Knight (1921: 199, fn. 1) as two of the half dozen contributions to the economic literature on uncertainty which by then had appeared in Great Britain and the USA.

Lavington's 1911 article (only eight pages) assessed the social importance of the banking system through its facilitation of capital movements, its provision of currency, and the other objectives which greatly contributed to the community's social well-being. It explains how banks as part of the system of financial intermediation carried out these functions under the then still existing gold standard. Banks have the power to create money through their ability to offer current (chequing) accounts whose use by bank customers influences the size of the money supply (ibid.: 54–55). The 1911 article also discussed whether banks were operating under free competition or whether they exerted some monopoly power given their above-normal profitability (ibid.: 55–56). Lavington dismissed the view that banking profits constituted 'a social evil'—after all, they were distributed to a substantial number of shareholders in the form of dividends. Moreover, with bank boards fully elected by shareholders, the self-interest of a competitive banking system closely resembled the public interest (ibid.: 57). Lavington (ibid.) claimed that free competition was preserved in modern banking, despite the growth in size of trading banks and the establishment

of branch banking. Growth in size also entailed some economic and social advantages. Some of these arose as the benefits of economies of scale; scale enhanced the security that banks could offer to deposit holders, while size would not diminish the local knowledge of bank managers. More generally, developments in modern banking gave money supply greater elasticity to meet changes in demand from business and the public over the course of the business cycle. Such flexibility could also yield harmful consequences through changes in the price level. A brief discussion of the operation of the money multiplier of the gold reserve in the creation of new bank deposits indicated this (*ibid.*: 58–59). Bank rate variations were useful for controlling such changes in the quantity of money, while combined action by the banking system on discount rates regulated currency supply effectively (*ibid.*: 59–60). Lavington (*ibid.*: 60) concluded with the statement that the self-interest of competing banks enhanced the material welfare of society which recent (but unmentioned) changes to the banking system had also encouraged.

Lavington (1912) addressed the problem of uncertainty in relation to the rate of interest and, more generally, issues of financial intermediation. Lavington (*ibid.*: 398) started this investigation by stressing that the future can only be imperfectly foreseen and that ‘the cost of imperfect foresight is a continuous maladjustment of resources’. Imperfect knowledge about the future also lowered productivity and enhanced the social costs of production, while its consequences raised the level of risk in rational business decision making, hence raising the necessary ‘compensating probability of exceptional gain’ (*ibid.*). A diagram of the ‘curve of prospective returns’ illustrated how any demand for resources under conditions of uncertainty required an additional return. Alternatively, Lavington (*ibid.*: 399) argued that ‘[u]ncertainty is a disutility for which a payment must be made in addition to the net rate of interest’. Following J.B. Clark, Lavington (*ibid.*: 400) described uncertainty as a form of ignorance in business decision making, as a consequence of financial insecurity, since investment which locked in resources thereby increasing their immobility, greatly added to uncertainty. However, uncertainty could be lowered for a firm by the cost of creating a financial reserve for it (*ibid.*: 401). More generally, uncertainty could be reduced through insurance and through issuing low-risk securities (such as government bills), an organizational characteristic (*ibid.*: 403–405). Lavington’s conclusions expressed overall results on uncertainty and the rate of interest. Costs of insecurity are maximized for capital invested ‘in an irredeemable security with a narrow mar-

ket' (ibid.: 406) which thereby needed a higher return. Moreover, similar bank securities attracted similar rates of return, while imperfections in financial markets could be eliminated by superior organization. In short, the presence of appropriate financial institutions lowered uncertainty, and enabled increased closer investment of capital which raised productivity and national income levels (ibid.: 406, 407, 409).

Lavington (1913) discussed the social value of speculation within a setting of the need to develop specialized financial institutions to take advantage of the efficiency gains from this increased division of labour. Appropriate speculative machinery, he argued, was an essential feature of the circulation of goods in an organized market. This also applied to capital market transactions bringing saving and investment decisions into balance despite the different motivations of savers and investors. Saving provided uncommitted productive resources by the act of foregoing their present enjoyment, while investment was command over capital resources and their employment enabled by the act of saving. Marshall's *Principles of Economics* had firmly established these simple propositions of saving–investment analysis. In modern economies, the process of placing saved resources in the hands of productive investors is the task of the capital market, comprising the banking system, the stock exchange, and trade credit. Lavington (ibid.: 36–37) described the securities created in their transactions as an efficient manner of risk-spreading, a role in which speculation on security prices played an active part. Referring to his 1912 paper on uncertainty and rates of return, and the need for financial intermediation in this, the stock exchange could be taken to make the securities it traded more marketable, thereby lowering their insecurity. Speculator activity tended to equalize the rates of return on what could be called 'equivalent investments', thereby enhancing the degree of competitiveness in financial markets, always a good outcome (ibid.: 45–49). Lavington concluded that competitive financial markets enabled prices to move closer to investment values, lowering cost of production by the more productive use of resources whose supply price was lowered by this type of financial intermediation. Speculation, in short, was useful for raising human welfare, a proposition previously made by Marshall.

Lavington (1911, 1912, 1913) gave his preliminary position on the nature, the advantages, and the modus operandi of the financial system as it existed in the decade before the start of the First World War. Banking here played a crucial role as the major creator and effective regulator of currency supply, best carried out by large, competitive banks earning profits. Lowering uncertainty was also assisted by a good financial system, by enabling firms

to create the necessary financial reserves as a safeguard for preserving their security. The 1913 article reviewed the positive role of speculation in stimulating the saving–investment mechanism, thereby raising productivity through increased capital intensity in production and lowering production costs. Lavington’s (1921) study of the capital market in England revisited this territory and explained it in considerably more detail.

3 The 1921 Book on *The English Capital Market*

According to Wright (1927: 504), Lavington’s work on the capital market was designed to fill in the details of ‘one corner of Marshall’s broad picture’ not effectively completed by him. The book drew considerably on material in Lavington’s three *Economic Journal* articles published before the war. It also commented on the subject of risk, a topic Lavington addressed in a two-part (1925, 1926a) *Economic Journal* article discussed later in this section. A note on sources acknowledged Lavington’s (1921 [1929]: 283) general indebtedness to Marshall’s and Pigou’s economic work. Looking at the references cited in Lavington’s book, none are dated after 1920. The 1929 printing contains only the Preface of the original 1921 edition, suggesting that it probably was a reprint corrected only for small errors. As Lavington had died two years before the issue of the 1929 reprint, this is not surprising.

The structure of *The English Capital Market* is as follows. The book is divided into four, very uneven, parts, with respect to size. Part I presents ‘Leading Ideas’ by way of a short introductory chapter; a discussion of the market for money, capital, and trade credit; concluding with a broad overview chapter, presenting a general view of the market. This presents the capital market as part of the specialization under a division of labour practised in a modern economy, designed to efficiently provide business with capital. Part II presents an outline of the theory of money in its six short chapters. Chapter V introduces the quantity theory of money as a supply and demand theory of the value of money. The demand for money is explained in Chapter VI, while Chapters VII–IX analyse the supply factors. Chapter X presents an overview. Part III of the book is devoted to ‘The Transport of Capital’. Its eight chapters analyse ‘The Flow of Resources into Investment’ (Chapter XI); ‘The Influence...of the Market on Waiting’ (Chapter XII); and on risk (Chapter XIII) including that from ‘Imperfect Knowledge’ (Chapter XIV); and the immobility of resources invested (Chapter XV). Chapter XVI looks at

the marketing of securities, and Chapter XVII at the influence of the market on the demand for capital before closing with a review of marketing operations (Chapter XVIII). Part IV, well over half the size of the book, devotes its twenty-five chapters to the institutions which make up the English capital market. Nine chapters (Chapters XIX–XXVII) discuss the banking system and its role in the provision of currency as an essential feature of the market which assists the circulation of capital in a modern economy. Fifteen chapters (Chapters XXVIII–XLII) examine the stock exchange as a market for long-term securities. It includes the role of the speculator in that market, both in general and in terms of the specific operations of the London Stock Exchange. Chapter XLIII examines the role of trade credit as another aspect of the capital market. The final chapter (Chapter XLIV) presents conclusions.

Some general features of the book are highlighted first. Presentation of its argument is within the setting of the pre-1914 gold standard, with bullion stocks the major reserve underpinning the note issue. Secondly, the book is internationally comparative, referring to German, French, and the US, as well as English, practice. Despite the original year of publication (1921), as much of the factual material is pre-war if not end nineteenth century. These data are largely derived from *The Economist*, from official inquiries into the monetary system and from reference works equally divided between English and German language titles, and, to a lesser extent, French and North American sources. Lavington's broad acknowledgement of work by Marshall and Pigou has already been mentioned.

Part II of *The English Capital Market* contains an outline of the theory of money, that is, a discussion of the theory of value of money based on supply and demand. The algebraic version of Lavington's theory is the Fisherine form of the quantity theory:

$$P = T/MV \quad (22.1)$$

where P is the price level, T the volume of transactions, M the quantity of money, and V the average velocity of circulation. Lavington substitutes nr for the MV of the Fisherine form, where n is the practical equivalent of M ('number of units of money') and r that of V ('the average velocity of its circulation'). Lavington explicitly depicted the theory as one of supply and demand, since his argument on the previous page identified P with D/S , where relevant demand factors explaining the value of money are indicated by T , the volume of transactions, while the relevant supply factors are the effective quantity of money, MV (Lavington 1921 [1929]: 23–24).

The next chapter explains the demand for money in terms of the distinction between ‘the yield of convenience’ in holding money and ‘the security’ associated with this action. Moreover, money’s ‘marketability’ (Lavington’s term for what would now be called ‘liquidity’) or general acceptability is part of its return for the holders of money, and explains why money is so attractive an asset to hold (ibid.: 30–31). There is no specific consideration of what became known as the Cambridge Cash Balance equation where the demand for holding money was directly related to the level of income of the holder and inversely to the rate of interest. Lavington (ibid.: 36–37) simply explained the supply of money by the instruments capable of acting as money (notes, coin, bank deposits in cheque accounts, bills of exchange drawn on secure borrowers, and, as was the case before the First World War, gold reserves and stocks of gold coin).

Lavington (ibid.: 65–66) then examined the role of the capital market from the perspective of Marshallian saving–investment analysis. Lavington (ibid.: 75–79) identified the term ‘waiting’ with both saving (postponement of, or waiting for, consumption) and investing (waiting for the output of production). Much space was devoted to the risk involved in these activities. Risk is explained by imperfect knowledge and uncertainty, a discussion in which Lavington (ibid.: 87–89) referred to Pigou’s treatment in *Wealth and Welfare* and to his own analysis in Lavington (1912). Moreover, risk is increased for certain investment by its degree of immobility. Marketability is therefore a valuable quality of an investment since it lowers risk for individual investors, and is an important service which capital market institutions render to investors through reducing risk. The supply price of capital is then explained (Lavington 1921 [1929]: 101) as comprising net interest, the price of risk bearing, and other costs of moving capital not specified by Lavington. How to reduce these costs is a specific task Lavington ascribed to the capital market, the analysis of which occupied much of the remainder of the book. This contains a detailed examination of the banking system, the stock exchange, and trade credit. On the basis of this analysis, the capital market can be depicted as an important additional agent of production, within the set which Marshall had called ‘organisation’.

Given their close association with risk analysis, Lavington’s (1925, 1926a) papers in the *Economic Journal* can be usefully reviewed here. Lavington (1925: 186–187) introduced risk as an indication that there can be no ‘perfect knowledge’ in the world as is, so that ‘complete calculation’ is impossible in business decision making. Undertakings of future events are by definition uncertain and are filled with risk or the ‘*unrelieved probability of loss*’ (ibid.: 187; italics in original).

Once again following J.B. Clark (in an unspecified source), Lavington (*ibid.*: 194) identified three costs associated with uncertainty, namely ‘(1) [E]xpenditure of the organizing capacity by which these losses [of uncertainty] are reduced; (2) [R]eduction in the real incomes of the producing parties as a result of the imperfect use of their productive resources; and (3) [R]eduction in the efficiency of individual incomes as a result of their uncertainty’. Lavington (*ibid.*) then examined payment for the bearing of risk and uncertainty using analytical apparatus developed by Pigou. This allowed him to argue that the growth of the firm tended to lower the costs attributable to uncertainty and risk; that stock prices set in the market reflected the costs of risk; and that risk reduced efficiency in using productive resources while uncertainty lowered the efficiency with which income can be used (*ibid.*: 195–198). Insurance cannot lower risk, only uncertainty for the insured, while costs attributable to risk can be said to fall on both producers and consumers alike (*ibid.*: 199).

In the second part of his paper, Lavington (1926a) appraised the following aspects of risk and uncertainty. Certainty of supply whenever that can be achieved considerably enhances consumers’ surplus. Lavington then argued that while losses arise from the incalculability of uncertainty, the additional expenses from specific risks are quite estimable. Moreover, much of the entrepreneurial task arises from problems associated with risk. Business risks are then identified with the possibility of war, insecurity of property, variable standards of value, and unstable tariff policies (*ibid.*: 201). Lavington illustrated this argument with a wheat/potato example, where fluctuations in output tend not to be offset (or neutralized) by opposite price changes. This phenomenon tends to make farmers’ incomes unstable, or as stable as if the prices of these commodities had been stable at their average level (*ibid.*: 202–203).

4 The 1922 Study of *The Trade Cycle*

The contents of Lavington’s book on the trade cycle are now examined. Its subject matter is covered in nine short chapters, treating both the social and economic aspects of the cycle. Lavington’s (1922: 7) Preface acknowledges that:

Most of the leading ideas [for the book] have been drawn from the writings of Dr. Marshall, Professor Pigou, Mr. D.H. Robertson, and from the great work on *Business Cycles* by Professor W.C. Mitchell of California. The facts and the subsidiary argument have come from a wider range of sources, including the writings of Professor Mitchell and M. Aftalion, to whom inadequate acknowledgement is made in the text.

Lavington (*ibid.*) added indebtedness to Hubert Henderson and 'other Cambridge friends' for valuable criticism and suggestions, while his use of the records of the Board of Trade, Lavington's former employer, was also acknowledged.

The introduction to the book also recognized that 1921 was a crisis year with much 'unwilling unemployment' and 'ample resources vainly [seeking] employment' (*ibid.*: 9). Price uncertainties, preventing reliable business forecasts, high protection costs limiting the ability of industry to successfully compete in foreign markets, and growing distrust in the labour market hampering appropriate wage settlements, are all parts of the crisis picture (*ibid.*: 10). The cycle, Lavington added, is a recurring phenomenon, as indicated by a century's history of prosperity and depression, the major causes of which needed further investigation (*ibid.*: 11–12).

Chapter II evaluates the nature of the business cycle. It suggests growth occurs in waves, whose rhythmical character demands an explanation. From peak to peak, the average cycle is estimated to last eight years. It is marked by three stages: rising activity, the peak (followed by the crisis and panic), and then declining activity. Wholesale prices follow a similar pattern, as does the rate of change in capital goods production (*ibid.*: 14–16). The chapter ends with the claim that cycles are the leading cause of unemployment in modern industrial society, and with a rejection of the Jevonian sunspot theory as an implausible explanation for the cycle.

Chapter III elaborates on the contemporary business situation which generates cycles. Entrepreneurs make business decisions either in private firms, partnerships, or joint-stock companies engaged in manufacturing, in wholesale, or in the retail trade (*ibid.*: 18–19). Modern production takes time, resources have to be invested to meet future demands, hence expectations about the future have to be formulated. Given this pattern, investment goods production tends to fluctuate widely, but considerable interdependence among all types of producers nevertheless exists. Incomes generate production, indicate the prevalent volume of demand, thus in turn influencing output decisions. The pattern of the income–output cycle is also reflected in the price level, introducing monetary factors to the analysis of the cycle. Hence, the state of business confidence is a key factor.

Chapters IV and V examine business confidence in some detail. Lavington (*ibid.*: 29–30) suggests that business estimates of future market conditions are influenced by waves of optimism and pessimism. These have little influence in a planned and regulated economy. In a capitalist economy, however, they tend to exaggerate business reactions in both the upswing and the downswing, partly through their impact on purchasing power and money supply, which follow activity levels. As Lavington (*ibid.*: 36–37) put it at the end of Chapter IV:

When markets are rapidly expanding, and prices rising, the most inefficient entrepreneurs find business easy and profitable; rationally based confidence gives way to optimism—judgements are infected by a general error; many businesses are extravagantly managed, many ventures are undertaken with no reasonable prospects of success; and causes are set in motion whose effects, in the form of realised business error, destroy the confidence from which they arise and bring the period of prosperity to an end.

Chapter V considers in more detail the role that monetary factors play in the trade cycle by investigating price influences on business confidence. Increasing confidence during an upswing is reflected in rising market expectations, rising output, and eventually rising prices. Initially, output expansion tends to lower prices, but its impact also falls on money supply as bank deposits are more actively used as business improves. Monetary growth gradually outstrips output growth, putting pressure on prices (*ibid.*: 47–48). Rising prices benefit entrepreneurial incomes, lowering incomes for landlords, capitalists, and especially for labourers. Rising entrepreneurial incomes gradually induce errors in business forecasts from excessive optimism and bring the upswing eventually to an end. The effect of price rises on the availability of purchasing power assists this process.

Chapters VI, VII, and VIII present a discussion of the course of the business cycle. This starts with the downward phase of ‘dull business’. Gradually,

the minds of business men turn towards the future and its possibilities, armed with the knowledge that no depression is ever permanent. Confidence builds slowly, assisted by other promising features of the economic situation: lower wages, reduction in stocks to very low levels, rising bank reserves and low interest rates, while the fall in wholesale prices comes to an end. The sentiment takes hold that the end of the depression is near, making business confidence rise further. Repair work, generating growth in purchasing power and renewed consumer spending, reappears. The cycle goes into its upswing phase, marked by growth in capital investment, an expansion of employment and of credit, a great increase in business commitments based on a precarious extension of confidence and inducing a drain on bank reserves and ultimately an encroachment of costs on the margin of business profits (*ibid.*: 65).

These conditions are sufficient for Lavington to generate a downturn and declining trade, just as in the upswing, the necessary monetary expansion is ultimately limited while the rapid growth in investment outstrips the requisite savings levels pushing yields in a downward direction and putting upward pressure on the structure of interest rates. Expected yields are not realized

as the boom progresses, eventually lowering the demand for new capital, lowering the rate of investment, and then the general level of demand for output. The fall in demand makes a depression inevitable (ibid.: 70–73, 78–79). The Lavington cycle rests on monetary factors including price levels, as well as real factors inherent in saving–investment analysis and the psychological factors of optimism and pessimism in business decision making.

In his final chapter, Lavington addresses the social consequences of the trade cycle. These are placed in a background deriving much from both free trade and free entry, in which real wages are growing considerably and where income distribution favours the middle classes and, to a lesser extent, the working class. This is not to say that business cycles have no harmful effects over the longer run. Their impact on unemployment, recognized by Lavington in his analysis, was considerable, as shown clearly in William Beveridge's *Unemployment: A Problem of Industry* (1909). But, as Marshall (1890 [1920]: 687–688) had shown, medieval artisans had experienced 'inconsistent employment' so that the problem was an old one, not necessarily confined exclusively to contemporary developments (Lavington 1922: 94, 96–99, 101). Lavington also argued that social reorganization cannot really provide a cure. The banking system, however, had done much to mitigate the extent of the cycle, and in particular had reduced the incidence of panics. Moreover, the policy of public works, particularly that of spacing the impact of public works to fall into depressed periods, was spreading as a remedy for cyclical unemployment (ibid.: 109–110). Greater flexibility of wages and prices could assist the speedier revival from periods of depression and unemployment: '[C]hecking the excessive growth of business confidence' by greater control over price changes, would likewise limit 'the extravagance of the boom and the intensity of the following period of depression' (ibid.: 113). Identifying remedies for the adverse consequences of the business cycle, in Lavington's view, was closely related to grasping the fundamental causes of the cycle.

As Lavington himself came close to admitting, there was little originality in his treatment of the business cycle. Its essentials were very similar to the treatment of the subject by Alfred and Mary Marshall's *Economics of Industry* (1879: Book III, Chapters 1 and 2). As mentioned previously, it also considerably resembled Pigou's treatment of cyclical fluctuations in Part VI of *The Economics of Welfare*, while the publication of Robertson's *A Study of Industrial Fluctuation* in 1915 had greatly assisted Lavington, particularly because of its underlying saving–investment analysis. Lavington's main contribution was his emphasis on financial markets and the uncertainty faced by businessmen when making financial and investment decisions. Furthermore, *The Trade Cycle* was

a neat little volume, clearly setting out the role of business sentiments and fluctuating confidence levels, so popular in the 1920s. It was by no means Lavington's major economic work, but an interesting contribution to the growing trade cycle literature nevertheless.

5 Lavington's Three *Economica* (1924, 1926b, and 1927) Papers

Although not all of these articles are devoted to monetary topics, two of them in fact addressing aspects of the theory of the firm, they can still be dealt with in this portrait of Lavington as a Cambridge monetary economist. The first of them, Lavington (1924), discussed the interconnection between short and long interest rates in the money market. After stating the general principle that interest rates tend to be higher, the longer the duration of a loan (*ibid.*: 292–293), Lavington (*ibid.*: 295–297) indicated that security markets transform long borrowings into short, highly marketable loan instruments. He provided data on three month bill rates and the yield on consuls, suggesting that these rates generally move together, but not invariably so. However, the data on short and long rates showed no normal relationship in which long rate changes are invariably those appropriate to short-term rates (*ibid.*: 298–299). Examination of the data suggested the following conclusions. Organized markets facilitate relationships between long-term and short-term interest rates, though the financial implications of the (1914–1918) war seem to have destroyed the conformity of movements in these rates revealed by pre-war averages. However, the profitability of business, greatly influenced by movements in the general price level by its influence on the demand for capital, seems to have a stronger impact on rate changes in the market (*ibid.*: 202–203).

Lavington (1926b) discussed the role of monopoly in preserving business stability, in particular the output strategies visible in monopolistic behaviour as compared with that of competitive firms (*ibid.*: 136). In general, this monopoly action is seen as less beneficial to the consumer as the alternative, less stable output policy of the competitive firm. However, Lavington (*ibid.*: 147) suggested in concluding his argument that there may be a substantial benefit to the public from this stable output strategy if it increased continuous employment of appliances and labour. Also, it probably raised supply of the commodity in question, as compared with the outcome under competitive conditions. Lavington's first excursion in print into an aspect of the theory

of the firm did also preserve significant links to his discussion of the evils of unemployment raised in the context of the social consequences of the trade cycle, as discussed in the previous section of this chapter.

Lavington's third and final article in *Economica* was devoted to a topic completely within the theory of the firm. More specifically, it broached topics which Marshall had covered in his 1919 *Industry and Trade*. Under the title, 'Technical Influences and Vertical Integration', the paper examined the proposition that the more restricted the variety of processes undertaken by a single firm, the more simple is the management of direction (Lavington 1927: 27). Marshall described this higher specialization as 'uniform continuous process', combined with more 'powerful appliances of production', constituting the 'dominant technical economies of modern methods of production' (ibid.: 28). Individualism in production was sacrificed here for the sake of standardization, with only high-quality goods (in the motor vehicles and in the woollen textiles industries) preserving individuality in production (ibid.: 34). Lavington concluded his discussion as follows (ibid.: 35–36). Form and size of business were influenced by love of power and the desire for monopoly, not just by economic efficiency. However, given the complexity and volume of tasks involved in a single business, such aspects of the specialized production process had consequences for the efficiency and organization of the managerial task. Following Marshall's position in *Industry and Trade*, Lavington urged his readers that the dominant technical economies are those attainable from the application of modern continuous process and powerful appliances, but that the pressure to attain this situation required vertical integration and lateral disassociation of processes. The range of appropriate processes is small, but the promise of large output is considerable. This modifies pressure for vertical integration in modern business, as shown in the modern steel industry with its strong requirement for 'balanced plant'.

The three *Economica* articles published by Lavington can be seen as a switch in research interests in line with his teaching responsibilities at Cambridge. The second and third were both devoted to topics in the theory of the firm even if the first of these stressed the highly desirable 'macro-economic' objective of stability in the employment of both labour and capital. Such 'macro-economic' considerations are totally absent from Lavington's 1927 article, devoted to vertical integration and the benefits of economies of scale from the application of 'modern continuous process'. Lavington's death in 1927 meant that this new research endeavour could not be further pursued. Hence, Lavington's explorations of industrial economics ended with his 1927 article. One can only speculate as to what other findings they may have led.

6 Conclusion

As a mature age student with employment experience in the banking sector before entering university, Lavington took both Part I and Part II of the then still relatively new Economics Tripos, gaining First Class Honours on both occasions. Lavington entered Cambridge too late to have had Marshall as his teacher. He was, however, taught by two of Marshall's most eminent students, Pigou and Keynes. His intellectual contact with Pigou, his 'professor', was by far the greater of these two teachers. There is little direct evidence of much contact with Keynes after Lavington had completed his economic studies, and many commentators (Bridel 1987; Eshag 1963; Patinkin 1965, 1976) treat Lavington as a strict Marshallian and Pigouvian and therefore part of the Cambridge old guard in so far as Keynes was concerned. This perspective is not completely accurate. Lavington's *Economic Journal* contributions, which Keynes would definitely have seen as one of its editors, discussed themes which were to become of importance for some of Keynes's own monetary research. This is discussed later. It is emphasized here that Lavington's early death in 1927, at just 46-years-old, facilitated a treatment of him in this way, when the commentators just mentioned were chronicling the road from Marshall (and Pigou) to Keynes's *General Theory*.

Furthermore, these same commentators effectively treat Lavington as the author of one book, even if they formally acknowledge that he published a second book, *The Trade Cycle*. In other words, Lavington's importance, historically contemplated, rests on his study of *The English Capital Market*, published in 1921, posthumously reprinted in 1929. References to his eight journal articles occur most infrequently. Nor is it generally recognized (Collard 1990 is an exception) that Lavington seems to have started in the years before his death to switch his teaching and research interests to questions of industrial organization. This is apparent in his last two *Economica* articles and, to a lesser but still significant extent, in the 1912 *Economic Journal* article. Lavington related this interest specifically to Marshall's work, as an explanation of 'organisation', the fourth agent of production, which Marshall had added to land, labour, and capital. The commentary literature, however, fully recognizes that Lavington was a staunch, if not dogmatic Marshallian, completely devoted to the system of the 'master'. He claimed 'that it is all in Marshall', even if his direct contact with him had been very limited.

This devotion to Marshall by Lavington the monetary economist is peculiar when it is recalled that Marshall himself published little on monetary economics during his lifetime. There was his (by then) still unpublished evidence

to official inquiries including that for the 1887 Gold and Silver Commission; a few journal articles (by 1925 collected in Pigou's edition of the *Memorials of Alfred Marshall*), and some chapters in the 1879 *Economics of Industry* published with his wife. *Money, Credit, and Commerce*, specifically designed to set out Marshall's position on monetary and trade issues, did not appear until 1923, a decade after Lavington's early *Economic Journal* articles on banking, on uncertainty and on the rate of interest, and on speculation. As mentioned earlier, these articles were noted by Knight (1921) as part of the small but growing literature on risk and uncertainty, crucial features of Lavington's depiction of the money and credit system as it was developing in the third decade of the twentieth century. Lavington's contributions to risk and uncertainty as recognized by Knight are interesting for at least two reasons. First, they show the importance of not ignoring his journal contributions but treating them as supplementary to the depiction of England's capital market given in his 1921 study. Second, these papers show that the analysis of risk and uncertainty in financial transactions was part of the research agenda on both sides of the Atlantic in the early decades of the twentieth century. In particular, it should be stressed that the investigation of uncertainty and risk was very much in the air at Cambridge, in Lavington's case with special reference to the rate of interest and the price of liquidity.

In this context, Lavington's Marshallian credentials may be briefly restated. In the first instance, they reveal that Lavington can be said to have designed his work as additions to material not fully treated by Marshall himself and within the basic Marshallian framework of analysis. That analytical framework consisted of both the *Principles of Economics* and *Industry and Trade*, but, with reference to money and cycles, also to some chapters in the earlier *Economics of Industry*, some specific articles by Marshall and his evidence to official inquiries. This comes out clearly in the obituaries of Lavington by Fay (1927) and Wright (1927), and in the text of Lavington's books and articles. Specific parts of Marshall's work to which Lavington explicitly adhered include his acceptance of 'organisation' as a very important aspect of the theory of production, the general cost analysis, the theory of the firm under competition and monopoly, and the overall saving–investment analysis in relation to the rate of interest. Moreover, Marshall's positions on the growth of the middle classes in contemporary society, medieval unemployment, and the theory of management, were accepted by Lavington as important truths. This does not detract from Lavington's originality in his work on money and financial institutions, uncertainty and risk, much of which cannot be found in Marshall's own writings and, as pointed out, anticipating Keynes's contribution in several respects. Schumpeter (1954: 895, 1,084) appreciated Lavington's originality in these

things because Lavington together with so many of his Cambridge economist colleagues ‘developed Marshallian teaching...on lines of their own’.

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23

John Maynard Keynes (1883–1946)

D.E. Moggridge

1 Introduction

John Maynard Keynes was a product of reforming Victorian Cambridge which admitted non-conformists to University degrees and expanded educational activities for women and pre-university candidates. His father, John Neville Keynes (1852–1949), was the first non-conformist Fellow of Pembroke College. His mother, Florence Ada Brown (1861–1958), daughter of John Brown, pastor of the Bunyan Meeting, Bedford, and biographer of Bunyan, was an early member of Newnham College, where she took a Cambridge Higher Certificate. Neville Keynes was University Lecturer in Moral Science, 1884–1911, and a University civil servant, first in the Local Examinations Syndicate, 1884–1910, and later as Registrar, 1910–1925. In 1914, Florence was the first woman elected to Cambridge town council, a post she held for three decades, and its first woman mayor, 1932–1933.

Maynard was educated at Eton and King's College, Cambridge, where he was equal 12th Wrangler in mathematics in 1905. He spent 1905–1906 preparing for the civil service examination—including a term attending Marshall's lectures, writing papers for him, and reading a formidable amount of economics, which turned out to be his worst subject in the examination—in which he came second overall. The first candidate chose to enter the Treasury, so

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Keynes went into the India Office (October 1906–June 1908). He returned to Cambridge at the invitation of Marshall to lecture in the Economics Tripos, created in 1903. He became a Fellow of King's in March 1909 and Girdlers' Lecturer in Economics in 1910. In 1911, he succeeded Edgeworth as Editor of the *Economic Journal*, a post he held until 1945. Back in Cambridge, he maintained contact with the India Office. His first book *Indian Currency and Finance* (1913) won him membership of the Royal Commission on Indian Currency and Finance of 1913–1914, where he made further contacts that stood him in good stead when he briefly advised the Treasury on emergency monetary policy after the outbreak of war in 1914.

In January 1915, Keynes joined the Treasury full-time. Initially, his activities covered a wide range of subjects, but he soon concentrated on Britain's overseas financial affairs. When 'A' Division was created in 1917 to deal with these, Keynes became its head. In 1919, he was the Principal Treasury Representative at the Paris Peace Conference. He resigned his Treasury position and left Paris in June 1919 over the terms of the Peace Treaty. On his return to England, he embarked on *The Economic Consequences of the Peace*, which appeared in England in December. It made him famous and wealthy.

He gave up his Girdlers' Lectureship in April 1920, when he also became a Supernumerary Fellow of King's. After that, his connections with Cambridge were almost non-pecuniary but still substantial: in November 1919, he became Second Bursar of King's, First Bursar 1924–1946. He continued to give eight University lectures per year dealing with his work in progress until 1937. He continued supervising students for King's. He was elected to the Council of the Senate on the reform ticket in 1920 and played an important role in the transformation of Cambridge as an academic institution after the Report of the Asquith Royal Commission, the resulting Act of Parliament and the subsequent revision of University and College statutes. That job completed he stood down from the Council of the Senate in 1926. For the same reason his father retired as Registrary at the end of 1925. After 1920, Maynard supported himself primarily through his journalism, City directorships, and his own investment income. From Tuesday to Friday, he lived at 46 Gordon Square in London, spent long weekends in King's and vacations in the country, after 1925 at Tilton in Sussex.

Keynes continued as an adviser to governments. He became more regularly involved after the formation of the Economic Advisory Council (EAC) in 1930 and its various committees, notably the Committee on Economic

Information, which met regularly between 1931 and 1939 and became an important influence on government thinking (Howson and Winch 1977). After the outbreak of war in 1939, his influence grew larger as regards both domestic as well as overseas finance, both in the prosecution of the war and planning for the post-war world. He was given a room in the Treasury from August 1940. His position was anomalous. He was a member of the Chancellor of the Exchequer's Consultative Council, a position that did not entitle him to a room in the building nor to lead British delegations overseas. He 'was just "Keynes", free to shoot at anybody—and anybody, regardless of rank, was free to go to him with his troubles' (Proctor 1949: 27). Being 'Just Keynes' had its advantages for he could take these troubles to where they mattered, even to Churchill, and set further enquiries in train, sometimes to be dealt with by himself: 'He was the strangest civil servant Whitehall has ever seen, less the civil servant and more the master of those he served than any I have ever seen' (Salter 1967: 88–89).

'If Maynard Keynes had died in 1925 it would have been difficult for those who knew intimately the power and originality of his mind to have convinced those who did not know him of the full measure of Keynes's ability' (Austin Robinson 1947: 35). Support for this position comes from Patrick Deutscher's (1990: 188–194) tabulations of citations in the fields of money and macroeconomics from the *Index of Economic Journals*. For the years 1920–1930, he ranked tenth with an average of around one citation per year—Pigou ranked third, Marshall fourth, and Hawtrey and Robertson joint sixth. In contrast, for 1931–1935, 1936–1939, and 1940–1944 he ranked first, with Robertson second in the first two periods and fourth in the third, Hayek third in the first, seventh in the second, and Hicks third in the second and second in the third. Further support could be drawn from Keynes's *Tract on Monetary Reform*, albeit a reworking of articles from the *Manchester Guardian Reconstruction Supplements*, which was basically Marshallian with a partial reworking of Cassel's theory of purchasing power parity, a treatment of the inflation tax (which Keynes had first discussed in a Treasury memorandum in 1915) and a consideration of cyclical issues which was 'unoriginal' (Laidler 1999: 112).

In examining the transformation of Keynes's position, I should like to look at the bases for the changes in his influence as he wrote and discussed *A Treatise on Money* (1930) and *The General Theory of Employment, Interest and Money* (1936) and his contributions to discussions of international monetary reform in the 1930s and 1940s.

2 Keynes and Macroeconomics

A Treatise on Money had a long gestation period: its first draft table of contents was dated 14 July 1924. By 2 August 1929, Keynes had received proofs of 320 pages of a single volume version of the *Treatise*. Working to a publication date of 1 October, Keynes began proof corrections and final rewriting. But he became increasingly dissatisfied with what he had done and began to attempt more substantial revisions. By the end of the month, he was writing his American publisher Alfred Harcourt that the revisions were so drastic that he was going to go over to a two-volume version and could not foresee publication before January 1930 (XIII: 117).¹

His public life added complications. On 4 November 1929, Philip Snowden, the Chancellor of the Exchequer, announced the appointment of a Committee on Finance and Industry under Lord Macmillan with Keynes as a member. Four days later, he had lunch with the Prime Minister, Ramsay MacDonald. Two further meetings followed in early December. The result was the appointment of an EAC with Keynes again a member. The Macmillan Committee and the EAC made large demands on Keynes's time: between November 1929 and the dispatch of the final text of the *Treatise* to the printers, his diary records 65 meetings of both bodies. Revised proofs started coming at the end of 1929, but it was April 1930 before a final set was available. Keynes was so busy that he did not have time to read some of the comments his readers made on the proofs: in the case of Ralph Hawtrey, he did not reply to some comments until two months *after* the book was published!

Keynes sent his book for publication on 14 September 1930, when he wrote to his mother:

This evening, at last, I have finished my book. It has occupied me 7 years off and on,—and one parts from it with mixed feelings ... Artistically it is a failure—I have changed my mind too much during the course of it for it to be a proper unity. But I think it contains an abundance of ideas and material (XIII: 176).

Keynes set out much of his *Treatise* analysis in terms of his 'Fundamental Equations' with P being the price level of consumption goods, R their real output, E the community's nominal income, O the output of consumption goods and investment goods, I' the income earned in the investment goods

¹ All citations to *The Collected Writings of John Maynard Keynes* take the form of volume number followed by page number.

industries, I the value of new investment, S nominal earnings, and Π the price level of output as a whole:

$$P = E/O + (I' - S)/R$$

$$\Pi = E/O + (I - S)/O$$

These ‘purely formal’ equations were ‘identities, truisms which tell us nothing in themselves. In this respect they resemble all other versions of the quantity theory of money’ (V: 125). Because saving was defined as ‘the sum of the difference between the money income of individuals and their money expenditure on current consumption’ (ibid.: 113) and investment as ‘the value of the increment of capital during the period’ (ibid.: 114), it followed that ‘the value of current investment...will be equal to the aggregate value of savings and profits’ (ibid.). Keynes’s definition of income was chosen to permit his measures of savings and investment to differ from one another. This was desirable, given that ‘decisions which determine savings and investment respectively are taken by two different sets of people influenced by different sets of motives, each not paying very much attention to the other’ (ibid.: 250).

Hawtrey, for example, pointed out that since the Fundamental Equations were tautologies, Keynes could not use them to suggest that an excess of investment over saving *caused* a rise in prices relative to costs, since he had defined saving and investment in such a way that the existence of such an excess meant that prices had risen (XIII: 132–133). That assumption was also challenged by his younger Cambridge colleagues as the widow’s cruse fallacy: ‘Thus profits, as a source of capital investment for entrepreneurs, are a widow’s cruse which remains undepleted however much of them may be devoted to riotous living’ (V: 125). The point was picked up by Robertson, who acknowledged the contribution of Meade in ‘putting me on the right track here’ (Robertson 1931: 406, fn. 7), and Hayek (1932: 3).

Keynes’s view of the relationship among saving, investment, and interest rates was similar to those of Swedish contemporaries. However, Keynes, unlike the Swedes or Hayek, was aware of the underlying capital theoretic difficulties.

Keynes’s *Treatise* adoption of a saving-investment framework increased the importance of the long-term rate of interest. This was coupled with a diminished emphasis on the effects of changes in short-term rates, which was the result of his increased knowledge of commodity and capital markets where the volume of investment in working capital was relatively insensitive to changes in the short-term rate of interest.

Given his own previous and his contemporaries' emphasis on changes in Bank rate, and the fact it remained the most visible and readily available instrument of monetary policy, Keynes was faced with the problem of explaining how changes in short-term rates could affect the long-term rate. He developed a theory of what is known as the term structure of interest rates, that is, the relationship between interest rates on securities with different terms to maturity, a problem not previously discussed in the literature. This showed much concern for 'conventional' expectational factors which impeded the flexibility of long-term rates.

In the *Treatise*, Keynes shifted his ground from the *Tract* and accepted the desirability of an international standard of value which would be an improvement on the gold standard. The principal disadvantage of any international standard with perpetually fixed exchange rates was the dilemma it could pose for a country whose domestic circumstances called for a monetary policy different from that which adherence to the standard entailed. The dilemma could become acute for a member of such a system with international capital mobility whose efficiency wages (money wages adjusted for productivity) were too high to allow it to achieve full employment at existing international interest rates, although these rates might be appropriate for the system as a whole. The authorities in the problem country could not use monetary policy to increase employment because lower interest rates would induce capital outflows and a loss of reserves which would force the authorities to raise interest rates. In these circumstances, there was a reserve weapon available—loan-financed public works or capital development schemes with an accommodating credit policy to meet the needs for working capital, so that investment and employment could expand at existing interest rates. This was the 'special case' for public works (VI: 337). In the general case, monetary policy concentrating on the long-term rate of interest and the adjustment of supplies of credit to the varying needs of the economy was the appropriate technique of aggregate economic management.

Late 1930 to early 1932 was the period during which professional economists absorbed and reacted to the *Treatise*. By late 1931, Keynes made a series of statements to others, including Japanese readers of the *Treatise* (V: xxvii), of his intention that he was 'endeavouring to express the whole thing over again more clearly and from a different angle; and in 2 years' time I may feel able to publish a revised and completer version' (XIII: 336).

The sources of criticism varied. One had been available *before* publication—Hawtrey's comments on Keynes's proofs were eventually published as Chapter VI of *The Art of Central Banking* in 1932 and did not get Keynes's attention until after publication. Other academic reviews and comments included Hayek's

‘Reflections on the Pure Theory of Money of Mr. J.M. Keynes’, Robertson’s ‘Mr. Keynes’s Theory of Money’ and further discussions with Robertson and Pigou. Then there were discussions with younger Cambridge colleagues, academic visitors, and students.

These discussions reflected the arrival of a younger generation in Cambridge. Richard Kahn and Piero Sraffa had already had some impact on the *Treatise*. The others included Austin Robinson, a recently appointed University Lecturer and Fellow of Sidney Sussex College, his wife, Joan Robinson, about to commence a career of prolific publication, and James Meade, recently appointed a Fellow of Hertford College, Oxford, who was spending the year at Trinity College studying economics. This group began to meet in the Michaelmas Term of 1930 in Kahn’s rooms in King’s to discuss the *Treatise*. Their discussion continued into the Lent Term before they became a more formal ‘seminar’ held in the Old Combination Room of Trinity. To this ‘seminar’, participation was by invitation, with undergraduates wishing to receive invitations having to satisfy an interviewing board of Kahn, Austin Robinson, and Sraffa. The two sets of discussion took on the name of the Circus, which has an alternative definition of a scene of lively action and a group of people engaged in common activity—in this case trying to understand the *Treatise*. Keynes did not attend. Kahn played, in Margaret Meade’s words on a later occasion, the role of the messenger angel, ‘who brought messages and problems from Keynes to the “Circus” and who went back to heaven with the results of our deliberations’ (XIII: 338–339; Lambert 1969: 250).

As noted, the criticisms that Keynes received from different sources varied. In many cases, they reinforced each other. If they came from outside and they were taken up or echoed by members of the Circus or merely Kahn, who from this time was Keynes’s intellectual confessor on economic–theoretical issues, they were likely to strike home. Kahn’s role in the process was critical for, as a Fellow of King’s, he could nag Keynes over an issue until Keynes saw where the difficulty lay. From the Circus and other sources of dissatisfaction to the beginnings of creation, where Kahn also played an important role, was but a short step.

In addition, Keynes was receiving suggestions of an alternative focus that would successfully take account of movements in output and employment. Just before Keynes finished the *Treatise*, Kahn, puzzled by Chapter 6 of *Can Lloyd George Do It?* (IX: 102–110) which dealt with the amount of employment that would be provided by the Liberal Party’s 1929 election proposals for public works, developed a framework for estimating the effects of loan-financed government expenditure on a public works programme

(now known as the employment multiplier). He presented his early results to the Committee of Economists of the EAC, of which he was secretary and Keynes the chair, in September 1930. In the months that followed, in conjunction with Colin Clark of the EAC and Meade in Cambridge, Kahn worked out the implications more fully before publishing them in the *Economic Journal* for June 1931 (Kahn 1931). Kahn's article had two implications for Keynes's work. Focusing on the supply curve for output as a whole, Kahn realised that the *Treatise's* Fundamental Equations were a limiting case. Keynes had assumed that output as a whole was inelastic to changes in demand. Relaxing that assumption, as Hawtrey had also realised in his criticisms of the *Treatise*, markedly affected the analysis and brought it 'very much closer to the actual conditions that prevail to-day [1931]', although neither Hawtrey in 1930 nor Kahn in 1931 perceived how much that difference would be (Kahn 1972: 9–10). Kahn also provided in 'Mr. Meade's Relation' an outline statement of the resources available for this investment when the supply of output was less than completely elastic to demand changes. The argument and its corollaries were not yet clear to Kahn—much less to Keynes.

Thus in the summer of 1931 Keynes had started to work it out all over again. In the course of a visit to Tilton which ended on the weekend of Britain's departure from the gold standard on 21 September, he and Kahn had been discussing some of the issues (XIII: 373–376). On 22 November, Keynes reported to his wife Lydia that after the usual distractions of the King's audit were over, 'I have begun writing in my chair about monetary theory' (Keynes quoted in Moggridge 1992: 536). He sitting in that chair in the famous cartoon by David Low (XIII: xvi). By late February 1933, with *The Means to Prosperity* (IX: 335–366) ready for publication in *The Times*, he had moved a considerable way from his position of 1930–1931.

To sort out how far Keynes was towards what became *The General Theory* at any time up to his sending it to his publisher for the last time at the end of 1935, we have to have some notion of the distinctively 'new' elements in the final product. I should note, however, that despite the initial discussion in the late 1930s and immediately after the Second World War, most of this interpretative literature dates from 1961. It followed a period of over 20 years of professional agreement as to what *The General Theory* was about. Many of the seemingly endless subsequent discussions reflect attempts to look at Keynes's work in the light of recent theoretical preoccupations or to claim Keynes's paternity for one's own heterodox ideas. But it also reflects the richness of a book that was breaking new ground—witness Keynes's own 'long struggle of escape' (VII: xxiii) from earlier modes of analysis.

After the publication of the book, in virtually identical letters to Roy Harrod and Abba Lerner, both of whom had published evaluations, Keynes set out the stages of his quest:

You don't mention effective demand or, more precisely the demand schedule for output as a whole, except in so far as it is implicit in the multiplier. To me, regarded historically, the most extraordinary thing is the complete disappearance of the theory of the demand and supply of output as a whole, i.e. the theory of employment, after it had been the most discussed thing in economics. One of the most important transitions for me, after my Treatise on Money had been published, was suddenly recognising this. It only came after I had enunciated to myself the psychological law that, when income increases, the gap between income and consumption will increase,—a conclusion of vast importance to my own thinking but not apparently, expressed just like that, to anyone else's. Then appreciably later came the notion of interest being the measure of liquidity preference, which became quite clear in my mind the moment I thought of it. And last of all, after an immense lot of muddling and many drafts, the precise definition of the marginal efficiency of capital linked one thing with another (Keynes to Harrod, XIV: 85; Keynes to Lerner, XXIX: 215; underlining in original).

This account of the stepping stones to *The General Theory* has four steps: (1) the consumption function, (2) the theory of effective demand, (3) 'the notion of interest being the measure of liquidity preference', and (4) the marginal efficiency of capital.

Scholarly accounts of the making of *The General Theory* have normally started from this list and sometimes added or subtracted items. Peter Clarke more or less stuck to the list, although his discussion adds another element, the notion that the whole might be more than the sum of its parts, which had been in Keynes's thought since his undergraduate contact with G.E. Moore and can hardly be 'new' to *The General Theory*. However, Keynes may have recognised the greater significance of organic unities in economics (Clarke 1988: 259–59, 269–272; O'Donnell 1989: 177–178). Don Patinkin, arguing that the notions of liquidity preference and the marginal efficiency of capital were both previously available – the former in the *Treatise* and the latter in the work of Irving Fisher – concentrated on the development of the theory of effective demand, most notably the equilibrating role of changes in income implicit in the multiplier (Patinkin 1976). But it is clear from the available texts that Keynes did not come to the marginal efficiency through Fisher but rather worked it out for himself and only had Fisher's work pointed out after the fact (Kent 2014). Robert Dimand (1988), in contrast, adds to Keynes's

list the notion that the labour market may fail to clear because workers cannot adjust their real wage by bargaining for nominal wages.

The dating of the development of Keynes's thought may also be subject to scholarly disagreement for another reason—the nature of the evidence required to indicate that he had taken a particular idea into his mode of thought. At one extreme, one could take Patinkin's approach in *Anticipations of the General Theory* (1982) towards others' claims to have anticipated *The General Theory*—the exposition of the doctrine in a form intended for a professional readership—a criterion suitable for the question of crediting a person with a scientific discovery. In his *Keynes's Monetary Thought* (1976), when coming to settling an upper limit to Keynes's own discovery of the central message of *The General Theory*, Patinkin used the evidence of lecture notes and other unpublished material (Patinkin 1982: especially 11, 16, 85; 1976). At the other extreme is Clarke's (1988: 258) criterion of 'indications of developments in [Keynes's] thinking which represented his initial insight, even if they were only disjointed flashes of illumination'. Such a criterion will produce an earlier set of dates than either of Patinkin's criteria, but one needs additional evidence to ensure that the disjointed flashes were more than just the 'mirages' that Keynes remembered:

To the extent to which one sees one's destination before one discovers the route is the most obscure problem in the psychology of original work. In a sense, it is the destination that one sees first. But then a good many of the destinations so seen turn out to be mirages. Only a small proportion of one's original intuitions survive the struggle of trying to find the route to them (Keynes to Falk, 19 February 1936, quoted in Moggridge 1992: 551).

The evidence from the documentary remains of the preparation for *The General Theory* is far larger than for the *Treatise*. There are draft tables of contents, fewer in number than for the *Treatise*, but a larger number of the related draft chapters surviving even from the earliest stages. Fragments, often running over several chapters, exist for over half a dozen drafts of the book, and there are three almost complete sets of proofs over which Keynes corresponded with other economists. Several sets of student lecture notes survive from his annual series of eight lectures. These were collected, transcribed, and collated by T.K. Rymes in his *Keynes's Lectures 1932–35: Notes of Students* (Rymes 1989a) and distilled into *Keynes's Lectures 1932–35: Notes of a Representative Student* (Rymes 1989b). In Keynes's case, the eight sets of more or less independently taken notes from good students, most of whom became professional economists, show high levels of agreement over what seems to have been said. On occasion there are fragments of the draft from which Keynes lectured. Much

more correspondence also survives from this period than from the years of the *Treatise*. Finally, there are Keynes's own publications, the publications of others involved in the process of creation, and subsequent memoirs and reconstruction of what went on. It is probably the case that with *The General Theory*, scholars have the most voluminous record surrounding the creation of any classic work in economics.

What does this evidence show? If one takes the view that the concept of the marginal efficiency of capital originated with Fisher, the emphasis of the story would be on output equilibration and liquidity preference: there are traces of the former in the autumn 1932 lectures. If one takes Keynes's letter to Harrod literally, that liquidity preference came *after* output equilibration, he must have had the former in place. This is Clarke's argument (1988: especially 164). At this point, any discussion of the chronology of Keynes's ideas runs into problems, for we have few dates for any fragments of manuscript until Keynes gives his next set of University lectures in the autumn of 1933, again under the title 'The Monetary Theory of Production'. By then it is clear that he had seen the equilibrating role of fluctuations in output associated with the theory of effective demand. If we want to pick an earlier date in 1933, we have to rely on circumstantial evidence such as a chapter, 'The Parameters of a Monetary Economy', which could have been written soon after the Michaelmas 1932 lectures, or the revisions to his essay on Malthus for *Essays in Biography* taking advantage of Sraffa's contemporary work on Ricardo which involved his correspondence with Malthus (X: 87–91, 94–100). Was it a coincidence that these passages on effective demand and Malthus's relations with Ricardo were added in October and November 1932 or that the concluding passages concerning savings and investment and the rate of interest were added in later December 1932 and early January 1933? Then there is *The Means to Prosperity*, a series of articles which appeared in *The Times*, completed on 6 March 1933, and published between 13 and 16 March. After publication, Keynes decided to think of expanding the argument in several directions, the first being 'the train of reasoning by which I arrive at the multiplier relating secondary employment to primary employment' (XXII: 63). An article on 'The Multiplier' appeared in the *New Statesman* for 1 April 1933 and was incorporated into the American version of *The Means to Prosperity*. Both Patinkin (1976: 79, 1982: 33–34) and Dimand (1988: 158–159) dismissed *The Means* as evidence because it does not contain the necessary formal theoretical justification—which was hardly appropriate for the place where the article appeared. That came later in Keynes's 1933 lectures and, for the marginal efficiency of capital, the summer of 1934. Even though he had started sending chapters to the printer on 13 September 1934, it was another 15 months before he returned the corrected

proofs of the Preface dated 13 December 1935, and the index. *The General Theory* came out on 4 February 1936.

It was priced at five shillings to make it readily available to students and, as it turned out, ensured that Keynes made almost no money from it. The book was reviewed everywhere. Roger Backhouse (1999) collected a selection of reviews in English during the first year after publication which turned up examples in such unusual places as the Kingston Ontario's *Whig Standard*. Of the reviews Keynes noted that he had 'no companions...in my own generation, either of earliest teachers or of earliest pupils' (XIV: 85): Dennis Robertson and Hubert Henderson were unimpressed; A.C. Pigou was positively irritated. The same could be said of senior American economists Frank Knight (born 1885) and Joseph Schumpeter (born 1883). He received a more sympathetic reception from Jacob Viner (born 1892). Finally, Alvin Hansen (born 1887) reviewed the book twice. His tone was sympathetic but suggested it would be no better than the *Treatise*, given its defective theory of interest and 'its special notion of equilibrium at less than full employment' (Hansen 1936: 83). Ironically, Hansen, who moved from the University of Minnesota to Harvard in 1937, would become Keynes's major senior interpreter in the USA.

On both sides of the Atlantic, it was younger economists who took to the book, analysed it, and tried to develop it. Some of the enthusiasts had helped in the development of the theory—Harrod, Kahn, Meade, Austin, and Joan Robinson. Others had been Keynes's supervision pupils—David Champernowne and Brian Reddaway—or had heard the theory developed in Keynes's lectures—R.B. Bryce, Alec Cairncross, Walter Salant, and Lorie Tarshis—or had heard it at the London and Cambridge research student seminars where Joan Robinson and Kahn tried to spread the word—Abba Lerner. Others had taken it from the book itself—Kaldor and Hicks (newly appointed to a Cambridge Lectureship from the London School of Economics who reviewed it for the *Economic Journal*). In 1936–1937, often in fruitful collaboration and interaction, as at the September 1936 meeting of the Econometric Society at Oxford, many of the younger economists discussed the book (Brown 1988; Young 1987). Keynes read most of the papers and 'approved' of all: we know of no adverse comments. But then as he had told Hicks in 1935: 'I deliberately refrain in my book from pushing anything very far, my main object being to press home as forcibly as possible certain fundamental opinions—and no more' (Keynes quoted in Hicks 1973: 8).

Many of the expositions attempted to formalise the model and did so along remarkably similar lines, as Oscar Lange, an early user of such models, pointed out in 1938 (Lange 1938). All worked in terms of a simple model that was to

become the mainstay of undergraduate expositions of the theory for the rest of the twentieth century.

Keynes's reaction to the reviews and expositions was interesting. He replied only to Viner's views at any length. In private, he expressed his views on some of the others, finding Pigou's review 'profoundly frivolous in substance', and bemused by what he took to be Knight's two main conclusions—'namely that my book caused him intense irritation, and that he had great difficulty in understanding it' (XIV: 87, XXIX: 257). Of the younger people's reviews and expositions he was tolerant, perhaps because he believed that 'if the simple basic ideas can become familiar and acceptable, time and experience and the collaboration of many minds will discover the best way of expressing them' (XIV: 111).

3 Keynes and International Monetary Reform

In discussing international monetary reform, I shall not deal with substantial proposals which were designed for a single country such as India in 1913, Britain in 1919 and 1922–1925, various European countries in the 1920s, and Britain and members of the sterling area after Britain left the gold standard in September 1931. Some schemes which were capable of generalisation—the *Tract on Monetary Reform*, and 'Notes on the Currency Question' (XXI: 16–28)—were largely transitional, designed to meet British problems at the time of the return to gold in 1925 or the departure in 1931. I will concentrate on the three major reform proposals that Keynes involved himself in, two of which proved influential enough to shape official British opinion. The three are the 'ideal' scheme of *A Treatise on Money*, the Keynes–Henderson plan of 1932–1933 and the Clearing Union proposals of 1941–1943.

Judging from the last two surviving tables of contents (XIII: 78–82, 113–117), Keynes's *Treatise* proposals were largely worked out during 1929–1930. At the time, Britain was struggling to remain on the gold standard at pre-war parity in the face of pressures arising from the Wall Street boom and then the slide into depression, a possible shortage of international liquidity as a result of the rising statutory reserve ratios accompanying Europe's post-war gold standard legislation, exchange rate undervaluations, and reserve asset preferences (XIX: 750–760, 775–780).

Keynes took most subjects of the *Treatise* at two levels—theory and application—with each getting its own volume. The theoretical volume of the *Treatise* argued that it was possible through interest rate policy in a closed economy to achieve price stability and full employment except in the face

of exogenous rises in wage costs per unit of output (efficiency wages in the language of the *Treatise*). In this latter circumstance, monetary policy was 'singularly ill-adapted' (VI: 245) for maintaining price stability. In an open economy, matters were more complex, for the equilibrium conditions of Keynes's model then required for each country that its rate of foreign lending (a function of relative interest rates) equalled its current account surplus (a function of relative prices or efficiency wages).

With these considerations in mind, Keynes turned to discuss international monetary arrangements in Chapter 21 of the first volume of the *Treatise*. He dealt with the problems of international balance with mobile international capital under three possible regimes—flexible exchange rates, gold standard fixed exchange rates, and gold exchange standard fixed rates. Flexible exchange rates had the advantage of working directly on relative price levels and of insulating the domestic economy from foreign-originated price changes but not interest rate movements. As Keynes summed it up:

With a local standard of value the dilemma which sometimes faces a central bank, that it may be impossible to preserve both internal equilibrium and external equilibrium, at the same time, presents itself much less acutely. For the central bank is free to vary both the rate of foreign exchange and the rate of interest, applying appropriate doses of each at the right moment. There is much less risk of loss of wealth and output due to the prevalence of general unemployment. For direct changes in the price of foreign-trade goods can be largely substitutes for unemployment as the first link in the causal chain whereby external equilibrium is restored and maintained (V: 325).

Such a regime, however, had the possible disadvantage that it discouraged foreign lending. Foreign lending was much more variable in the short run than the current account balance. There would be a need to respond with changes in the rate of interest which would not be appropriate to the internal situation. Keynes was also inclined to believe that international adjustment was asymmetrical with the reserve-losing country suffering the brunt of any change unless it was large in relation to the rest of the world or more willing than reserve-receiving countries to accept wider swings in its reserves as a proportion of money income (V: 302–304). The gold exchange standard brought additional asymmetries, for there was the problem of the behaviour of reserve centres (V: 315–318), as well as movements of balances between reserve centres for local rather than systemic reasons. Keynes was prepared to concede that the only satisfactory way of working the gold exchange standard system would be under the aegis of an international bank which would be the sole depository of central bank exchange reserves, the aggregate of whose balances

in favour of central banks would be determined by considerations of international policy. In such a system, the proportion of reserves held by central banks at the international bank would truly be a substitute for gold, whilst the benefits of reciprocal action between one country and another would be maintained (V: 318–319).

When it came to applications, Keynes devoted more space to contemporary international problems which took up more than half of the eight chapters on the management of money. He elaborated on the problems of the gold exchange standard: of statutory reserve ratios tying up too much of the world's monetary gold and of the relationships among central banks when dominant central banks might use their power to promote national policy at odds with the interests of the system as a whole—something he thought had happened in the USA in 1928–1929 and in France in 1930 (VI: 256)—before turning to questions of national autonomy and should standards of value be international. In 1930, unlike in 1924, Keynes was prepared to answer yes to this last question, largely because he believed that foreign lending was on balance desirable and because he seemed to believe 'the various difficulties of an internationally managed gold standard... could be overcome within a reasonable period of time' (VI: 303). Such a regime could be the classical gold standard with immutably fixed exchange rates, albeit with wider gold points than in the past. Wider gold points were one way of increasing national autonomy—one which had become more acute in the post-war period with increased international investment and slower current account adjustment (VI: 276–277). Other ways of strengthening national policy autonomy were increased internationally available reserves, either owned or borrowed, and a willingness to use them to tide over a country experiencing short-term disequilibria, more regulation of long-term lending to tailor it to the evolving current account position, attempts to reduce the size or domestic impact of short-term capital flows by gold devices, and reduced links between internationally and domestically effective short-term interest rates and official foreign exchange operations. Thus the 'rules of the game'—a phrase invented and popularised by Keynes (X: 220, XXI: 42, VI: 272)—would operate over the medium to long term but not in the short run.

To be successful as an international system, the gold standard necessitated an attempt to regulate the international value of gold 'to conform to a somewhat crude tabular standard' made up of an index of ocean freight rates and the prices of 62 major commodities moving in international trade (VI: 357). Keynes's minimalist proposal was an extension of the 1922 Genoa Resolutions on Currency (VI: 354–358). His maximalist proposal was a supranational

central bank. The supranational bank would need no capital, although its liabilities would be guaranteed by the member central banks. The liabilities of the proposed bank would be supranational bank money (SBM), which would be gold convertible in both directions with a 2% margin between buying and selling prices. Individual national currencies would only be encashable against SBM in the first instance, and SBM would be equivalent to gold for reserve purposes. Members of the system could obtain SBM by depositing gold, receiving SBM from another central bank, or borrowing from the supranational bank—the borrowing quotas and interest rates to be determined by the supranational bank itself, with the former bearing some formula-like relation to the member banks' deposits. The supranational bank could also conduct open market operations to affect the supply of SBM requiring the consent of the relevant monetary authority for purchases denominated in its own currency. If securities came to be denominated in SBM, the supranational bank could operate at its own discretion. The objectives of the bank's management would be to stabilise the value of SBM in terms of the tabular standard and to avoid international discrepancies between market and natural rates of interest (VI: 358–360). The bank would need to be independent with 'a high degree of authority and discretion in daily management, subject to the ultimate control of a board of supervisors consisting of the adherent banks' (ibid.: 360).

There Keynes left the plan, although he thought that the recently opened Bank for International Settlements (BIS) might serve as a nucleus out of which his supranational bank might evolve (ibid.: 363). The proposal seems to have come late in the drafting of the *Treatise*. He did not raise the matter in his 'private evidence' to the Macmillan Committee, although some of the underlying analysis appeared in the course of Governor Norman's evidence (Committee on Finance and Industry 1931a: QQ. 9197–9229) and in the Committee's *Report* (Committee on Finance and Industry 1931b: Part II). Probably he did not believe that the scheme had any chances of success. Nevertheless, various aspects of the proposals kept reappearing in the years that followed, initially in 'Notes on the Currency Question' of November 1931 (XXI: 16–28) and the Keynes–Henderson Plan of 1932–1933.

The Keynes–Henderson Plan first appeared in a memorandum that Hubert Henderson sent to Sir Richard Hopkins, Second Secretary in the Treasury, on 17 May 1932, with the comment, 'Don't trouble to tell me it isn't practical politics. Assuming that it were, is there anything wrong with it?' (Henderson quoted in Howson and Winch 1975: 115). Under the scheme, entitled 'A Monetary Proposal for Lausanne', which referred to the forthcoming conference on reparations, the BIS would issue gold equivalent notes interest-free to every government that recognised them as the equivalent to gold, agreed

to return to a fixed parity, abandoned exchange restrictions, and agreed to repay the notes as prices rose towards their 1928 level. Governments could also use the new reserves to repay external debts and/or finance the balance of payments effects of expansionary domestic policies. Allocations to members of the scheme would be on the basis of some criterion such as 50% of the gold value of a country's exports in 1928, these initial quotas providing the basis for the phased repayments. Outstanding balances in excess of a country's quota would earn 5%. The total of the note issue, allowing for a 5% cushion to strengthen the resources of the BIS, would amount to about £1,000 million in gold.

Henderson's scheme received some Treasury discussion prior to Lausanne. During the Conference itself, Keynes wrote to the Prime Minister: 'I hope you will have seen the currency plan which Henderson has put forward. I should like to say that I am very strongly in favour of it. This or something which amounts to the same thing, though dressed up differently, seems to me what the situation requires' (XVIII: 378).

After Lausanne, the Henderson Plan found another forum. The Committee on Economic Information of the EAC, of which both Henderson and Keynes were members, included a version of the scheme in its Fourth Report of 20 July, recommending it as an important subject of discussion for the forthcoming World Economic Conference which the countries represented at Lausanne had asked the League of Nations to organise (Howson and Winch 1977: 278–281).

Keynes and Henderson were also involved in British preparations for the World Economic Conference as members of another EAC Committee on International Economic Policy, which agreed at its first meeting on 3 August to look at the later version of the Henderson Plan. The Committee included a scaled-down version of Henderson's proposal, drafted by Keynes and Sir Basil Blackett, in its first Report of 6 October. It then reverted to the original Plan of July 1932 after reading a Treasury memorandum on its first Report and another on the work of the Preparatory Committee for the World Economic Conference on its second.

By then, a version of the Plan was in the public domain. Keynes started publicising it at the end of 1932. On 30 November 1932, he finished an article on the forthcoming conference and sent a copy to the Prime Minister. The article appeared in the *New Statesman* on 24 December (XXX: 210, 215–216) and later in *The Means to Prosperity* (IX: 357–364), originally published in *The Times* between 13 and 16 March 1933.

Keynes's proposal in *The Means* differed in detail from Henderson's. First, the BIS disappeared, first tentatively in a version of 17 February

(XXI: 232), then completely in *The Means to Prosperity* pamphlet, where Keynes proposed a new international authority. Second, in allocating quotas under the new scheme, Keynes used 1928 gold reserves as the criterion, with the proviso that no country should receive more than \$450 million. Third, the gold value of the new notes was defined in terms of the US dollar—just as it left gold! Fourth, Keynes was more explicit about the control of the new institution, electing the governing board by votes weighted by countries' quotas in the new scheme. Fifth, the governing board could alter either the initially low rate of interest (or nominal level) or the volume of the issue outstanding to avoid the gold price level of primary products in international trade rising above some norm which would be between that of 1933 and 1928. Sixth, the assistance could be used not merely to remove exchange restrictions, but also to organise arrangements to liquidate blocked foreign balances and remove tariffs and quotas imposed for balance of payments rather than permanent national policy reasons. Finally, the scheme did not envisage a return to a rigid gold standard regime: rather, Keynes proposed a sort of crawling peg regime with the range of fluctuation around the peg of + or $-2\frac{1}{2}\%$. As he put it, echoing *A Tract on Monetary Reform* (IV: 149–150):

The de facto parity should be alterable, if necessary, from time to time if circumstances were to require, just like bank rate—though by small degrees one would hope. An unchangeable parity would be unwise until we know much more about the future course of international prices, and the success of the boards of the new international authority in influencing it; and it would, moreover, be desirable to maintain permanently some power of gradual adjustment between national and international conditions (IX: 362; underlining in original).

The Plan came to nothing: it was not in the briefs for the British delegates to the Conference.

Keynes's attitude towards controls on capital movements was also altering. One sees it clearly in a letter to L.F. Giblin discussing the Australian economic situation and a possible change in Australia's fixed exchange rate with sterling:

Whether or not it will be opportune in the near future to modify your rate of exchange, I think that the kind of monetary and economic policy which you are now pursuing... may involve such modification from time to time in the future. At any rate informed opinion will, rightly or wrongly, endeavour to anticipate such changes from time to time. This will lead to movements in balances which may be on a formidable scale. Since the exodus of balances will arise principally whenever the falling away of your London balances suggests the possibility of a

further devaluation, they will always tend to aggravate a delicate position. The point is, however, obvious, and I need not enlarge on it.

But although this is obvious, the natural conclusion has not, I think, been drawn as decidedly as it should be. I am more and more convinced that the employment from time to time of an alteration in the exchange rate as a part of monetary management is inconsistent with freedom of capital movements. I believe that most countries, Great Britain no less than Australia, will sooner or later have to have completely controlled exchange rates in the sense that no transactions which do not arise out of current trade will be permissible except by licence (Keynes to Giblin, 22 April 1936, John Maynard Keynes Papers, King's College Archive Centre, Cambridge University: L36/46–47).

I now turn to Keynes's last, and most famous, plan for international monetary reform—the Clearing Union (called a Currency Union until its fourth draft). As with the *Treatise* proposal, the Clearing Union came with a fairly full justification—one amplified in extensive related correspondence. Unlike the *Treatise*, the Clearing Union proposals went through a number of drafts—eight in all—and there were significant changes. I shall not work through all the drafts in detail. Instead, I look at Keynes's overall justifications for the proposals, such drafting changes that illustrated these, and one puzzle.

Keynes's first draft set out clearly his view of the problems to be addressed. Some of these were peculiarly British: the wartime loss of export markets, the decline in the surplus on invisibles, and the large increase in short-term overseas liabilities despite the liquidation of many of her overseas investments in the sterling area and elsewhere. But it was Keynes's views of how the international monetary system had worked previously, more than parochial British worries, that shaped his scheme.

Keynes's first, and major, generalisation was that the international monetary system had only 'worked', in the sense of allowing the international division of labour and economic expansion to go forward without severe balance of payments problems, for two brief periods in the previous 400 years: the age of Queen Elizabeth I, when the world was awash with silver from the New World, and the age of Queen Victoria, when the system of international investment centred on London put the onus for long-term adjustment on the creditor (XXV: 21). There was not, he asserted, any smoothly functioning, automatic, *laissez-faire* mechanism that would preserve equilibrium in theory or historical experience. Nor had the interwar period thrown up any useful approaches to the problem except that there was the germ of a good idea in the German system of clearing agreements (*ibid.*: 21–23)—something that he had already written on the previous year (*ibid.*: 8).

Keynes believed that the failure of freely convertible international metallic standards lay in a single cause: they placed the burden of adjustment on debtor or deficit countries. This had occurred for a number of reasons, the most important of which were the fact that reserves were finite, and borrowing made adjustment compulsory for the debtor but optional for the creditor and that the deficit country was normally small in relation to the rest of the world, so that even under the price-specie-flow mechanism the deflationary pressure on the debtor was greater than the upward inflationary pressure on the rest of the world (*ibid.*: 27–28). In addition, as Keynes had argued from *A Tract on Monetary Reform* onwards, deflationary adjustments were more difficult and socially disruptive than inflationary adjustments. Indeed, it was the fact that potential surplus countries carried the burden, through either inflation or foreign lending that had made the Elizabethan and Victorian experiences exceptional. He continued:

I conclude, therefore, that the architects of a successful international system must be guided by these lessons. The object of the new system must be to require the chief initiative from the creditor countries, whilst maintaining enough discipline in the debtor countries to prevent them from exploiting the ease allowed them in living profligately beyond their means (*ibid.*: 30).

At the end of his ‘lessons from the past’, Keynes discussed freedom of capital movements. In the nineteenth century, he argued, such flows had maintained international payments equilibrium and at the same time developed the resources of the world. They had ceased to do this in the interwar period—indeed, in the 1930s, capital had flowed from countries with adverse current account balances to those in surplus, aggravated by flows of refugee and speculative funds. As there was no security against this happening again, especially in a world where social changes affecting the position of wealth owners would occur or be threatened, he concluded that any post-war scheme would normally involve regulated capital movements (*ibid.*: 30–31).

Moving from general principles to particular proposals brought a number of problems. There was the matter of rules versus discretion, where Keynes tried several variants, starting more with rules and moving towards more discretion, particularly regarding members’ policies (*ibid.*: 80–81, 178–179, 466–467). He also faced the problem of finding the appropriate balance between ‘discipline’ on the part of debtors and ‘initiative’ on the part of creditors, most notably towards creditors, to the extent that Keynes finally confessed: ‘We have been very gentle towards the creditors because we are a little scared of them. Possibly that is a part of the scheme that could be stiffened

up, if those concerned are prepared to stiffen it' (*ibid.*: 211). This confession brought forth Dennis Robertson's piece of verse, which he later expanded (Robertson 1952: 173):

Are we to love, honour, cherish or thank or

To kick the bottoms of the blokes who hold bancor (Robertson quoted in *ibid.*: 215).

What has rarely been noticed in previous commentaries is that with the substantial loosening of the conditions on creditors and relatively slight adjustment of the position *vis-à-vis* debtors came other changes. Most notable were the changes in quotas: for example, for Britain, the first two drafts gave a quota of £623.4 million; in subsequent drafts where a formula appeared it was £1,475.1 million or less and £1,106.3 million (Mitchell and Deane 1962: 284). Keynes was at least implicitly aware of the trade-off between liquidity and adjustment. Similarly, as the adjustment provisions for creditors were eased, the transition period to the new regime became less explicit (compare *ibid.*: 50–51 with 81, 187–189).

This brings us to the puzzle: the choice of exchange rate regime was the adjustable peg. As we have seen, Keynes had not advocated such a regime in his previous full-blooded international schemes. He seems only to have considered the adjustable peg prior to 1941 for the case of sterling area countries pegging to sterling, where the 'obvious' or 'natural' conclusion was, as he had told Giblin in 1936, 'completely controlled exchange' rates (see above, p. 531).

Yet, in the Clearing Union from the earliest drafts, Keynes thought in terms of discrete change in parity of the order of 5%. Indeed, when Roy Harrod suggested in December 1941 the possibility of smaller changes, Keynes stated that it seemed 'it would be preferable if a change [in rate] were necessary, to make it by a single significant bound rather than a series of small steps' (*ibid.*: 97). One would have thought that discrete exchange rate changes of the order of 5% would produce large speculative changes along the 'obvious' lines of his letter to Giblin, yet Keynes, the experienced speculator, thought that changes of this order would not be 'obvious' as transaction costs would eat up speculative profits (*ibid.*: 107–108). It appears that the norm of exchange controls with convertibility limited to current account transactions played a more central role in the Clearing Union vision than is often realised.

I also think, however, that there was another factor at work. Keynes had probably changed his mind over the efficacy of exchange rate changes. As always, he took the view that they would be most appropriate when a country's efficiency wages in terms of money had got out of line with those elsewhere—

not something that happened very suddenly (*ibid.*: 105 (see also 323–324), XXVI: 32–33). In addition, he appears to have come round to the view that the usefulness of exchange rate changes as a method of restoring international equilibrium was limited. To some extent, this reflected analytical confusions, which came out clearly in his 1944 discussion of quotas versus depreciation with Marcus Fleming (*ibid.*: 287–304), yet it also reflected a shift in view towards elasticity pessimism, a worry that money wage indexation would impair the effectiveness of devaluation and as hopes diminished that a scheme for stabilising primary product prices would get off the ground and remove another source of balance of payments difficulties. Yet he could also be inconsistent and agree with Jacob Viner that the published version of the Clearing Union ‘had unintentionally provided in effect for more rigidity in exchange rates than it [would] be wise to bind the post-war world in advance’ (XXV: 324).

The Clearing Union represented a negotiating position. Keynes recognised that compromise was inevitable. One of the functions of the Plan was, by providing an alternative to any US proposals, to get the Americans committed to there being some plan and to secure their involvement in post-war international economic institutions (*ibid.*: 242, 368). It was clear to Keynes soon after the Clearing Union and the American Stabilization Fund proposals were published that the final compromise would follow the lines of Harry White’s scheme (*ibid.*: 268, 282–283, 297, 308–314).

There then began the long process of discussion, drafting, redrafting, negotiation, and compromise that ended with the Bretton Woods agreement of July 1944 which has been fully documented by others (Horsefield 1969: Chapters 3, 4, 5, and 6; Pressnell 1986: Chapters 4, 5, 6, and 7). In the process, both sides gave ground as the British, faced with a less ambitious, more interventionist plan, tried to enlarge it and ensure it was less ‘grandmotherly’, as Keynes put it (XXV: 333) and, especially on exchange rates, gave more room for national sovereignty. The smaller scheme also raised the problem of negotiating the transition to peace, for it was too small to play an important role in financing the disequilibria involved. Again there was compromise, with the Fund having a role in monitoring or regulating the inevitable exchange restrictions, but with the ultimate date for current account convertibility left undefined, and with the Fund having some role in exchange rate changes.

The final stages in the process of creating the Fund and getting the necessary accessions to membership to turn it into a going concern coincided with the period in which Keynes was involved in planning and negotiating financial assistance for Britain’s transition to peace. Indeed, parliament voted on the

American Loan Agreement and accession to the Bretton Woods institutions at the same time as it agreed to sponsor American Proposals for Consideration by an International Conference on Trade and Employment. This coincidence, along with certain passages in Keynes's last writings, particularly his House of Lords speech on 18 December 1945 and his posthumous 'The Balance of Payments of the United States', has led some to the view that Keynes, under American pressure, changed his mind on many issues and moved him in a 'liberal' direction. Such a view is not new.

Although it would take another paper to sift through and marshal all the relevant evidence in volumes XXIV and XXVII of the *Collected Writings*, the readily available evidence casts doubt on this interpretation. The first piece of evidence relates to the convertibility provision of the Loan Agreement where Britain agreed within one year of the Agreement coming into force to end the transition period protection allowed under Article XIV(2) of the IMF Articles of Agreement, thus accepting current account convertibility. The initial proposal for sterling area convertibility came from Keynes and was part of his instructions agreed before the negotiations. The Cabinet got cold feet when Keynes followed his instructions—colder feet when the Americans suggested that convertibility be extended beyond the sterling area and for it to take effect at the end of 1946, six months before the date finally agreed (Moggridge 1972a; XXIV: 442, 570–577, 587–604). The sterling area offer was dependent on the amount of American (and Canadian) assistance available. In these respects, the terms were worse than what Keynes called 'Justice' (although better than what he thought likely in the spring of 1945). They were an improvement on 'Temptation' and the amounts were not that different from what Keynes thought necessary (Moggridge 1972b).

The story is still more complex. Within ten lines of the reference to 'modernist stuff, gone wrong and turned sour and silly, [which] is circulating in our system, also incongruously mixed, it seems, with age-old poisons' in the posthumous paper, Keynes was commending overall import control, along with exchange rate variations, to the 'classical medicine' (XXVII: 445). His notes for his Cambridge Political Economy Club talk of 2 February 1946 refer to the mixture of the classical doctrine supplemented by exchange variations and overall import control as 'the modern version of economic liberalism' (Keynes quoted in Moggridge 1992: 824) and then with reference to the Anglo-American settlement of the Fund, the World Bank, and the International Trade Organisation, 'My complaint would be that they do not go far enough in a liberal direction' because of their provision for tied loans, subsidies (especially to shipping) and protection (Keynes quoted in Moggridge 1972b: 167).

Moreover, the paper was very carefully constructed and widely discussed (Moggridge 2001).

The paper had originated during the Loan negotiations when, in collaboration with Frederic Harmer (Treasury) and David McCurrach (Treasury Delegation in Washington, D.C.) Keynes wrote a paper, 'Will the Dollar Be Scarce?', which he sent to London on 25 October. The paper concluded:

It is a safe rule that the contingency against which all of us are most concerned to guard against is almost certain not to occur. Too many people expect the dollar to be scarce for it to be at all likely that this will really happen. If it does there will have been few examples in the history of the world when so many people have been so right. Hoping that this does not detract from the force of our conclusion we may add that the U.S. Treasury experts who have been working at this subject have come to the same conclusion as we have (Keynes quoted in *ibid.*: 815–816).

After he returned from Washington, Keynes worked the paper up for publication in the *Economic Journal*. He told Roy Harrod on 4 January and sent him a first draft three weeks later. The article received Treasury approval on 2 February, the day Keynes talked to the Cambridge Political Economy Club. As he agreed with his Treasury colleagues that the paper should not be published before the Loan Agreement was through Congress, Keynes took the opportunity to update his statistical material. While he was in Washington and Savannah for the inaugural meetings of the Fund and the World Bank, he circulated galley copies to several people, including Graham Towers, the Governor of the Bank of Canada, and Harry White, and revised the paper again. At the time of Keynes's death on 21 April, this re-revised paper was on his desk with a covering note to Harrod who received it on 25 April. The June 1946 issue of the *Journal* was delayed a month, and the paper was published in July after the Loan had passed Congress.

Reviewing the two volumes of the *Collected Writings* which contain the papers relating to the Clearing Union and Bretton Woods, John Williamson concluded (1981: 543–544):

Time after time in these volumes, one finds Keynes urging graceful concessions with a view to achieving agreement and maintaining the momentum towards co-operation ... Elsewhere he conceded as gracefully to the force of circumstances as to the force of argument ... The next leading economist with policy influence who chooses to use his authority to make concessions that succeeded in achieving a second best will surely merit the sort of tributes that Keynes received.

4 Conclusion

One of the characteristics of Keynes's working habits that changed in the course of his career was his increasing dependence on collaborators. If one looks at the Prefaces to his books, acknowledgements to individuals do not appear until *A Treatise on Money* (1930). In the August 1929 draft Preface to that book, he named Dennis Robertson, Hubert Henderson, Frank Ramsey, Piero Sraffa, and Richard Kahn (XIII: 83). The final version reverted to Henderson and Robertson (both born in 1890) and Kahn (born 1905). In *The General Theory*, Ralph Hawtrey (born 1879), Roy Harrod (born 1900), and Joan Robinson (born 1903) joined Kahn in the acknowledgements, while a generally younger cohort of Cambridge-connected economists entered his correspondence—John Hicks (born 1904), James Meade (born 1907), R.B. Bryce (born 1910), Alec Cairncross (born 1911), David Champernowne (born 1912) and Brian Reddaway (born 1913). Abba Lerner (born 1903) and Nicholas Kaldor (born 1908) became part of his circle of non-correspondents. The development of national income analysis brought others in, notably Colin Clark (born 1905) and Erwin Rothbarth (born 1913), while Richard Stone (also born 1913) was in regular touch with Clark (XII: 799, 802). With Rothbarth, Meade, and Stone, Keynes's proposals in *How to Pay for the War* (1940) and the Budgets of 1941 and after, each accompanied by a National Income and Expenditure White Paper, put in place a system of analysis that proved very useful for the estimation of the necessary changes in policy. The same structure of estimates also found use in wartime discussions of what could be 'afforded' in discussions of post-war policies such as the Beveridge proposals for social insurance and what seemed plausible in thinking about post-war employment policy (XXVI: Chapters 4 and 5).

One other result of the national income exercises was the White Paper on Employment Policy (Cmd. 6527, May 1944). Here, Meade was the moving force in the inspiration and, as Director of the Economic Section of the Cabinet Office, implementation of the policy during the first two years after the war (Howson and Moggridge 1990a).

Keynes's collaborators also played a crucial role in the development of the post-war international economic order as it emerged in the IMF and World Bank, where the scale and complexity of the negotiations required strong support for Keynes (Howson and Moggridge 1990b: Chapters 3 and 4). In the case of the international trade negotiations, it was again Meade who provided the initial scheme for an international Commercial Union and, with the support of other younger colleagues as well as his often more

sceptical seniors, saw it eventually form the basis for the December 1945 Anglo-American agreement to sponsor an International Conference on Trade and Employment, where Meade's influence was recognised by all (*ibid.*: 186). 'Meade's work for the Section ended with his "triumphant return"... to Geneva' where he was involved in negotiating the General Agreement on Tariffs and Trade, an interim arrangement which would operate while a new International Trade Organisation was negotiated and approved (Howson and Moggridge 1990a: x). It was a very different environment from the years immediately after 1919.

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24

Gerald Frank Shove (1887–1947)

Claudio Sardonì

1 Introduction

Gerald Frank Shove was born to Herbert Samuel Shove and Bertha Millen at Faversham in 1887. He died in 1947 in Cambridge. He was educated at Uppingham School and at King's College in Cambridge, where he spent all of his academic life. A member of the society of Apostles, he was a close friend of Keynes and of most of the protagonists of the economic debates in Cambridge during the 1920s and 1930s. Shove's influence on those debates is not well documented by his writings. He published very little and wanted his correspondence notes to be burnt after his death.¹

Shove's colleagues and friends in Cambridge did little to make his work better known outside their circle. Austin Robinson, 30 years after Shove's death, devoted a few lines to him (Austin Robinson 1977: 28), and Richard Kahn wrote a short biography of Shove 10 years after that (Kahn 1987).²

¹ A few letters and some of his lecture notes have survived. See Harcourt and Araujo (1995) for a study of some of Shove's surviving correspondence.

² Austin Robinson limited himself to pointing out that Shove's gifts were critical rather than creative and that his pacifist ideas were at odds with the younger generation's positions in a 'world that was war-mad' (Austin Robinson 1977: 28). Kahn wrote: 'At no time in his life was Shove a ready writer. Although he produced only a few answers in each paper, they were of...outstanding merit ... His fastidious self-criticism explains why he rarely published his results and never wrote a book, but it was generally recognised that behind his greatness as a teacher lay originality as a dauntless thinker' (Kahn 1987: 327).

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In recalling the Cambridge intellectual atmosphere of the 1920s and 1930s, Kahn considered the importance of Shove's contributions in three areas: the debate on increasing and decreasing returns, the related debate on imperfect competition, and the Keynesian revolution. For Kahn, Shove made his most significant contribution on the issue of returns, but he may have underrated Shove's role in the debate on imperfect competition. Kaldor (1935: 34, fn. 3) described Shove's work on imperfect competition as containing 'one of the most penetrating analyses so far published on this whole subject'.

Kahn also held that Shove did not play any significant role in the Keynesian revolution. However, although there is no direct evidence that Shove played a direct role in the making of the Keynesian revolution, there is another aspect of the relationship between Shove and Keynes that is important. They both had a similar attitude towards the Marshallian inheritance. Shove, like Keynes, tried to retain the richness, complexity, and realism of Marshall's approach, even though he was aware of the need to go beyond this inheritance.³ Shove's attitude towards Marshall led him, on several occasions, to clash with economists of the younger generation who, in order to achieve a higher degree of rigour and formal precision, were trying to develop Marshall's economics by sacrificing those characteristics of Marshall that Keynes and he appreciated most.

Section 2 of this chapter examines Shove's position in the debate on returns while Section 3 looks at Shove's position in the debate on imperfect competition. Section 4 is concerned with the relationship between Shove and Keynes. Section 5 concludes.

2 The Problem of Increasing Returns

Shove always emphasised the extreme complexity of the economic system and the need for analytical tools to cope with such complexity. An illustration of his approach to economics is his contribution to the *Economic Journal's* 1930 Symposium on increasing returns and the representative firm.

The main issue discussed in the Symposium was the compatibility of the theory of competition with the existence of significant internal economies of scale that give rise to increasing returns. As Robertson (1930) put it, both Pigou and Sraffa were convinced that competition was incompatible with internal economies, but the former responded by essentially ignoring internal economies, while the latter made recourse to the theory of monopoly. As for Shove, he argued that competition and internal economies could be compat-

³On Shove's attitude towards Marshall, see Harcourt (1992).

ible, although he did not make the traditional distinction between internal and external economies and considered a kind of competitive equilibrium that was different from equilibrium under perfect competition.

Shove (1930: 94–116) started his analysis with a long quotation from an unpublished study of his.⁴ The quotation is concerned with those characteristics of long-period equilibrium that, for him, Marshall wanted the notion of the representative firm to display. The most important of these characteristics are: (i) if an entrepreneur has a special aptitude for the work in which he is engaged, his earnings are higher than those he could get anywhere else; (ii) the actual earnings of individual firms may also differ from one another due to random causes; (iii) a firm's profit varies over the different stages of its life; and (iv) equilibrium for the industry as a whole does not imply equilibrium for all individual firms operating in it.

For Shove, however, the use of Marshall's representative firm is appropriate only to describe equilibrium as the outcome of the rise and fall of individual firms. Marshall's representative firm is not useful when, as in Shove's article, equilibrium is considered as 'arising from the ebb and flow of all kinds of resources from one occupation to another' (*ibid.*: 96). In this case, a different approach is required, and Shove proceeded to his description of equilibrium.⁵

Long-period equilibrium in an industry is realised when it is not profitable for any agent to modify the existing allocation of resources among firms—in other words, when it is not profitable either to concentrate resources in a smaller number of firms or to distribute them over a larger number of firms. This sort of equilibrium position is naturally affected by the initial distribution of resources and, hence, there exists an infinite number of equilibria, each corresponding to a particular initial distribution (*ibid.*: 98). This approach to equilibrium makes it possible to allow for any degree of heterogeneity of resources without any need to aggregate them. For Shove,

the economic problem presented by the real world seems to be much more a question of sorting out and fitting each into its appropriate niche a vast number of heterogeneous individuals and activities than of regulating and directing into the proper channels large homogeneous streams of standardised productive agents: a jig-saw puzzle rather than a problem in hydrodynamics (*ibid.*: 99).

⁴This study concerned the relationship between costs and output. It was never published and the manuscript must have been destroyed after Shove's death.

⁵Robertson (1930: 87), instead, was convinced that it was possible to allow for internal economies without having to abandon the theory of competition. For this purpose, he used the Marshallian notion of the representative firm.

The more traditional approach to equilibrium, based on the equality between supply and demand at the industry level, encounters a significant difficulty. In order to construct the supply curve of a certain good, it is necessary to construct the supply curves of all the factors of production that, to be aggregated, must necessarily be measured in quantitative terms, that is, independently of their value. But all attempts to find this measure have failed (ibid.: 99–100).⁶

Shove then considered the ‘more difficult matters’, that is, the issue of increasing returns and their compatibility with competition. Having defined internal and external ‘economies of large-scale *industry*’ (ibid.: 104; italics in original)⁷ and pointed out that the expansion of an industry as a whole can give rise to both types of economies in individual firms, he distinguished between ‘(1) the improvements in organisation which are made possible or profitable by an increase in the total output of the industry as a whole, and (2) the improvements in organisation which an individual firm...would obtain by increasing its own output while that of the industry as a whole remains unchanged’ (ibid.: 101).

Shove used the term ‘economies of large-scale industry’ for the effects that the expansion of the industry has on the productivity of individual firms’ resources, and ‘economies of *individual expansion*’ (ibid.: 104; italics in original) for the effects of an increase in the size of a single firm, the size of the industry as a whole being given. He regarded this distinction as more useful for trying to ‘unravel the relations between the size of an industry and the efficiency of the businesses which compose it’ (ibid.).

Equilibrium is compatible with both economies of large-scale industry and of individual expansion. But it is incompatible with some types of economies of large-scale industry, which Shove called economies of concentration, that is, those advantages that derive from an increase in the average size of the business and can be enjoyed at the given degree of knowledge and intelligence of the operating entrepreneurs. Since, in equilibrium, no redistribution of resources is possible, economies of concentration must be ruled out (ibid.: 104–105).⁸

⁶Although, in his Symposium article, Shove referred only to labour and land, he was also aware of the specific difficulties related to the measurement of aggregate capital. He dealt explicitly with the problem of the measurement of capital in his review of Hicks’s *Theory of Wages* (Shove 1933a: 470–471).

⁷Internal economies are those advantages that a firm obtains ‘from the efficient organisation of *the resources which it employs directly*’ (Shove 1930: 101; italics in original). External economies are the advantages that a firm derives from ‘the organisation of *those outside resources, not in its own direct employment, whose services or products it uses*’ (ibid.: italics in original).

⁸Shove also introduced the term ‘economies of *rationalisation*’ (ibid.: 105; italics in original), that is, increases in efficiency due to a redistribution of resources within the industry made possible by improvements in the degree of knowledge, expertise, and so on.

Having introduced this classification, Shove went on to consider the compatibility of economies of individual expansion with competitive equilibrium. If these economies exist and firms do not reap these advantages, there must be obstacles that prevent them from doing so. The two most important obstacles are increases in the costs of both transport and marketing, which a firm would bear if it tried to advance into its competitors' markets (*ibid.*: 105). Such obstacles make equilibrium compatible with increasing returns due to the expansion of individual firms. These obstacles, on the other hand, would not exist if the industry as a whole expanded. In this case, in fact, single firms would not have to struggle to conquer a market share belonging to others.

The notion of competitive equilibrium adopted by Shove is not that of a perfect competitive equilibrium,⁹ but he also considered the case of perfect competition.¹⁰ Again, equilibrium would be compatible with increasing returns. Shove, following Marshall, introduced the time element into his analysis:

It may be true—to take an extreme case—of every firm in the industry that its average cost would be lower if, *other things being equal*, its share in the aggregate output were larger. But expansion takes time, and further time is required before the newly-installed equipment can yield all its fruit. During this interval other things are not likely to remain equal ... If a firm could enlarge its output instantaneously, then indeed the predominance of internal economies would, on our present hypothesis, be incompatible with competitive equilibrium; but since it cannot, the two conditions can be reconciled (*ibid.*: 109–110; italics in original).

Shove's attempt to set the analysis of returns within a dynamic context was followed by Harrod (1930, 1931) in his early works on imperfect competition, but it was rejected by other participants in the debate.¹¹

At the time of the Symposium, Sraffa had already introduced his notion of imperfect competition (Sraffa 1926), and the debate on market forms was taking place in Cambridge as well as elsewhere. In his article, however, Shove continued to refer to competitive markets, even though his idea of competition was different from what was commonly meant by perfect or

⁹ He was aware of this: 'Mr. Sraffa will say, perhaps, that the equilibrium reached under these conditions is not competitive but monopolistic ... Whether a given situation is to be called competitive or monopolistic is, of course, a question of words ... We have simply to inquire whether, and if so why, substantial economies of mass production are consistent with the survival of a large number of competing firms. I shall therefore merely observe that if competition implies not only a perfect market but also a complete absence of transport charges, it never exists in practice. We are no longer being asked to explain what happens in the real world but to solve a purely abstract and hypothetical problem' (*ibid.*: 108–109).

¹⁰ That is to say, an unrealistic situation in which neither transport nor marketing costs are significant.

¹¹ Two years before Shove and Harrod, Young had assigned the time element great importance for the analysis of increasing returns (Young 1928).

free competition. Shove's view was in several respects closer to Marshall's idea of competitive markets. Marshall never gave a precise definition of competition, but he described in several well-known instances the behaviour of firms in terms that were more compatible with a hypothesis of imperfect competition than with the prevailing (in the 1930s) definition of a perfectly competitive market.¹²

3 The Discussion about Equilibrium and Imperfect Competition

In 1930 and 1931, Harrod published two important articles in which he showed that it was possible to have long-period equilibria characterised by the existence of firms whose size is less than the optimum, that is, firms that produce at a higher average cost than is technically possible. In equilibrium, price equals average cost and marginal cost; therefore, if the firm produces an output less than that which could be produced at a minimum average cost, this implies that it experiences decreasing average costs and has excess capacity. In other words, there is equilibrium with increasing returns. In order that such an equilibrium be possible, firms must face a downward-sloping demand curve. The downward-sloping nature of individual demand curves depends on firms' increasing difficulty in marketing their additional output (Harrod 1931: 566). Harrod expressed in a more formal way what Sraffa had already pointed out in 1926.¹³

Harrod, like Shove, tried to explain this kind of equilibrium by introducing some dynamic considerations.¹⁴ However, Joan Robinson who, in 1932, published an article on 'Imperfect Competition and Falling Supply Price', did not follow Shove and Harrod along this path. Her analysis was rigorously static.¹⁵

¹²On Marshall's notion of competition and its relationship with the Cambridge debate on imperfect competition, see Whitaker (1989: 172–184).

¹³'The chief obstacle against which [firms] have to contend when they want gradually to increase their production does not lie in the cost of production—which, indeed, generally favours them in that direction—but in the difficulty of selling the larger quantity of goods without reducing the price, or without having to face increased marketing expenses. This...is only an aspect of the usual descending demand curve, with the difference that instead of concerning the whole of a commodity...it relates only to the goods produced by a particular firm' (Sraffa 1926: 543).

¹⁴Harrod, for example, argued that conditions of increasing returns are normal in an industry where the rate of expansion of the optimum plant is higher than the rate of expansion of demand. For a more detailed reconstruction of the 1930s debate on imperfect competition, see Sardonì (1999).

¹⁵The time required for reaching equilibrium was ignored; firms were supposed always to be in equilibrium; the supply of every factor of production to the industry was assumed to be perfectly elastic; there were no economies of large-scale industry. Finally, all firms were assumed to be similar with respect to

In particular, Robinson argued that the equilibrium of individual firms does not imply that the industry as a whole is in equilibrium. In the long period, new firms could enter the industry and cause aggregate output to vary. The industry will reach equilibrium when profits have reached their normal level in all firms.

Such a notion of industry equilibrium raised two issues: Is free entry compatible with imperfect competition? And how is normal profit to be defined? For Robinson, the profit that a firm expects in order to be induced to enter an imperfect market must be higher than under perfect competition because the entry into imperfect markets is more difficult than into perfect markets. This, however, does not mean that profits in an imperfectly competitive industry are higher than normal; in fact, the entry into other imperfectly competitive industries is difficult as well. But, in any case, this problem is not relevant because, for a particular industry, it is sufficient to define the normal level of profits as that level just sufficient to dissuade the entry of new firms (Joan Robinson 1932: 547). The normal profit so defined is then included in firms' long-period costs. The industry is in equilibrium when price equals average total cost—a condition that can be fulfilled only when the individual demand curve of the firm is at a tangent to its average cost, that is, there is equilibrium with excess capacity.

Robinson ascribed this result to Kahn. Shove, in fact, called it 'Mr. Kahn's theorem' (Shove 1933b: 118) and took issue with it. Shove raised two main questions. The first was concerned with supply functions and, more particularly, with the relation between changes in demand and costs. Shove argued that 'the cost at which a firm provides any given output depends on the way in which it provides it, and this in turn depends on the character of the demand for it' (ibid.: 115). Therefore, the cost curve, in general, cannot be drawn, as Joan Robinson drew it, as if it were independent of the demand curve. The second question concerned the concepts of normal profit and equilibrium. Shove argued that the industry equilibrium, defined as the absence of a tendency for output to change, does not require profits to be normal. Profits can be below the normal level provided that they are not below that level which would cause some firms to leave the industry (ibid.: 119). If there were perfect competition and, hence, no costs of entry into an industry, the two limits for profits would coincide. But, given the assumption of imperfect competition, entry costs are positive, so that the two profit levels do not coincide.

As a result, despite below-normal profits, the industry output does not vary. Hence, the stability of the industry output does not require that price equal average cost. The equality between price and average cost is necessary

costs and conditions of demand, even though they were not alike from the viewpoint of buyers (Joan Robinson 1932: 544–545). Imperfections were assumed to derive only from differences in transport costs.

only to ensure the maximum level of profits: 'Mr. Kahn's theorem does not mean that the individual demand curve of the firm must be tangent to its average cost curve in order that there should be no tendency for the industry's output to alter, but merely in order that the firm should be earning this maximum profit' (ibid.: 121). The equilibrium of the industry requires only that the price lies between the average cost defined to include Joan Robinson's normal profit and the average cost defined to include the minimum profit below which firms would leave the industry.

The last part of Shove's article was devoted to his alternative line of approach, which is based on the idea that the complexity of actual markets makes it very difficult to expound the analysis neatly and precisely.¹⁶ The main difficulty lay in the fact that, in general, costs cannot be regarded as independent of the state of demand. Total cost can be divided into two parts: the minimum cost of output (dependent on technical factors) and the special cost, related to advertising, transport, and so on. While the minimum cost in general depends only on the level of output, the special cost partly depends on the state of demand and partly on the technical constraints faced by the firm.¹⁷ In this context, a change in the character of demand is likely to alter the special costs associated with different levels of output. Economic theory must develop along such lines in order to account for these aspects of the actual world; otherwise, it becomes a mere intellectual abstract exercise.

In her *Economics of Imperfect Competition* (Joan Robinson 1933), Robinson essentially maintained the same line of approach. Shove's review of Robinson's book, although kind in tone, was in fact quite critical (Shove 1933c). He held that the book, far from being innovative, was a careful and detailed re-exposition of the conventional theory (ibid.: 657). Shove argued that the part of the book devoted to imperfect competition was less satisfactory than the part on monopoly, mainly because the degree of abstraction adopted by Robinson was unable to deal with the case of imperfect competition.¹⁸ The high degree of abstraction in Robinson's book made the analysis easier but also more distant from the real world.

¹⁶ 'So long as we are content with a rough and ready indication of the forces at work, we can keep fairly near to the facts: but any attempt to make our treatment exact is apt to lead either to a degree of abstraction which renders the apparatus inapplicable to the actual phenomena we set out to explain or to a degree of complication which makes it cumbrous to use' (ibid.).

¹⁷ For example, the cost at which a firm can provide advertisements, facilities, transport, and so on.

¹⁸ Moreover, 'in this branch of her work the author has not so consistently resisted the temptation to use a "technique" which is admittedly designed "for studying equilibrium positions" ... in an attempt to analyse the effects of change' (ibid.: 659).

Both Robinson and Shove were aware of the difficulties that arise in the analysis of the firm and the industry when the assumption of perfect competition is removed. Shove insisted on the need to cope with these difficulties rather than assume them away, whereas Joan Robinson assumed most of them away in order to render the analysis tractable.

Almost 10 years later, in 1942, Shove returned to the issue of the unrealism of the contemporary analyses of imperfectly competitive markets, and he contrasted these with Marshall's approach. According to Shove, Marshall was aware of the relevance and realism of non-competitive market forms. Sometimes, in discussing non-competitive markets, Marshall left his value theory ambiguous and vague, but this was because he 'accepted the view that value under monopolistic competition is theoretically indeterminate, and concluded accordingly that pure analysis could not accomplish much in that field' (Shove 1942: 320). Yet it was Marshall who first developed some of the analytical elements on which the modern theory of imperfect competition rested. He had introduced the device of contrasting the individual supply curve with the individual demand curve; the notion that equilibrium is realised when marginal cost equals marginal revenue; that in equilibrium marginal firms must earn a zero profit (*ibid.*: 321). Younger economists refined those concepts and made them 'more precise', but this was accomplished at a considerable cost to the realism of the analysis:

The attempt to fuse realistic study with theoretical analysis has, on the whole, not been followed up ... Partly no doubt this has been due to the itch for precise results: not all of us are content to act on the late Prof. Wildon Carr's admirable motto (which might well have been Marshall's), 'It is better to be vaguely right than precisely wrong' (Shove 1942: 323).

Shove's criticism of the younger generation's mishandling of the Marshallian inheritance raises the more general issue of how modern economists engage with economists of the past. Once again, Shove's ideas were developed in the context of a criticism of Joan Robinson's position. The occasion was the publication of her *Essay on Marxian Economics* (Joan Robinson 1942). In his review article of the book, Shove (1944) considered also Robinson's critique of orthodox economics and questioned the usefulness of the very notion of economic orthodoxy. For Robinson, for example, orthodox economists postulated the harmony of interests between classes; accepted the capitalist system as a natural and inevitable form of social and economic organisation; based their theory of value on the assumption of perfect competition; regarded

the distribution of income as determined by natural laws; and accepted the validity of Say's Law. Shove objected that it is impossible to identify a set of economists who share all of these characteristics (ibid.: 50–53).¹⁹ For Shove, 'what is wanted...is not an attempt to overthrow or disintegrate, but rather an effort, as detached as possible, to understand, to sift truth from error and to formulate precisely the problems which have been left unsolved' (ibid.: 60).

Shove stressed the limits of the past generation of economists and the need for theoretical advancement, but he also tried to maintain a 'generous' attitude towards the economists of the past. At the same time, he criticised his younger colleagues for having concentrated on those aspects of past economists' analyses that lent themselves to be developed more 'precisely' but were also more unrealistic and removed from the real world.

Perhaps Shove's emphasis on the need to develop a more realistic approach to economic theory influenced the younger economists in Cambridge in a more general though indirect way. Their enthusiastic taking up of Keynes's new ideas and their loss of interest in the formal analysis of individual firms and industries can be interpreted as an expression of their quest for a higher degree of realism in economics. They became convinced that the pressing problems of actual economies in the 1930s could be understood and dealt with by developing the analysis of the economy as a whole and by not assuming away essential aspects of the real world such as uncertainty and money.

4 Shove and Keynes

Shove was not a protagonist of the Keynesian revolution. Despite his long-standing friendship with Keynes, he did not participate in the elaboration of Keynes's new approach to the analysis of the economy as a whole in any direct way. There is, however, an exchange of letters between Shove and Keynes after the publication of *The General Theory* that reveals Shove's sympathetic attitude towards Keynes's book.

Shove believed that, although concentrating on microeconomic analysis, he had been following a line that was not very different from the one Keynes adopted in the development of his theory of aggregate output and employment. Shove wrote to Keynes:

¹⁹ When Shove considered Robinson's criticisms of Marshall and Wicksell, he accused her of being careless and unfair in her reading of their works. Shove argued that Robinson attributed to Marshall and Wicksell positions that they never held and, more generally, that her criticism of orthodoxy was driven by her ideological stance rather than by a scholarly attitude towards different theoretical approaches (ibid.: 60).

I have enjoyed reading the *General Theory* very much ... I thought you were too kind to the 'classical' analysis as applied to the individual industry and firm. Unless very special assumptions...are made, it seems to me either wrong or completely jejune. I have been groping all these years after a re-statement of it on lines similar in some respects to your solution for the system as a whole, stressing in particular 'expectations' and the influence of current and immediately past experience upon them. But I can't make it precise (Shove to Keynes, 15 April 1936, in Keynes 1973: 1).

Keynes replied:

What you say about the classical analysis as applied to the individual industry and firm is probably right. I have been concentrating on the other problem, and have not, like you, thought very much about the elements of the system. But you ought not to feel inhibited by a difficulty in making the solution precise. It may be that a part of the error in the classical analysis is due to that attempt. As soon as one is dealing with the influence of expectations and of transitory experience, one is, in the nature of things, outside the realm of the formally exact (Keynes to Shove, 21 April 1936, in *ibid.*: 2).

In his published works, Shove did not particularly emphasise the role of expectations in the analysis of firms and industries, but he certainly insisted on the complexity of economic phenomena and on the need to develop adequate analytical tools, which could not be too formal and 'precise'. In this sense, Keynes's reply is consonant with Shove's position: the quest for 'precision' possibly is one of the 'mistakes' of classical analysis.

Keynes, as is well known, never showed much interest in the 1930s debate on imperfect competition. Shove's contributions to that debate may help us better understand Keynes's attitude and scepticism on the issue. Keynes could ground his macroeconomic analysis on a notion of markets without any need to become involved with the 'esoteric abracadabra' of the contemporary theory of imperfect competition (Keynes 1973: 831). He could refer to a notion of competition and a vision of how markets work in reality that derived from Marshall.

Keynes and Shove belonged to the same generation of Cambridge economists who had been directly influenced and taught by Marshall. They both understood that it was necessary to go beyond Marshall.²⁰ Yet they never wavered in

²⁰Although Keynes probably had a keener sense than Shove of the limitations of the Marshallian inheritance.

their commitment to Marshall's approach to economic theory, in particular, his readiness to trade off analytical 'precision' for the sake of greater realism.²¹

Other Cambridge economists of the older generation, such as Pigou and Robertson, were less interested in the attempt to develop the Marshallian tradition along new lines; they certainly were less 'revolutionary' than Keynes. The younger generation was less 'generous' towards Marshall and tried to further economic theory along new and different lines. In the early 1930s, the younger economists concentrated on the attempt to develop a formal analysis of imperfectly competitive markets. Shove clearly did not appreciate that attempt, and Keynes, too, had doubts about the usefulness of the new developments in the analysis of individual firms and industries. In a sense, the younger economists came closer to the Cambridge realistic tradition when they lost their interest in the development of formal microeconomic analysis and turned their enthusiasm to Keynes's new theory.

5 Conclusion

Shove was a close friend of Keynes and many other Cambridge economists of that generation; he also taught many of the younger economists. He did not publish much but participated actively in the debates that took place in the 1930s; in particular, he made significant contributions to the debates on increasing returns and imperfect competition.

In the debates of the interwar period, Shove was a defender of Marshall's inheritance, even though he always shunned a dogmatic defence of Marshall's economics. What Shove appreciated about Marshall was his attempt to build a theory suitable to analyse and explain the extreme complexity of the functioning of market economies. He regarded the Marshallian approach as superior to one that assumed away most of the complex features of the economy. Shove's analytical and methodological stand led him to clash with some of the younger economists who followed a more formal analytical approach when dealing with the issues that interested Shove most.

Shove did not participate in the process that led to the Keynesian revolution, but he was sympathetic to what Keynes was doing. Shove and Keynes shared the same admiration for Marshall and his attempt to keep economic

²¹ Like Marshall, Keynes and Shove always looked with suspicion at excessive formalism in economic analysis, even though their attitude should not be interpreted as a rejection of the use of mathematics in economics. Keynes criticised 'pseudo-mathematical' methods of formalising economic analysis rather than the use of mathematics per se (Keynes 1936 [1973]: 298). Shove, although appreciating the merits of the mathematical treatment of some economic problems, stressed the need to apply cautiously the results of formal analysis to the real world. See, for example, his observations on Hicks's concept of elasticity of substitution and its use in the analysis of real economies (Shove 1933a: 468–469).

theorising as close as possible to the real world. They also felt that they had been trying, at least to some extent, to develop the Marshallian inheritance along similar lines, although in different fields.

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25

Dennis Holme Robertson (1890–1963)

Mauro Boianovsky and Charles Goodhart

1 Introduction¹

Much of what is now taken for granted as mainstream macroeconomics derives more from Dennis Robertson than from John Maynard Keynes. For example, that there is an equilibrium (nowadays described as NAIRU (non-accelerating inflation rate of unemployment)) in the labour market, and that any attempt to push demand above that level will just end in spiralling inflation; that the (quasi-) equilibrium² real interest rate is determined by real forces of thrift and productivity; that monetary policy is potent, so long as government deficits are not overwhelming, and should be primarily aimed at maintaining price stability; that government intervention to fix wage growth (incomes policies) or investment (indicative planning) will usually make things worse; and that the urge to invent, to develop, to improve will prevent stagnation. We call this technical

¹This chapter is partly based on some of our previous works on DHR (see Goodhart 1992; Goodhart and Presley 1994; Boianovsky and Presley 2009; Boianovsky 2014).

²Robertson would have noted that this 'equilibrium' value is always shifting, so it is a 'quasi-equilibrium'.

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progress, but Robertson used a more literary message, from Walt Whitman, 'Urge and urge and urge, Always the procreant urge of the world', a verse with which Robertson both opened and closed his first book, *A Study of Industrial Fluctuation* (1915) [1948].

Robertson never claimed to have constructed an overall system of thought about macroeconomics (such as Keynesianism, monetarism, and neoclassicism). He was distrustful of all 'isms', and believed not only that there was much value to be found in the theories of our forebears, but also that relationships between economic variables were changeable, depending on context. The world was dynamic, not static; there was no single final truth, nor equilibrium, rather a messy progress through an unknowable future.

Robertson was temperamentally conservative and became more so as he aged.³ Like his friend, mentor, colleague, and from the 1930s onwards sometime adversary, Keynes, Robertson had been politically a staunch Liberal in his early years. It was, however, Robertson's misfortune to be linked so closely with the even more brilliant and more radical Keynes, whose attempt to uproot, and sometimes Robertson felt to misrepresent, the theories and teaching of his predecessors, including those at Cambridge, for example, Marshall and Pigou, so upset Robertson. It was even more his misfortune that the temper of the time, particularly after the Great Depression, became more socialist and left wing. Robertson became rather isolated at Cambridge, and some of his work, notably the Cohen Council Report, of the 'three wise men' (Cohen Council 1958), was met by considerable left-wing abuse.

His was not an easy life. He had no family to fall back upon; he was homosexual when this was still legally and socially unacceptable, and, as far as is known, largely repressed it, with no regular partnership (see Fletcher *op. cit.* and Skidelsky 2003: 391–392). He was, however, bolstered by the community life of Trinity College, and by his cats, which were much in evidence when one of the present authors visited him at Trinity in 1958. Despite these various tribulations, he maintained a gentle optimistic balance, an occasionally sharp but self-deprecating wit, a determination to maintain the macroeconomic verities, whatever the current politico-economic fashions, and a lapidary writing style. He died too early (1963) to see the rebalancing of the mainstream of macroeconomics towards his own position, though he would have deplored the increasing mathematical technicality of modern macro, and would have had enormous pleasure in poking fun at the more extreme forms of 'rational expectations' and 'efficient market' theories.

³ For two good short biographies, see those in *Robertson on Economic Policy* (1992), mainly by Dennison, and in *Essays on Robertsonian Economics* by Presley (1992). For a longer, somewhat metaphysical, biography, see Fletcher's *Understanding Dennis Robertson* (2000). For a comprehensive treatment of Robertson's macroeconomics, see Presley (1979).

His father, the Reverend James Robertson, was a brilliant man, a Fellow of Jesus College, Cambridge, 1859, poet, mountaineer, and classicist, who ascended rapidly through the ranks of public school teaching (Cheltenham, Rugby, Harrow) to become Headmaster of Haileybury in 1884 at the age of 48. He did well (see Thomas 1987), until disaster struck. A boy called Henry Hutt was charged with stealing and sacked. His parents did not believe the charge and sued the school. A notorious court case followed, at which Robertson apparently did not acquit himself well (Rattigan's play 'The Winslow Boy' is supposedly based on all this), and the boy and his parents won—whether rightly or wrongly is unknowable, though Thomas (*ibid.*) provides some evidence that it was wrongly. Haileybury's reputation suffered, and Robertson felt forced to resign. He went to live at Whittlesford at Cambridge, with his career in tatters and most of his income gone. Into that background Dennis was born in 1890, the sixth, and last, child of James and his wife, Constance, a keen musician. Under those circumstances, it was not surprising that Robertson was tutored by his father at home (no money but lots of time). But what a pressure cooker it must have been! At least it did its job. He took second place in the Eton Scholarship election, going there in 1902. Keynes, Hawtrey, and Robertson all came from the same stable, King's Scholars at Eton followed by Cambridge, the fount of modern macro.

Be that as it may, Robertson early on showed his intellectual prowess, becoming Captain of the School (1907–1908) and taking the Newcastle Prize (the highest classical prize) as well as many other prizes, on his way to a major scholarship at Trinity. He turned to economics halfway through his undergraduate years—having obtained a First in Part I of the Classical Tripos—a switch whose cause is unfortunately not documented. With Keynes as his supervisor, he took Part II of the Economics Tripos in 1912, again getting a First. Keynes had become Director of Studies in Economics at Trinity in 1910 (Harrod 1951: 150).⁴

Despite the pressures of working for a First in economics in two years, Robertson nevertheless found time to become President of the Union, President of the Cambridge University Liberal Club, and President of the Amateur Dramatic Club, as well as winning the Chancellor's Medal for English Verse, for the third time running, *all in 1911*. He continued to write verse throughout his life.

⁴The father of one of the authors, A.L. Goodhart, went to Trinity in 1912 with the intention of studying economics. He was told by his Tutor that "Trinity had no economist. But they were sending their economics undergraduates to an economist at King's [J.M. Keynes], but that they [in Trinity] did not regard him as "sound". A.L.G. decided then to turn to a study of law. (Traditional family story.)

Cambridge economics and Marshallian economics were then synonymous. Although Marshall had retired, his *Principles of Economics* remained the recommended textbook. Pigou and Keynes, both former pupils of Marshall, continued to uphold the Marshallian tradition during Robertson's undergraduate studies.

After graduation, Robertson remained a research student in Cambridge. In 1914, his research thesis won him a Trinity Fellowship (having previously won the Cobden Prize); the thesis was to become *A Study of Industrial Fluctuation*. The war interrupted academic work; he joined the army and was posted to Egypt and Palestine, where he became battalion Transport Officer in charge of 'a circus of mules, camels and donkeys' (Dennison and Presley 1992: 20); he was awarded the Military Cross in May 1917, not returning to Trinity until 1919.

This heralded the beginning of his most productive period as a monetary economist. His widely read book, *Money*, was published in 1922, as a Cambridge Economic Handbook. Although a textbook for undergraduates, it quickly established Robertson's reputation as a monetary expert, remaining widely used until the 1950s, with a new edition as late as 1948. Friedman described it as 'a masterpiece of exposition as well as of content' (Friedman quoted in Presley 1979: 2). But *Money* was only the first of several books then attempting to analyse the role of monetary factors in the trade cycle. Robertson's *Study of Industrial Fluctuation* had presented a *real* theory of such fluctuations. His later writings sought to examine the behaviour of money, the rate of interest, and saving and investment in the cycle, and thereby the most appropriate counter-cyclical policies.

The collaboration between Keynes and Robertson in the 1920s resulted in several major works, though none published under joint authorship. These included not only *Money*, but also Robertson's *Banking Policy and the Price Level* and Keynes's *A Tract on Monetary Reform* (1923) and *A Treatise on Money* (1930). The 1930s witnessed less of a combined effort. Each went their separate ways, Robertson developing the theory of fluctuation he had expounded in 1915, whilst Keynes worked on *The General Theory of Employment, Interest and Money*. After 1936, they became involved in debates over aspects of Keynes's theories.

With a few exceptions (in 1926–1927 and 1933–1934), Robertson remained in Cambridge until 1938, when he was elected to a Chair in Banking at LSE, thereby temporarily escaping from the ferment of the Keynesian revolution in Cambridge. During the war, he worked as economic adviser to Sir Frederick Phillips, Third Secretary in the Treasury, with responsibility for overseas finance. This took Robertson to Washington, D.C., in 1943 in preparation for the Bretton Woods Conference. Here again he worked with Keynes in the British delegation. Meanwhile, on Pigou's retirement, and with

a partial reconciliation with Keynes following Bretton Woods, Robertson was able to return to Cambridge as Professor of Political Economy, a position he occupied until retiring in 1957. Much energy was devoted in this period to his lectures (which were published in three volumes, as *Lectures on Economic Principles*), and to general policy issues.

He gave evidence to the Macmillan Committee on Finance and Industry in May 1930, and to the Canadian Royal Commission on Banking and Finance in 1962. He was a leading member of the Royal Commission on Equal Pay (1944–1946), and the only economist amongst the aforementioned ‘three wise men’ of the Cohen Council on Prices, Productivity and Incomes (1958), whose first report was mainly written by Robertson. Honorary degrees were given to him by several British universities, as well as by Louvain, Columbia, Amsterdam, and Harvard. He became a Fellow of the British Academy in 1932, a Fellow of Eton in 1948, and was Knighted in 1953.

Robertson wrote nine books covering almost every aspect of economics, and had 91 articles published between September 1912 and September 1962, with 30 appearing in the *Economic Journal*. Many of these articles were collected to form six further books over the period 1931–1966.

Robertson was a classicist, with a majestic command of both language and logic, but he was no mathematician. He was however ready to support his theories with empirical exercises, as in his thesis and first book. Whether he would find the current state of economics, with its worship of technique, congenial is dubious. He responded to the encroaching requirements of such techniques with wit. The best-known example is his ‘Non-Econometrician’s Lament’, from the appendix of *Economic Commentaries* (Robertson 1956: 174):

As soon as I could safely toddle
 My parents handed me a model.
 My brisk and energetic pater
 Provided the accelerator,
 My mother, with her kindly gumption
 The function guiding my consumption
 And every week I had from her
 A lovely new parameter,
 With lots of little leads and lags
 In pretty parabolic bags.
 With optimistic expectation
 I started on my explorations,
 And swore to move without a swerve
 Along my sinusoidal curve.
 Alas! I knew how it would end;

I've mixed the cycle and the trend,
And fear that, growing daily skinnier,
I have at length become non-linear.
I wonder glumly round the house
As though I were exogenous,
And hardly capable of feeling
The difference 'tween floor and ceiling.
I scarcely now, a pallid ghost,
Can tell ex ante from ex post;
My thoughts are sadly inelastic,
My acts incurably stochastic.

2 Trade Cycles and Equilibrium

In our view, Dennis Robertson's main claim to fame as an economist lies, first, in his analysis of fluctuations in real factors, innovation, technical progress, the return on capital, investment, and so on, as the fundamental drivers of the trade cycle, and, second, in his emphasis on the accelerator principle, whereby an increase in overall demand, from whatever source, can generate an increased demand for investment, though with characteristic modesty, Robertson attributes his discovery of this to Aftalion (Robertson 1954b: 183). As Robertson liked to say: 'Dogs [consumption] wag tails [investment], as well as tails wag dogs'; n.b. business investment over the cycle is vastly more volatile than consumption. In *A Study of Industrial Fluctuation*, monetary considerations do not enter until page 211 (out of a total of 254 pages), and then in a purely supporting role, the idea that trade cycles are primarily monetary in character is roundly dismissed. Robertson later argued in a paper on 'The Monetary Doctrines of Messrs. Foster and Catchings', from *Economic Essays and Addresses*, that even a Robinson Crusoe, or a pure barter economy, would/could exhibit cycle-like misallocations of real resources (Robertson 1929 [1931]: 141).

He developed his business cycle theory over a long period, from the *Study of Industrial Fluctuation* to the third volume of his *Lectures*, published two years after his retirement from Cambridge. Robertson's best-known contribution is probably *Banking Policy and the Price Level* (Robertson 1926 [1949]), where he attempted to extend the real analysis of the *Study* to establish the notion of forced savings as a key concept in the business cycle. Nevertheless, he usually referred to his 1934 *Economic Journal* article on 'Industrial Fluctuation and the Natural Rate of Interest' as the core of his interpretation of economic fluctuations. This

piece introduced a diagrammatic representation of the market for loanable funds and established the twin notions of a natural rate of interest and a natural (or normal) rate of unemployment, which equilibrate the market for goods and for labour, respectively. That article grew out of another piece published earlier in the *Economic Journal*, where Robertson (1933) introduced the effects of unexpected price level changes on the level and distribution of output and on the savings curve in his loanable funds diagram. Whereas Friedman (1974: 40) regarded the determination of the natural rate of interest and the study of the saving–investment sector as ‘unfinished business’, this was precisely the business of Robertson in the 1930s.

Robertson’s dynamic method was based on period analysis, a ‘step-by-step approach’. He assumed a period of time, called a ‘day’, which is finite but nevertheless so short that income received on a given day can only be spent or saved in the next period—the ‘Robertsonian lag’. Furthermore, he assumed that output is given in the current period, so price level changes clear the market for goods during the ‘day’. (Problems with this approach, that later arose with Keynes and his followers, are discussed later in Section 3.) Unforeseen price changes affect real wages in the short period, since money wages (assumed contractually to be given during the ‘day’) are decided on the basis of the previous price level. Besides the effects of price fluctuations on the demand for labour, Robertson advanced the notion—later developed by Phelps (1969) and Lucas (1972)—that producers may temporarily mistake changes in the price level for changes in relative prices and adjust their supply of effort accordingly. In long-period macroeconomic equilibrium, defined by the equality between saving and investment at the natural rate of interest, price level expectations of workers and firms are correct and wages and profits are at their ‘normal’ level (see Robertson 1934: 651). The normal, equilibrium, rate of unemployment is positive because of changing economic conditions interacting with search in decentralised labour markets and heterogeneity of workers and jobs.

Keynes (1936 [1973]: 180–183, 242–243, 327) rejected Robertson’s concept of a normal rate of unemployment, leading to Robertson’s criticism that Keynes had overlooked workers’ reaction to market real wages distinct from their long-run expected values. Disequilibrium in the labour market is accompanied by disequilibrium in the market for loanable funds, with ensuing pressures on the market rate of interest. Robertson dismissed the Keynesian concept of liquidity preference and what Robertson named the ‘liquidity trap for saving’ (unless the only interest rate that could equate saving and investment at normal unemployment was negative). Robertson

explained the short-run oscillation of the level of employment around its normal value by the effects on output of unanticipated price changes. Rising prices affect the long-run rate of economic growth positively through forced saving, whereas changes in the price *level* have only short-run effects on employment and output. So, in the Robertsonian framework, money is neutral, but not ‘superneutral’.

Robertson (1915 [1948]) criticised the ‘monetary’ school of the trade cycle for overlooking the ‘*inelasticity*, in times of slump, of the demand for certain important things which are being relatively over-produced’, especially capital goods. Hence, ‘any attempt to expand output on the part of these trades would, even under barter, furnish an inducement to other trades to *restrict* output’. So ‘general overproduction’ (in this sense) will cause the ‘business classes’ to reduce their effort to a larger extent than the ‘working-classes’, who ‘tramp the streets striving to rid themselves of the blessings of leisure’ (Robertson 1923 [1931]: 133; italics in original). Robertson approved of the description of unemployment during the depression as ‘involuntary’, since overproduction of capital goods causes an inelastic effort-demand for *all* commodities (including instrumental goods), which is greater for businessmen than for workers, since the latter do not demand capital goods: ‘For those reasons it is plain that the scale of production which commends itself to the business class may be smaller than that which commends itself to the working classes’ (Robertson 1915 [1948]: 209–210; see also Robertson 1926 [1949]: 21–22).

In Figure 25.1, equilibrium is illustrated by the intersection between the curves of demand for investment DD' (‘representing the declining marginal productivity of new lendings in industrial use’) and of supply of saving SS' (‘representing the rate of new available savings per atom of time’), at the natural rate of interest PM (Robertson 1934: 651). Following Marshall (1890 [1920]: 533) and Ramsey (1928), Robertson usually assumed that the aggregate curve of savings is upward-sloping and positive if the rate of interest exceeds the pure rate of time discount. Should there now occur a shift upwards of DD' to D_1D_1' , via an exogenous increase in the marginal productivity of investment through, for example, technical progress, and the banking system keeps the market rate of interest at its initial level, the rate of lending will exceed the flow of new savings to the extent MM_1 , through newly created bank money.

Given the ‘Robertsonian lag’, real disposable income is then reduced by rising prices, and consumption falls below its expected value, which Robertson called ‘automatic lacking’ (Robertson 1926 [1949]: 48). Furthermore, additional saving may be ‘induced’ through the rise in prices, since individuals will seek to restore the real value of their money balances by reducing consumption (ibid.: 49). So, with monetary disequilibrium, developments in the

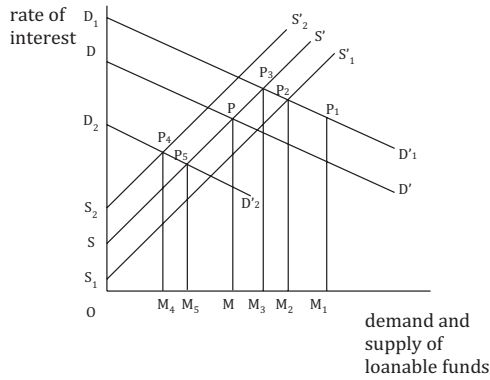


Figure 25.1 The natural rate of interest (Source: Robertson 1934: 652)

goods market decide the rate of change of the price level whereby lacking (in its voluntary, automatic, and induced forms) equals investment.

In his 1933 article on ‘Saving and Hoarding’, Robertson introduced yet another form of forced saving, brought about by a reduction of real wages to the extent that prices in the current period exceed the level expected when money wages were initially set. This followed from his distinction between ‘two classes, “the public”, whose rates of money income are prevented by contract or custom from varying during such short periods of time...and “entrepreneurs”, of whom this is not true’ (Robertson 1933: 401). While automatic and induced lacking only take place during the process of rising prices, forced saving which results from the ‘distortion of contracts occasioned by a rise in prices which has already occurred’ continues even after inflation stops; that is, the curve of voluntary lacking shifts to the right because of a change in income distribution in favour of the ‘entrepreneurs’ (ibid.: 411). The displacement of the savings curve SS' to $S_1S'_1$ is also explained by the effects of the decline in real wages on labour demand, which will ‘progressively increase total incomes and redistribute them in favour of entrepreneurs’ (Robertson 1934: 652).

Robertson (ibid.) coined the phrase ‘*quasi-natural* rate’ to describe the rate of interest P_2M_2 which would equilibrate investment and saving under the new conditions. Assuming that the actual rate of interest rises towards its quasi-natural level, the economy will settle at the level of savings (and investment) OM_2 , with stable prices and a rate of unemployment lower than its long-run average value. However, as pointed out by Robertson (ibid.: 653), this ‘*quasi-equilibrium*’ is temporary, since excess demand for labour at real wages which are lower than expected by workers will bring about an increase

in money wages in the next period, raising real wages back to their long-period equilibrium level and shifting the savings curve back to its initial position. Meanwhile, capital accumulation made possible by the forced saving process will reduce the marginal productivity of the stock of capital goods. Robertson (*ibid.*) describes this as ‘saturation with *existing* instruments’ (italics in original), displacing downwards the marginal productivity of ‘*new* lendings’ (italics in original) to $D_2D'_2$. The ensuing falling prices then causes automatic and induced ‘dislacking’ (an unanticipated increase in consumption by the public at lower prices, followed by a reduction in real money balances to their planned level (cf. Robertson 1926 [1949]: 48–50), as well as an increase in real wages involving ‘the shrinkage of income and its redistribution in favour of non-savers’ (Robertson 1934: 653). Hence, the savings curve shifts downwards to $S_2S'_2$, intersecting $D_2D'_2$ at the new quasi-natural rate of interest P_4M_4 . If the bank rate of interest is also reduced to P_4M_4 , the new position of quasi-equilibrium in a depression will feature a flow of savings OM_4 (equal to the demand for investment) and a rate of unemployment higher than its average value over the business cycle. Such a quasi-equilibrium lasts longer than the quasi-equilibrium position in a boom since, because the ‘short period is not of the same length at both ends’ (the length of life of durable capital goods is usually longer than the time taken to build them), the curve $D_2D'_2$ is more stable than $D_1D'_1$ (Robertson 1934: 654, 1957–1959 [1963]: 140). Moreover, owing to the partial downward rigidity of real wages, $S_2S'_2$ will not easily return to SS' .

Robertson (1934: 655) stressed that

if, in a society which has already become a prey to fluctuation, full employment of the factors of production, in their existing distribution between consumption and construction trades, is taken as the objective of policy, there seems a virtual certainty that normality will be overstepped, and the ball of cyclical fluctuations set rolling again.

Robertson probably had in mind a reduction of the bank rate of interest to such a level as to reduce real wages to an extent that $D_2D'_2$ would intersect a savings curve at the level of investment OM and a rate of unemployment lower than ‘normal’.

Some of the controversy between Keynes and Robertson following the publication of *The General Theory* should be read against the background of Robertson’s 1934 article. According to Keynes (1936 [1973]: 327), ‘Mr D.H. Robertson assumes, in effect, that full employment is an impracticable ideal and that the best that we can hope for is a level of employment much

more stable than at present and averaging, perhaps, a little higher', a position he described as 'defeatist'. Keynes's rejection of Robertson's view of monetary policy was preceded (*ibid.*: 180–182) by a strong criticism of the analysis of the determination of the rate of interest in the 1934 diagram (Figure 25.1), which Keynes adapted from Robertson (incidentally the only diagram used in *The General Theory*). In Keynes's view, Robertson's diagram could not be used to determine the rate of interest, since the savings function is not independent from the investment curve as a shift in investment causes a change in income and, therefore, a displacement of the savings curve. There are, therefore, not enough equations to decide the rate of interest, which should be solved by bringing in liquidity preference to determine the rate of interest from outside the savings–investment mechanism.

The question is whether a change in real income caused by a shift of the investment function, and the Keynesian multiplier, can displace the savings curve to such an extent that the quasi-natural rate of interest is pushed all the way back to the original level of the natural rate of interest. This has been discussed by Axel Leijonhufvud (1981: 165–169; see also Kohn 1981: 859–860), who argued that, in the case of a downward shift in investment, the market rate of interest would remain at its initial level PM (above the new natural rate P_5M_5) without any excess supply in the market for loanable funds (because of the corresponding shift of the savings curve through the multiplier) and, therefore, with no pressure towards the appropriate adjustment of the market rate of interest. In such a state of 'unemployment equilibrium', the price that is 'wrong' is the rate of interest, not the money wage.

Although Leijonhufvud successfully captured some elements of the Robertsonian approach, his general conclusion of unemployment equilibrium is not that of Robertson, *except* if money wages are fixed. Robertson's usual assumption was that money wages are 'relatively sticky' (Robertson 1957–1959 [1963]: 440; see also Robertson 1928a: 11 and Robertson 1933: paragraph 4), that is, not flexible enough to clear the market within a single unit period. Under these conditions of 'wage flexibility with a lag', if the bank rate of interest is above its natural level, the economy will suffer from continuous and steady deflation accompanied by constant (not rising) unemployment. Keynes's claim that a reduction in investment would cause unemployment to 'grow and grow until, as a result of the consequent reduction in real income and therefore in saving, a stable position is again reached at a very low level both of money income and of employment' is acceptable only under the assumption of fixed money wages (Robertson 1957–1959 [1963]: 442–443). As explained by Robertson (*ibid.*: 443), the money wage will be falling at the same rate as the price level, with a constant real wage rate (above

its long-run market clearing value) and a constant (but below long-period equilibrium) level of employment. So there will be continuous excess supply in the loanable funds market and downward pressure on the rate of interest caused by falling prices and wages, contrary to Leijonhufvud's interpretation (see also Kohn *ibid.*: 873–874).

Robertson's insight that a rate of unemployment below (above) its normal equilibrium level is accompanied by a continuous increase (fall) in money wages and prices is behind his criticism that Keynes did not contemplate the notion that a high level of employment in the transition to minimal unemployment could be associated with a positive rate of change of prices and money wages—that is, the notion of a Phillips curve. As put by Robertson (1936a: 436, b [1940]: 109; cf. Keynes 1936 [1973]: 303–304) on 'true inflation', in Keynes's view 'not until unemployment is conquered can inflation in any damaging sense be said to begin'. However, it was only in his *Lectures* (Robertson 1957–1959 [1963]: 437–438) that Robertson explicitly reacted against Keynes's (*ibid.*: 327) criticism as quoted above:

On p. 327 of the *General Theory* you will find that I am subjected by Keynes to mild reproof for having in the 1930s set my sights too low. That may or may not have been; certainly I thought—apart from all my criticism of detail—that the general tenor of that famous book, with its dramatisation of the contrast between general and mass unemployment on the one hand and 'full employment'—a phrase I have always mistrusted—on the other, over-simplified the problem of objectives as it then presented itself (see also Robertson 1948: 204–205).

Robertson did not change his views about the concept of a 'natural' or 'normal' rate of interest after the publication of *The General Theory*. However, Keynes's criticism led him to discuss aspects that were not explored in his 1934 article, for example, the effect of expected changes in the price level on the rate of interest. Robertson (1936: 178–179) pointed out that, with rising prices caused by excess investment, 'any given proportion of wealth or income idle in the form of money is being diminished by the expected depreciation of money, and dishoarding takes place'. Furthermore, an expected rise in prices tends to increase the rate of interest through its effects on the demand and supply of loanable funds—the so-called Fisher effect, which Keynes (1936 [1973]: 142) dismissed. The important point, made by Robertson in a letter of March 1935 to Keynes, is that the rate of interest will not rise in equilibrium to the full extent of the expected rise in prices. As explained by Robertson (see Keynes 1973: 522; italics in original), the rise in the rate of interest

will be damped down by the tendency of the owners of stores of money (a) as *interest* rises, to take money out of store and lend it thus increasing the supply of loanable funds, (b) as *prices* rise, to take money out of store and invest it themselves in labour or commodities, instead of adding to the demand for loanable funds for those purposes.

Both factors (a) and (b) contribute to diminish the Cambridge K and to reduce the long-run value of the ‘real’ rate of interest.

The Fisherian real rate of interest falls under anticipated inflation because of the reduction in real money balances. Investment and saving are both higher than in equilibrium without steady, anticipated inflation—money is not ‘superneutral’. Such an analysis fits very well with Robertson’s notion of ‘induced lacking’ as an equilibrating mechanism able to turn initially involuntary (‘automatic’) saving into a voluntary decision in the course of the business cycle (cf. Laidler 1999: 96). As pointed out by Robertson in his letter, Keynes’s discussion of the Fisher effect was marred by his habit of expressing liquidity preference in nominal terms instead of in real terms. Hence, contrary to Keynes, there is nothing contradictory about people holding a larger *nominal* amount of money at a higher (nominal) rate of interest, since they are in fact holding a diminished quantity of *real* balances.

3 Money

Since Robertson tended to give much less weight than certain contemporaries to monetary considerations in the determination of cyclical fluctuations, it may be thought surprising that he took on the authorship, after returning from the First World War, of the 1922 Cambridge Economic Handbook on *Money*. Keynes might have initially intended to do it himself, but, being too busy to do so, may have put pressure on his erstwhile student, junior, and friend to write it in his stead.

Though the book soon became justly famous, this was in some large part because of the lucidity of its presentation and its wit and humour; space allows but one example: ‘The mere assurance of convertibility, it is thought, would have the same kind of soothing effect as the sound of church bells in the distance, and be equally unprovocative of action’ (Robertson 1948: 60). It was also the first occasion when Robertson used Alice quotations at the head of each chapter, henceforth one of his trademarks. (As macroeconomics has gained in mathematical rigour, so it has lost in style.) But, leaving style to one side, it is in

some other respects, and with hindsight, slightly odd as a textbook on money. Although the value of money, the inverse of the price level, is emphatically stated to be the outcome of the interaction between the demand and supply of money—a quantity theory approach—there is relatively little formal discussion of the factors determining the demand, or the supply, of money.

The demand for money is taken to be conventionally related to incomes. Although ‘the demand for money, like that of the demand for bread, turns out to be the result of a process of individual weighing up of competing advantages *at the margin*’ (ibid.: 36; italics in original), the only example that Robertson gives of a serious shift in velocity, the Marshallian k , arises as a result of extreme inflationary (or deflationary) expectations, as in the German hyperinflation (ibid.: 117–119). For the remainder, Robertson tends to assume that velocity is generally stable, despite acknowledging that money is whatever may be currently acceptable in exchange (ibid.: 3–4). If the latter is true, might not financial innovations, and/or general reactions to monetary mismanagement, make both the definition of money and its velocity unstable? Again, there is remarkably little reference to the role of interest rates in equating the demand and supply of money. Nor is the supply of money treated with great care. The role of the central bank in controlling the supply of money does not appear until quite late in the book, in Chapter VIII on ‘The Question of the Cycle’.

Instead, Robertson focuses on three other questions. The first, which was a recurring theme for him, was whether it would be preferable to aim for a stable price level, a price level that declined slowly in line with productivity, or a gently rising price level, to provide encouragement for entrepreneurs. Whichever might be chosen, he was keen to add that its achievement should *not* be in the guise of a rigid rule, but a rough norm, or average, allowing some faster inflation in investment-led booms, balanced by some declines in prices either in depressions or when productivity gains were particularly strong. He was also prescient in his criticisms of the ‘real bills’ doctrine, which he termed the ‘Principle of Productive Credit’ (see Robertson 1928b [1940]).

The second main issue, as would be expected in a book first written in the early 1920s, concerns the exchange rate regime to be adopted. The discussion of this, in Chapter VII, is beautifully balanced. Moreover, in line with his quantity theory approach within a closed economy, Robertson develops the view that, in the longer run, the exchange rate between two economies is determined chiefly by relative national monetary growth operating through their respective price levels (see Humphrey 1992).

However, the core and focus of the book is in Chapter V on ‘Money and Saving’. The key quest of Cambridge macroeconomics in general, and of Robertson and Keynes in particular, in the interwar period was to analyse

how the ex ante desire to invest and to save, which were driven by different forces and were often far from identical, could be adjusted so that at every point of time ex post investment and saving had to be exactly equal in a closed economy. While (real) interest rates, dependent on productivity and thrift, were a part of the story, Robertson does not put much weight on that equilibrating force, in either *A Study of Industrial Fluctuation*, *Money*, or *Banking Policy and the Price Level*.

Instead, the deus ex machina is forced saving via changes in the price level. Entrepreneurs generally have little wealth of their own, and so turn to banks for loans to provide ‘circulating capital’. This gets spent on labour, raw materials, and so on, and the money stock rises (loans create deposits). With the output available fixed from production the day before (D_{t-1}), the Robertsonian lag, and money velocity assumed constant (k is fixed), prices have to rise. But the rise in prices reduces real incomes, again with nominal incomes having been earned in the previous day, and thereby forces income recipients to spend less than they had intended, as already described in Section 2. That, however, is far from being the end of the story. The rising prices, especially, but not necessarily, if wage rates remain sticky, encourage all entrepreneurs to raise output in this same period. But what was central was that the adjustment process was via price and monetary changes, involving ‘forced saving’, or the various forms of ‘lacking’ already discussed.

This central plank of Robertson’s theory was extended and embellished in *Banking Policy and the Price Level*, especially the key Chapters V and VI. What is remarkable, given what was to come later, was the recorded close involvement of Keynes in that analysis and his apparent commendation of it. In several ways, however, *Banking Policy and the Price Level* was something of a disaster. Robertson had a habit, shared with Lewis Carroll, of making up words; thus, in *Money*, he preferred to call bank deposits ‘chequeries’. Moreover, rather than pursue simplicity, he felt obliged to detail all possibilities. The result, of which *Banking Policy and the Price Level* is the main example, could be an almost impenetrable jungle of words.

Yet, the basic message of the price adjustment/forced saving approach is perfectly simple and intuitive. Moreover, Robertson was writing *Money* shortly after the 1919–1920 post-war cycle, largely driven by changes in prices of commodity inventories, and had the inflations in Central Europe to observe, so viewing the adjustment process through a flex-price lens was entirely understandable.

What happened between 1926 and 1936 was that Keynes moved on from a perfectly flex-price to a (temporarily at least) fixed-price model, so that adjustment took place primarily via output changes rather than through the

price mechanism (see, for example, Skidelsky 2003: 481–482). Why he failed to take Robertson with him, or perhaps failed to try to do so, is not clear. Thus, rather than having initial injections of (investment) expenditures leading businessmen to rewrite price tags continuously, the expenditures (output being given) would result in (unplanned) reductions in inventories of finished goods. Robertson had been aware of the possibility of destocking, but had asserted that stocks of finished goods were relatively small. In practice, he then, post-1936, retreated quite a long way from his forced savings theory. Thus in his ‘A Survey of Modern Monetary Controversy’ (Robertson 1938 [1940]: 106), which was aimed to take some of the heat out of the disputes, he wrote, ‘The increase in expenditure tends to make prices rise, but the use of accumulated stocks, and the expansion of output, tend to prevent them from rising’. Note that in the above brief account, the Robertsonian lag, whereby everything available today for sale is produced in an earlier period, and period-by-period dynamics are maintained, the Keynesian analysis still holds. Nevertheless, Keynesians tended also to assume that output could be varied within the immediate short period to meet changing demand and, even in some extreme cases, that the full multiplier process could be completed within the immediate short period. Robertson had some fun at their expense, and reasonably enough preferred his own methodology, but it did not disguise the fact that in this key field of analysis, Robertson had lost the battle, and he knew it.

Robertson never again tried to develop a holistic account of macroeconomic adjustment, once ‘forced savings’ had been largely shot down. Instead, he tended to snipe at the errors and excesses of the Keynesian revolution. There were several of these targets for Robertson. In particular, the fact that most goods and services exhibit sticky prices in the short run does not mean that businessmen will not adjust prices in the medium and longer term when trends in input prices, competitive output prices, and demand for their own product became more clearly apparent. But Keynesians initially extended the stickiness of prices to continue indefinitely over time, until full employment was reached, a reverse-L-shaped supply curve. Moreover, there was a tendency from the late 1930s to the 1950s to lower the assumed level⁵ of such full employment from perhaps 5% of the workforce unemployed in the 1930s to 1%, or even lower, in the 1940s and 1950s. Inflation occurring above this level of unemployment

⁵Skidelsky (2003: 711–712) has Beveridge giving a value of 8% to this and Keynes regarding 5% as ‘normal’.

was then largely attributed to cost-push pressures, and the socially ‘correct’ remedy was incomes policies.

All this was anathema to Robertson. The heterogeneity of the real economy, bottlenecks, transitional costs, and so on, meant that there would be no kink at full employment. In a debate at the 1959 Conference of the International Economic Association on Inflation, Robertson referred to Phillips’s (1958) conclusion that a rate of unemployment of 2.5% should be able to preserve price level stability as ‘very favourable to the optimists’ (Robertson quoted in Hague 1962: 456). In Robertson’s opinion, the original element in Phillips’s article was not the negative correlation between the rate of growth in money wages and the rate of unemployment, but rather its contention that the relationship between these two variables was stable. Robertson noted that ‘one could not put much reliance on the results because it assumed there was a fixed psychological function relating the attitude of trade unions to the level of employment over a whole century’ (ibid.). Interestingly enough, as Blyth (1975: 306) notes, Robertson (while at the LSE) played a role in influencing Phillips to add the determination of prices and wages to the traditional Keynesian model of the early 1950s. Even more important, Robertson saw no proper empirical, or theoretical, basis for the progressive reductions in estimates of full employment, and believed that cost-push pressures only occurred because of the excessive level of demand, and that incomes policies could not work in such conditions. He had the courage to argue (in the Cohen Council 1958) that the equilibrium (NAIRU) level of unemployment was somewhat over 2%, not only higher than the then estimates of most Keynesians, but also higher than previous outcomes. For such counter-revolutionary views, he was excoriated by the *bien pensant* socialists of the day, including many in the Cambridge Faculty of Economics. Currently, however, when central banks worry about whether the NAIRU might be anywhere between, say, 4% and 6%, and any suggestion to adopt an incomes policy would be laughed out of court, it is patently obvious that Robertson won this larger battle. Mainstream macro is now Robertsonian, rather than Keynesian.

Robertson believed in the relative stability of the velocity of money, and was a sceptic about the stability of the consumption function. In what was known as the ‘battle of the airwaves’ between Friedman and Meiselman (FM) and Ando and Modigliani (AM), he would have been one of the few British economists to take the side of FM. Indeed, he adopted at one time, or another, almost all the analytical parts that lay behind Friedman’s monetarist counter-attack on the Keynesian position. Yet he never quite put it all together in that way, perhaps because he saw himself as Blondin, the

tightrope walker, keeping a balance between different schools of thought, all of which had merit (Robertson 1938 [1940]).

A possible contributory factor might have been a slight hesitance, or uncertainty, about the determinants of the supply of money. Robertson is crystal clear that loans create deposits, and in some cases, he leaves it at that, for example, in his address on ‘Is There a Future for Banking?’ (Robertson 1952: Chapter 14). But then the question arises as to what limits the expansion of bank loans? In a slump, it often is the demand for such loans; analogies arise about taking horses to water. However, in a boom, Robertson sees the limiting factor as the availability of cash reserves, and he adopts, though mostly in words, the money multiplier analysis, which was becoming standard. At the same time, Robertson is aware of a problem: The banks can always get more cash by (forcing the discount market into) borrowing from the Bank of England, admittedly at a penalty rate, but one that is not much higher than the policy rate. Interest rates will then rise, but not by much unless the Bank is prepared to raise the whole structure of (short) rates significantly.⁶ This problem became even worse after the Second World War when British banks were stuffed full of saleable short-term government debt, with the government being a largely interest-insensitive borrower.

Robertson was one of the few to recognise some of the flaws in monetary management by means of ratio controls. Thus:

If a proportion fixed by custom is arbitrary and misleading, a proportion fixed by law seems at first sight to be positively mischievous. An iron ration which you must not touch even in the throes of starvation is something of a mockery. Against such criticism it may be urged (though not too loudly) that in finance as in war rules are made to be broken on occasion, and that their object is not to ensure that certain things shall never be done, but that they shall not be done without good reason (Robertson 1948: 57).

And, ‘...it resembles the procedure of a certain municipality which tried to guard against a shortage of cabs by ordaining that there should always be at least one cab on the ranks’ (ibid.: 62). One of the authors of this paper has reused this analogy on numerous occasions, without having been fully aware of its provenance with Robertson.

⁶From *Money* (1948: 162–163): ‘But on these difficult matters the reader may well be excused from seeking further enlightenment in this little book, and the author from trying to impart it. It is enough to realise that by the use of their double weapon, Central Banks can up to a point check the expansion of the money-supply [sic]; and that while we cannot be sure that their power to do so is in all circumstances complete, there is good reason to believe that if it were used earlier and more resolutely than it has sometimes been in the past, many of the evil excrescences of a trade boom could be lopped away’.

Interest rate fluctuations play a relatively minor role in *A Study of Industrial Fluctuation, Money, and Banking Policy and the Price Level*; indeed, remarkably so in the latter two books. Whereas productivity and thrift play a fundamental role in determining the quasi-equilibrium longer-term real interest rate, in practice, short-term factors, such as gold flows, changes in bankers' confidence, and shifting expectations of future inflation, could easily drive actual market rates away from the equilibrium level.

It is, therefore, somewhat ironic that Robertson's best-known clash with Keynes was over interest rate determination, the loanable funds versus liquidity preference debate. As in the case of price stickiness, it was triggered by Keynes in his attempt to differentiate his product from what had gone beforehand, even though he himself was arguably going too far. First, Keynes argued that saving was, almost entirely, a function of income, and hardly, if at all, of interest rates, and that investment was (empirically) rather insensitive to interest rates. As a result, interest rates could not be determined by forces causing *ex post I* to equal *ex post S* at full equilibrium (or in other words, the Hicksian IS curve was very inelastic).

Second, Robertson, supported initially by Keynes, had argued that the demand and supply of money were equilibrated by changes in the general level of prices, the quantity theory of money. But now Keynes had shifted towards claiming price (or money-wage unit) stickiness so long as employment was below full employment. If so, then what would equate the demand and supply of money? Keynes's answer, of course, became the liquidity preference theory. The extreme version of the liquidity preference theory could not, and did not, last, partly under fire from Robertson. Keynes was obliged to accept a 'finance' motive for holding money, and Hicks, with his IS-LM analysis, brought about a partial reconciliation.

Robertson was not fully reconciled with the IS-LM analysis, even though it did bring back productivity and thrift into the analysis of the determination of interest rates, from which Keynes had once hoped to banish it. Even so, it made the adjustment of the economic system to disturbances to supply-side monetary shocks occurring primarily via interest rates, whereas Robertson would argue that the money/bond margin was but one margin out of many; so supply-side monetary shocks could impinge the economy directly and not just via interest rates (see, for example, his paper on 'What Has Happened to the Rate of Interest?') (Robertson 1949 [1952]).

Another, more policy driven, concern of Robertson's was that the Keynesian analytical apparatus was used, primarily after the Second World War, to justify using fiscal policy to maintain full employment, while holding interest rates down as low as possible (Dalton) in order to encourage investment, to

lower the debt burden (on government), and, perhaps, to push the rentier into euthanasia. In part because of Robertson's belief in the quantity theory, he believed that this line of policy would become inflationary, distortionary, and ineffective. His later, post-Second World War writings are full of pleas both for a higher general level of interest rates and for a much more aggressive counter-cyclical variation in them (see Dennison and Presley 1992).

To conclude this section and sum up, Robertson's central analytical concept of forced saving following flex-price inflation was discredited when Keynes switched from a flex-price to a fixed-price adjustment mechanism. But wait long enough into the medium term and almost all prices regain a flex-price character. In this latter context, the balanced wisdom of Robertson/Blondin has eventually, albeit post-mortem, triumphed over the initial excesses of the Keynesians. Modern macro is primarily Robertsonian (rather than Keynesian), except that the IS-LM (now three equation) model still has money supply shocks channelled primarily via the interest rate, whereas Robertson would have preferred to revert to a more general quantity theory mechanism.

4 Robertson and the Cambridge Approach to Utility and Welfare

In the post-Second World War era, Robertson attempted to defend the Cambridge utilitarian tradition against the 'new welfare economics', developed earlier following Robbins's influential criticism of the legitimacy of interpersonal comparisons of utility. Robertson claimed that welfare economics *should* be based on cardinal utility while rejecting the ordinalist revolution in consumer and welfare theories.

A main feature of Cambridge welfare economics uses the acceptance of interpersonal comparisons of welfare, though not necessarily associated with cardinal utility measurement. Robertson (1951 [1952]: 17, 38) saw himself as part of the 'Cambridge set up', coming from Marshall, the 'Cardinal Club', and the 'Pigouvian world of measurable utility'. As both Robertson and Pigou (1953: Chapter 5) were aware, Marshall's cardinalism was nuanced since he insisted that the utility of sensations cannot be measured directly, but only indirectly by their observable effects. This notion of utility as a 'mental metric' of desire was common to Marshall, Pigou, and Robertson, as noted by Sen (2000: 67). Its core assumption, according to Robertson (*ibid.*: 15, 1957: 72–73), was the psychological rule of diminishing marginal utility, based on 'introspection and observation' and interpreted as a corollary of the idea that

utility is quantitative and measurable. Along with the requirement that utility is additively separable, diminishing marginal utility implies strict convexity of Edgeworth's indifference curves. The Marshallian consumer is supposed to have a cardinal capacity of knowing the rate at which marginal utility declines, a knowledge that enables her to maximise her economic welfare or utility by distributing expenditure so that the marginal utility of a good is proportional to its price. This does not mean that marginal utility can be directly measured, but that the price a consumer pays for each good is an indirect measure of its marginal utility.

Robertson began with an examination of Pareto's claim that lumps of utility can be set out in *order* of magnitude, but that one cannot ask *how much* greater one lump of utility is than another (Robertson 1951 [1952]: Section 2). However, Pareto and his followers did not consistently adhere to the postulate of ordinal utility, since the notion of marginal utility was still present in their assumption about the signs of the second derivatives of the utility function. Apart from the convexity assumption, Robertson also criticised the methodological principle of Occam's razor. Although the theory of demand could be presented in an objective and behaviouristic fashion (as in Chapter 4 of volume 1 of the *Lectures*), Robertson did not accept the view that consumer theory should merely *describe* choices rather than explain them (see also Robertson 1954a).

Robertson's attempt to protect Cambridge from the criticisms of new welfare economics was based on his view that the replacement of the law of diminishing marginal utility of income—which made possible interpersonal comparisons of utility—by compensation criteria and the social welfare function did not represent progress. His defence of cardinal utility was not restricted to welfare economics, but included also demand theory, since ordinal utility would not provide a better account of consumers' behaviour, especially the crucial assumption that indifference curves are convex towards the origin, associated with his emphasis on material welfare as the proper domain of economics. Robertson had some influence over Cambridge students at the time, including Amartya Sen, who arrived at Trinity College in 1953. As recalled by Sen, his teachers at Trinity, Dobb, Sraffa, and Robertson, were his main influence:

And then Robertson was very important. He presented good, critical reasons for being sceptical of behaviourism, including the notion of revealed preference that had by then taken hold of economics. The idea that we can understand human beings in terms only of their behaviour, and then only their non-verbal behaviour, never through conversation...was very alien to the Marshallian part

of the Cambridge tradition, a tradition I came to admire a lot. The natural heir to that tradition in my student days was Dennis Robertson (Sen quoted in Klamer 1989: 138).

5 Conclusion

Dennis Robertson had a long and fruitful career as a Cambridge economist, since his student days in the 1910s up to his retirement in the 1950s. Marshall's overall influence is visible throughout Robertson's work in monetary economics (the Cambridge approach to the quantity theory of money, the loanable funds view of the determination of the interest rate, etc.) and in his contributions to microeconomics as well (the theory of the firm, utility, and demand). Indeed, Robertson's *Lectures* may be seen as the last Marshallian textbook. But in his contributions to monetary dynamics, Robertson went well beyond the orthodox Marshallian research programme, even if, in contrast with Keynes, he always regarded himself as walking along the footsteps of the old master. This is related to Robertson's character, who saw the evolution of economic theory as a continuous process instead of a succession of 'revolutions' and abrupt changes. Robertson's character caused him some problems at Cambridge in the 1950s, when he had a hard time getting along with Keynesian economists Joan Robinson and Richard Kahn (see Harry Johnson's 1978 recollections of his period at Cambridge). With the exception of his 1926 difficult book, Robertson excelled as a 'charming writer', who 'would sneak up on the unwary reader and gain his acquiescence by a siren song' (Samuelson 1963: 518).

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26

(Edward) Austin (Gossage) Robinson (1897–1993)

G.C. Harcourt

1 Introduction

Austin Robinson was born on 20 November 1897, at Farnham in Surrey, the eldest son of Albert Robinson, ‘an impecunious clergyman’, and Edith Robinson (née Sidebotham), the daughter of a clergyman. It was a happy marriage; they had four children who had a happy childhood. Their upbringing fostered self-reliance, fun, and games as well providing an introduction to a sense of duty and the practical application of Christian principles.

Through scholarships, Austin went to Marlborough where he read Classics and then obtained a scholarship to Christ’s College, Cambridge (his father’s college) in 1916. Before taking up his scholarship, he served in the Royal Naval Air Service, training as a pilot of sea planes which he loved. He went up to Cambridge in 1919. The First World War with its appalling loss of life and gross injustices had a traumatic effect on Austin’s generation at Cambridge, though he was never a pacifist ‘in the technical sense’ (Austin Robinson 1992: 204).

In writing this chapter, I have drawn on my Memoir of Austin Robinson for the British Academy, published in *Proceedings of the British Academy* (Harcourt 1997); my Kingsley Martin Memorial Lecture, ‘Two Views on Development: Austin and Joan Robinson’, published in the *Cambridge Journal of Economics* (1998: 367–377) and my entry on Austin in D.A. Clark (ed.) *The Elgar Companion to Development Studies*, Cheltenham, UK; Northampton, USA: Edward Elgar (2006: 60–67).

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Austin obtained a First in Classics after 15 months and then went with relief to read economics. (Hearing Maynard Keynes's lecture on what became *The Economic Consequences of the Peace* (Keynes 1919 [1971]) had a major influence on his decision.) C.R. Fay's supervision produced ferocious debates, forcing Austin to make explicit and coherent arguments. When Fay went to Canada, Austin was supervised by Dennis Robertson and Gerald Shove. Though he was well instructed by two powerful minds, supervisions never again had the same magic.

Austin graduated with another First in 1922. He began research at Corpus Christi, which was then renowned for its unique brand of High Church and High Toryism. Arguments with intelligent colleagues with very different views on economic and social matters were of inestimable value to Austin, especially after he became a Fellow in 1923 and had to teach as well as to argue and understand. By 1925, he had moved from lecturing on Money, Credit, and Prices, with which he was never happy, to what was and remained his favourite subject, Industry (Cairncross 1993: 17–18).

He married Joan Maurice, who had been his pupil, in 1926. Soon after, they went to India for nearly two years where Austin tutored the young Maharajah of Gwalior. The experience kindled his (and Joan's) lifelong love of India and its people and introduced them to the problems of development in a most practical way. Austin contributed a first-class piece of applied political economy to *The British Crown and the Indian States* (1928). Drawing on inadequate statistics, he showed judgement and imagination in estimating fiscal flows to and from the Princely States to British India.

The Robinsons returned to Cambridge in 1928. Austin started afresh his long academic career there, only seriously interrupted by distinguished service in Whitehall during the Second World War. He became a University Lecturer in 1929 and a Fellow of Sidney Sussex in 1931. Austin was appointed to a Chair in 1950, retiring in 1965, only to continue an extremely varied and intensive career until his death in 1993.

Keynes had known Austin when he was an undergraduate—oral tradition has it that Keynes regarded him as 'his brightest student'. He asked him in 1934 to be Assistant Editor of the *Economic Journal*. This was the start of 60 years of service in all to the Royal Economic Society (RES)—Editor of the journal for 36 years, Secretary of the Society for 25 years. He was a member of the editorial board that supervised for the Society the publication of the 30 volumes of the *Collected Writings* of Keynes; indeed, he was judged to be 'the moving force getting Keynes's writings collected and edited' (Hahn, 6 June 1994, quoted in Harcourt 2001: 150).

Increasingly in the post-war period, Austin was drawn towards the problems of developing countries. He was also an indefatigable founder of and

worker for the International Economic Association (IEA), of which he was Treasurer (1950–1959), President (1959–1962), and General Editor of its volumes (1950–1980). Austin edited or co-edited 12 volumes of its conferences. The bulk of these were concerned with development issues, and to all of them Austin made lucid, carefully considered contributions. Ken Arrow and Tony Atkinson (July 1994, personal correspondence) noted that Austin thought small conferences generated the most valuable discussions but in order to guard against exclusiveness, he urged the need for regional conferences. ‘As General Editor, he wielded his pencil forcefully’—the pay-off was the academic quality of the conference volumes.

In the following sections, I discuss Austin’s two classic volumes in the early industrial organisation literature; his early work in the 1930s on development through Christian institutions; his wise words on *The General Theory*; his wartime experiences; his roles in the IEA and his contributions to the economics of developing economies in the post-war world; and his roles in the Cambridge Faculty of Economics and Politics and in Sidney Sussex. The concluding section sums up the contributions of Austin whom Alec Cairncross, his biographer, dubbed ‘an Economic Advisor’ and whose admiring Cambridge colleagues regarded him as the role model par excellence for aspiring applied political economists.

2 School and Early Cambridge Days

At Marlborough, Austin was ‘rigorously drilled’ in classical grammar by an eccentric schoolmaster, A.C.B. Brown. This allowed him to jump all the necessary hurdles but it dimmed his enthusiasm, so that his heart was never completely captured. It did ensure that Austin wrote in a distinctively agreeable style in his books, articles, reviews, and letters—he was a prolific writer of letters which were noted for their lucid elegance as well as their substance.

When Austin went to economics, Fay, who had persuaded Christ’s to allow him to do so, lent Austin the Marshalls’ *Economics of Industry* and R.H. Tawney’s *The Acquisitive Society* to start him off. Austin read them, Frank Taussig’s *Principles* and Marshall’s *Principles* during the day in the summer of 1921, while working each evening as ‘poor-man’s lawyer’ in the dockyards of Liverpool, getting ‘a remarkable education regarding the life and problems of the poor’ (Austin Robinson 1992: 205). Austin found Fay an enthusiastic supervisor who was sublimely uncomprehending of the economic theory to be found in Marshall (much as Fay worshipped Marshall himself).

Back in Cambridge after India, Dennis Robertson asked Austin to write the book on *Monopoly* for the respected Cambridge Economic Handbooks

series. Austin ended up writing two books (1931, 1941) as he cleared the ground for *Monopoly* by writing on *The Structure of Competitive Industry*, a project which became a book in its own right. Its *Economic Journal* reviewer, Philip Sargant Florence (to whose Lectureship Austin had been appointed when Sargant Florence went to the Chair at Birmingham), rightly praised it as the potential classic it was to become—‘a most original contribution... lively style... obvious... fund of industrial experience to back it [up]’ (Sargant Florence 1932: 66). He was as complimentary about *Monopoly* when nearly a decade later he was again the *Economic Journal* reviewer (perhaps as Review Editor, Austin wanted to see how Sargant Florence reacted second time around). Sargant Florence gave *The Structure of Competitive Industry* both high praise and stringent criticism. The praise was for the excellent structure of the argument which gave outstanding unity to the book. Austin looked at the optimum size of firms from a number of points of view—technique, management, product(s), marketing, for example—then brought all these aspects together, reconciled in the size of a real firm. The criticism related to a fuzziness of definition, that in much of his argument, it was not clear whether Austin was referring to plants’ ‘scale of operations’ or to firms’ ‘scale of organisation’.

Nevertheless, Austin’s work essentially established in an excellent way in Cambridge what we now can call industrial organisation. He blended together a judicious mix of theory, facts, and policy—always his approach to economic issues—thus deserving ‘the gratitude of all who wish to bring description closer to theoretical economics’ (ibid.: 69). His reviewer had one main criticism of *Monopoly*, that Austin confused the difficulties of creating a monopoly and circumventing competition with those of controlling an established monopoly. Despite this, Sargant Florence felt the author managed ‘to pack in most of the real world of monopoly while arguing all the time patiently from first principles’ (Sargant Florence 1941: 483).

3 Christian Beliefs and African Economic Development in the 1930s

Austin’s Christianity and his interest in development came together when in the 1930s he took part in two major studies of African problems, the first of which required him to visit what is now Zambia. The Archbishop of York asked A.C. Pigou in 1932 to suggest someone to join a commission of enquiry under the auspices of the International Missionary Council, to spend six months in Africa analysing the impact of copper mining on indigenous

society. Cairncross (1993: 51) says Austin's chapters in *Modern Industry and the African*, Davis (1933), 'constitute one of the first attempts by an economist to arrive at a view of what makes for successful economic development in a backward country'. There, Austin used the new ideas that were emerging in Cambridge as Keynes moved from *A Treatise on Money* (Keynes 1930 [1973]) to *The General Theory* (Keynes 1936 [1973]), spurred on by the criticisms and suggestions of the Circus, of which Austin was a key member.¹ They gave him the rudiments of a national accounting framework in which to think about structures and imbalances as between rural and urban sectors, overseas trade and development, and the impact of government expenditure and taxation on economic systems.

Austin was also always interested in individuals as such (and their groupings); so, as he thought about rural underemployment and poverty, he was keen to use the potential skills and aspirations of people *where they were*, rather than advocate large migrations or the creation of huge urban concentrations. In his letters, he wrote much about the characteristics of the Africans with whom he came into contact, using as his numeraire the various groupings of Indians he had known and/or observed in the 1920s. He wrote reflecting first impressions: 'In India where servants are perfect we say "This is evidently a servile race. They can't rule themselves". In South Africa we say "These people can't even lay a table. How can they run a country?"' (Austin Robinson quoted in Cairncross *ibid.*: 55). For Austin, economic development had to build on the characteristics of the people as they were, or would become, and he was what we now call a 'horses for courses' person. He was always suspicious of all-purpose general theories and their accompanying models which were thought to be applicable regardless of time or place. He said of the Cambridge developments of the years associated with Keynes and his colleagues:

It was... a great step forward in economic thought when Keynes insisted that we should have... a theory that was valid not only with full (or near-full) employment, but also with unemployment—and that we should know quite clearly which of the propositions of economics were universally valid, and which were valid only in conditions in which it might be true that an increase of one activity was possible *only* at the expense of another activity (Austin Robinson 1947: 44; italics in original).

¹ The Circus was a group of young economists—Austin and Joan Robinson, Piero Sraffa, Richard Kahn, and James Meade—who met to discuss *A Treatise on Money*. Their deliberations were usually reported to Keynes by Kahn who then conveyed Keynes's reaction to the Circus members.

His other work on Africa in the 1930s did not require him to go there but it was nevertheless a major contribution, two long chapters, 157 pages in all, in Lord Hailey's *An African Survey* (1938). Austin spent the vacations of the three years 1934–1937 working in Chatham House on the chapters. The *Survey* itself was set up in response to an appeal by General Smuts in 1929—he called for a survey of Africa's affairs as a whole, reviewing developments in each country and to what extent they were affected by and gained from modern knowledge. Austin drew on the work of S.F. Frankel on capital investment in Africa and Charlotte Leubuscher on African foreign trade for the external aspects (Chapter 19), and on Hailey's own 'immensely conscientious' notes for the internal aspects (Chapter 20). The quality of Austin's chapters was such as to give the 'chapters a place amongst the classics of economic literature' (Hall quoted in Cairncross *ibid.*: 73).

4 Austin's Response to *The General Theory*

Austin increasingly assimilated the new lessons Keynes was developing, so much so that he was to review *The General Theory* (Keynes 1936 [1973]) for *The Economist* (29 February 1936), the only ever signed review in that journal (and then it was initials only, E.A.G.R.). Austin's review was given a title of which he disapproved (it was misleadingly—because far too narrow—called 'Mr Keynes on Money') and also editorial cuts may have altered its emphasis and balance. When Austin complained to Keynes of this, Keynes said it served him right for publishing in the yellow press. The review was perceptive and accurate as to the essential nature of the new theory. It could be read with profit today by modern students to allow them both to get the essence of the theory and of how the advanced world still works. Austin's classical training was in evidence. Of Keynes's polemical passages, he wrote: 'Like Horace's schoolmaster, Mr Keynes whips his pupils into agreement, where modest reasonableness, many will feel [not Austin, though], would better have achieved this end' (Austin Robinson 1936: 472).

What is illuminating, considering the muddled debates that were to occur, was that Austin had a clearer view of the meaning of the equality of saving and investment and the roles which it played in the analysis than perhaps even the author himself. He refers also to Keynes's masterly and clear style in previous writings and deplors its comparative absence in *The General Theory*: 'Many will sigh for the earlier Keynes who possessed in unusual bounty the gift of translating theoretical ideas into realities and conveying them in words of one syllable' (*ibid.*). Austin himself uses plain language to good effect to describe the existence of the underemployment rest state and the process by which it

may (or may not) be reached in the economy as a whole. His keen sense of industrial organisation is evident when he explains that the non-profitable levels of output as a whole away from the rest state mean that the positions are not sustainable, even in the short term.

5 Austin and Whitehall

Austin spent the war years in Whitehall, working in two different sections. He went first to the Offices of the War Cabinet, subsequently joining the Economic Section when it, and what became the Central Statistical Office, was set up. Austin came to Whitehall much impressed by Keynes's talk to the undergraduate Marshall Society in Cambridge on the issues contained in *How to Pay for the War* (Keynes 1940 [1978]). It also convinced him of the fundamental need for reliable estimates of national income and expenditure on a continuing basis. These were to be provided by Austin recruiting James Meade 'to get the logic right' and Richard Stone for his 'remarkable familiarity with British economic statistics' (Cairncross 1993: 79). Cairncross tells us that Austin 'always regarded [getting] the annual national income accounts on a consistent basis as his chief contribution to the war' (ibid.). In February 1942, Austin became the Economic Advisor and Head of the Programmes Division in the Ministry of Production. The lessons he learnt in these two sections he regarded as the most important elements in his long apprenticeship as an economist (Austin Robinson 1992: 219).

His wartime tasks and experiences reaffirmed his belief that macroeconomic analysis without simultaneous attention to the microeconomic details of firms and industries, supplies of specific types of labour and capital goods, and of infrastructure, is seriously flawed. As someone who had absorbed Marshall very deeply, Austin always connected together the long-term development implications of short-term changes and vice versa.

After the war in Europe ended, Austin went to Germany as a member of a small committee on how Germany should be treated in the post-war era. He kept a diary which was 'remarkably lucid, coherent and perceptive [conveying] a remarkable picture of the contrasts between town and country, occupiers and occupied, movement on the roads and inertia elsewhere, devastation and disorder on the grand scale but some things still working normally and in good order' (Cairncross ibid.: 91). In a letter to Keynes of 16 June 1945, Austin wrote: 'Fact, cold hard fact, is almost certainly different [but he] preferred [his] stories, and as the theologians say when pressed too hard, the story may convey the picture without being literally true' (Austin Robinson 1986a: Preface, no page number). Austin went

to Russia where he emphasised perceptively ‘the complete ascendancy of defence over opulence in the mind of the Communist government—an ascendancy that continued throughout the postwar years in a measure unequalled anywhere else’ (Austin Robinson quoted in Cairncross 1993: 94).

Austin drew on his wartime experiences 20 years later when in his Marshall Lectures of 1965, *Economic Planning in the United Kingdom: Some Lessons* (published in 1967), he set out what is still a blueprint for policy making in a free society which is nevertheless determined to employ all its citizens and direct its overall development in the long term as well as in the short term. He returned to the same themes in his review article (Austin Robinson 1986b) of Cairncross’s account of the transformation from war to peace (Cairncross 1985). As well as playing a key role in manpower planning during the war, Austin was also involved in the determination of the import needs and export possibilities of the UK in the post-war period. Though Cairncross applauded his general approach, he thought Austin was too pessimistic about the possible outcomes in his detailed estimates of what was possible and needed.

Austin was never persuaded on this and as late as 1986 pointed out that the original estimates, made in 1943, were made on the assumption that the war against Japan after Germany was defeated would be a long, drawn-out affair, 18 months to two years or more. Dropping the atomic bombs in August 1945 drastically shortened the relevant time period and brought forward the beginning of the transition. Austin argued that they had identified the main problems: the balance of payments where exports were no more than 28% of their 1938 volume. There were shortages of steel, timber, coal, and energy generally, and also of certain labour skills. Cairncross summed up: ‘[W]hen the risks are high, as they were in 1947, it is not the outcome that is the best measure of a man’s judgement but how the risks seemed to good judges at the time, and there were few who foresaw a future materially more fortunate than [Austin] did’ (Cairncross 1993: 108).

Austin returned to university life after the war, feeling that he was not ‘tough enough to carry on indefinitely under the pressure [he] had worked during the [war] years’ (Austin Robinson 1992: 218). His reputation was such that Whitehall and the government would not let him go completely. Twice for extended periods he was called back at Chancellor of the Exchequer Stafford Cripps’s insistence. He spent a year in London helping to draft the *Economic Survey for 1948* and the *Economic Survey for 1948–1952*, which involved six months in Paris with the Office of the European Economic Community (OEEC) ensuring that the Marshall Plan could go through. He chaired ‘the committee that drafted the collective report to Congress, showing that we collectively had plans that would make us viable’ (ibid.: 219). With that task

done, his ‘long apprenticeship’ ended, he was on ‘the threshold of a subsequent 40 years as an academic’ (ibid.). Nevertheless, he kept his links with government and government service for many decades afterwards; he served on selection boards for the civil service and through the National Institute of Economic and Social Research (NIESR) and development agencies, he influenced advice given and personnel chosen. His scholarly contributions were recognised by his election as a Fellow of the British Academy in 1955.

6 Austin and the IEA

Cairncross (1993) cites the IEA volumes either edited by Austin or to which he contributed chapters in his bibliography of Austin’s writings as evidence of Austin’s lucid, carefully considered contributions to the problems of developing economies. A selection of the titles alone indicate the breadth of Austin’s interests and knowledge: *The Economic Consequences of the Size of Nations* (1960); ‘Foreign Trade in a Developing Economy’, a chapter by Austin in Kenneth Berrill (ed.), *Economic Development with Special Reference to East Asia* (1964); *Problems in Economic Development* (1965); *The Economics of Education* (edited with John Vaizey 1966); ‘The Desirable Level of Agriculture in Advanced Industrial Economies’, a chapter in Ugo Papi and Charles Nunn (eds), *Economic Problems of Agriculture in Industrial Societies* (1969); *Backward Areas in Advanced Countries* (1969); *Economic Growth in South Asia* (edited with Michael Kidron 1970); *The Economic Development of Bangladesh within a Socialist Framework* (edited with Keith Griffin 1974); and *Appropriate Technologies for Third World Development* (1979).

His commentaries were always clearly expressed, he combined optimism tempered with caution, and he tried to delineate clearly the boundaries within which academic economists could speak with (relative) authority and outside of which they were trespassing without good reason. Thus, in the 1960 volume (which arose from a conference held in 1957—as with the effects of changes in the quantity of money, the publication of IEA volumes is subject to uncertain, long, and variable lags), he wrote that it was ‘not for us, as a group of academic economists, to reach political conclusions, and we made no attempt to do so’ (Austin Robinson 1960: xxi). The subject of this particular conference—the relation of size to economic prosperity—had, its editor wrote, received very little discussion in the 180 years since the publication of *The Wealth of Nations*. Typically, Austin started by getting definitions straight and asking why the concept of a nation was relevant for economic analysis. He found the answer in the discontinuities which the boundary of a nation

provides—some natural, some institutional, for example, tariffs, limits on the movement of labour. In our day (Austin's then), the nation had renewed itself because it had become the unit for government action and economic activity.

Austin pointed out that the definition of size differed according to the purpose in hand. At the conference, they examined the USA (a rich country) and Switzerland, Belgium, and Sweden (which were exceptions to the size rule). Austin noted that Switzerland achieved necessary economies of scale by relying on export markets, while Belgium achieved high living standards by concentrating on the unfashionable factors of industrial efficiency and hard work (both dear to Austin's heart). He pointed out that with few exceptions technical economies are exhausted by firms of quite moderate size. He also formed the impression that most of the major industrial economies of scale could be achieved by a relatively high-income-per-capita country with a population of 50 million. Foreign trade could provide an escape (from size) but a precarious one and the economic arguments for further integration of nations, so as to create wider markets, were not overwhelmingly conclusive—the political arguments were, of course, another matter. Size was obviously useful for defence but not exclusively for anything else.

Austin's chapter on foreign trade in developing countries in Berrill's 1964 IEA volume started with a list of intellectual debts: Ragnar Nurkse, Harry Johnson, Berrill himself, David Bensusan-Butt, Hla Myint, and Phyllis Deane. He first identified two impacts of international trade on the development process. The first was positive: by adding specialisation and accumulation in those activities in which productivity is highest, the process of development may be accelerated. The second, which was negative, arose because often the propensity to import runs ahead of the power to export, so imposing constraints associated with threatening balance of payments difficulties. If higher rates of interest are used, for example, to protect foreign exchange reserves, they may lead to an uneasy equilibrium characterised by underloading of the economy and a slow rate of development. Though the ratio of exports to imports reflects in the very long term the size of country concentration and range of endowments, the exports to income ratio is the ultimate constraint, a point which Austin illustrates by reference to the historical experience of the UK and Japan. A typical Austin emphasis is that the better use of resources may often have been more important than a slightly higher rate of accumulation.

He lists five channels of causation whereby a rise in the exports to income ratio may contribute to the acceleration of growth: by a transfer of resources from low to high productivity areas; by ridding any industry of dependence solely on home markets (but if this is achieved by foreigners' investing and

producing, the benefits to the home country may be minimal); by the spread of higher industrial efficiency first introduced through international trade; by what we now call the demonstration effect, knowledge of new products, or products not previously known in the country leading to increased desires to produce them and for increased incomes to purchase them. The most important aspect for Austin though is that a high level of trade and possible imports provides a means of escape from both major and minor errors of planning and production. He illustrated these principles by looking at the experiences of India and Pakistan. An important emphasis that emerged was that he was sceptical of the potential of price changes, for example, devaluations, as opposed to the power of income and quantity changes.

In his opening address to the Second World IEA Congress in Vienna in September 1962, the subject of which was the problems of economic development, Austin said that the topic was chosen deliberately, adding: 'Just as in the 1930s almost all schools of economists were concerned with problems of economic fluctuations...today [they were] mostly concerned with attempting to understand the causes of economic growth' (Austin Robinson 1965: xv). Austin expressed the wish that these developments would help to eliminate poverty which does so much damage to human happiness and that they would help to close rather than to widen the gap between the poor and the rich nations. He referred to the profound difficulties associated with defining and measuring the stock of capital goods in a world of continually changing prices and technologies, adding that even more insoluble problems arise when we try to define and measure stocks of scientific and engineering knowledge or of freedom of opportunity—all variables which complement one another in the development process.

He criticised Walt Rostow's (then) attempts to generalise historical experiences of rapid growth in more advanced economies and to apply this directly in policies for 'backward countries'. For Austin (as for Marshall), change is continuous, not abrupt, that is to say, in general there is no 'take-off'. Nevertheless, to increase the speed of development attention must be paid as much to creating the right institutions and economic framework as to potential supplies of capital. Especially vital is education to allow developing countries to absorb knowledge and skills. Reflecting the influence of Keynes and his followers, Austin referred to the need to understand the causes of fluctuations in the prices of primary products and to devise schemes to reduce them. Austin returned to the role of foreign trade in development, to export-led growth and balance of payments constraints. He stressed the need to model interrelationships between countries, taking explicit note of the sizes of price and especially of income elasticities of exports and imports. Austin urged that,

in order for small emerging countries to escape from the penalties of smallness, markets be opened to both their traditional and newly emerging exports, even manufactures—still a tract for our times.

The quantity and quality of the population of nations were always foremost concerns of Austin's. He gave explicit voice to it in the volume on *The Economics of Education* (1966) which he edited with John Vaizey, a pioneer of the subject in the UK. In the introduction, Austin itemised the conceptual difficulties and the deficiencies of the available statistics. He was also careful to show that education was gravely misconceived if viewed solely (or even at all) as a consumption good. In these days of consumer sovereignty in all things, it is refreshing to be reminded that investment and production are vital aspects of economic and social life as well, and that while a balance must be struck, neglect of any is detrimental to human welfare. Austin has wise things to say about taking into account the future effects on activity of the stocks of educated persons as well as analysing the current flows, and that in our statistics, we neglect the collection of data on the educational attainments of immigrants and emigrants at our peril.

In 1969, Austin edited a volume on backward areas in advanced countries. All advanced countries have such areas; one reason why they persist is because individual entrepreneurs cannot be expected to take into account all the factors which from a national point of view are relevant for the location of industries. Austin was (and remained) an unrepentant interventionist. He argued that with the possible exception of the USA, people were not indifferent to where they live or have lived. It followed that the principles of international trade rather than the analysis of a single country were appropriate for considering backward areas and what may be done about them.

Appropriate Technologies for Third World Development (1979) was a topic especially suited to Austin's humanitarianism and 'nuts and bolts' philosophy. All his working life, he emphasised that development on the spot using already established communities was most to be preferred. Promoting the appropriate technologies for such a process had been hampered by artificially cheap capital facilities, tax holidays, and similar measures. He also stressed that there are appropriate products as well as methods of production, very much a close-to-the-ground view which reflected his frustrated engineer side²—as did his emphasis on the crucial role which the ability to provide adequate maintenance of machines plays in the process of development.

² Christ's had been 'unsympathetic to (Austin's) view that he should use a classical scholarship to be taught the more professional aspects of designing aeroplanes' (Austin Robinson 1992: 204).

Other volumes which he edited relate to developing countries such as Sri Lanka and Bangladesh, on which we comment below. Austin also wrote many reports on development themes. Cairncross singled out for special praise a report for the United Nations Development Programme (UNDP) Robinson (1976), which Austin wrote in the mid-1970s at the request of I.G. Patel (who had been his pupil in the 1940s). Cairncross regards it as the single best and most impressive account of the principles of development to come from Austin's pen. We discuss now its main features, drawing on Cairncross (1993: 150–152), features already present in embryo in his 1920s work in India and 1930s work in Africa.

His focus was on 'the massive underemployment and unemployment in many developing countries' (*ibid.*: 150). Austin asks why they are so persistent and he sets out six constraints on a policy of increasing demand to draw these workers into employment and allow incomes to rise.

The usually dominant constraint is the failure of domestic food production to match expanding incomes, so that import demand rises. Unless exports match this, expansion is constrained by balance of payment problems. Austin's orders of magnitude for a typical developing country with population growth of 2.5% a year and a target growth rate of 7% a year are that the constraint will bite if agricultural output does not grow by 5% a year. Top priority must therefore be given to overcoming this constraint by creating the necessary agricultural surplus.

Austin also stressed that the 'weakness in the exchange mechanism between town and country was sometimes the main constraint' (*ibid.*: 151). Undernourished farm workers consumed the additional food so that the demands of the urban population, swollen by an inflow from rural area, went into imports; hence effective organising for buying, financing, transporting, and distributing the agricultural surplus was needed in the city. As befits an economist of the same university as Malthus, Austin also recognised the need to limit the import content of consumer goods, not least 'luxury' goods.

The fourth limitation was inadequate accumulation due to low saving rates, inefficient methods of finance, and also the high import content of investment.

The fifth and sixth constraints are associated with the limitations of skills available—administrative as well as productive, especially in industry where education systems may not be geared to produce them. Austin thought it may be necessary to create "small-scale low-capital-intensive occupations" with "very large numbers of small craftsmen, traders, entrepreneurs starting successful small business" (*ibid.*) in order to bypass the problem.

Strangely, Austin does not mention cultural factors which could be an important part of the explanation of differences between countries, for

example, acceptance of discipline in the industrial sector: strange, because, as we have seen, his letters from India and Africa are full of details on just these characteristics of the local populations.

Austin then discussed the dual economy aspect of development—the contrast between modern sectors and traditional sectors, and the choice this raises of whether to go for rapid development through faster growth and lower capital inputs per jobs, or a gradual transition and the consequent need to ‘revitalise and reinvigorate the traditional economy’ (ibid.: 152). He had advocated the latter advance in the 1930s.

Finally, he recognised fully the problems associated with rapid population growth which in some cases meant absorbing ‘as much as three quarters of all national investment...in merely standing still’ (ibid.).

We may illustrate Austin’s approach, in particular, his well-developed sense of relevant orders of magnitude in the simple macro-development models which he carried in his head, by briefly examining the arguments of his Kingsley Martin Memorial Lecture, ‘The Economic Development of Malthusia’ (Austin Robinson 1974), which was given in Cambridge on 6 March 1974. He used Bangladesh as his example. He started by stating the question which was asked ‘[o]ne hundred and 75 years ago [by] a shy young Fellow of Jesus’ (Austin Robinson 1974: 521). The question is ‘whether economic development was possible, or whether it would be frustrated by the growth of population’ (ibid.). To say that ‘Malthus has been discredited by subsequent history’ is, said Austin, ‘a very dangerous half truth’ (ibid.), for while the advanced countries have broken through the Malthusian barrier into cumulative growth, the rest of the world has not; it ‘continues to live under conditions of near stagnation, little above the subsistence level, in very much the conditions that Malthus envisaged’ (ibid.).

Austin worked out two scenarios for the next 20 years in Bangladesh according to whether it continued with Malthusian-type birth and death rates, or with European types through which it (Europe) had broken out of the Malthusian trap. He relates these statistical exercises to the actual plans then being proposed in Bangladesh. His sense of the interrelationships of the broad aspects of the economy is beautifully done. He shows that in the most favourable scenario, a considerable proportion of the problems of unemployment, underemployment, and poverty would be overcome by the end of the period, while with the other scenario, Malthus’s worst fears would have been realised and an opportunity available then (1974) would have been lost forever. In 1996, Austin’s ‘waking hopes’ (ibid.: 532) were nearer to being achieved than his worst fears realised (see, for example, Reddaway 1996).

The distinguished development economist John Toyé (personal communication) writes that:

Austin was very influential in the early days of the UN...his criticisms led to a restructuring of the UN Department for Economic Affairs and the redesign of the UN's *World Economic Report*. [He was also] the driving force behind the setting up of the Overseas Development Institute in London. His purpose was to raise public and political interest in development issues. He served on its Council [for] over thirty years...and [his] peremptory guidance...was invaluable in keeping it on the straight and narrow.

7 Austin in the Faculty of Economics and Politics and Sidney Sussex College

In the Faculty of Economics and Politics at Cambridge, Austin not only taught but also played a major role in its administration. The building which now bears his name (it was so christened at the party in honour of his 90th birthday) is very much the outcome of his enthusiasm and persistence. As well as lecturing and supervising, Austin had long spells as Secretary of the Faculty Board and also as its Chairman. The clashes between the Keynesians and the Robertsonians were fierce and unyielding in the post-war years. Austin did his best to bring peace and maintain cohesion. James Meade, who came to Cambridge in the late 1950s and who was witness to some of the toughest debates, thought that Austin tried hard to be fair and obtain principled compromises, even if often in practice they favoured one side more than the other. In any event, Austin was faced with a virtually impossible task in a Faculty where consensus is defined as agreeing with whoever is speaking.

In September 1965, Austin retired from his Chair to have nearly 30 years more of extremely active life. He was physically frail towards the end—he was knocked off his bicycle by a motorist about 10 years before he died and injured his back. It continued to trouble him despite the efforts of a renowned osteopath who ministers, usually most effectively, to the underworld of the back sufferers of Cambridge. Nevertheless, some of his best papers were written in his 80s and early 90s. The editors of the *Cambridge Journal of Economics* often used him as a reliable, critical, but fair-minded referee. In a book published in 1984, *Economics in Disarray*, Austin's contribution, a comment on Peter Wiles on the full-cost principle, stands out for its clarity and deep economic intuition. It reflects his knowledge of firms, his exchanges in the 1950s with the full-cost theorists of Oxford, and his experiences from his

years as a Syndic of the Cambridge University Press. He also wrote his superb autobiographical essay, 'My Apprenticeship as an Economist' for Szenberg's 1992 volume on *Eminent Economists*, which, together with his obituary of Keynes in the March 1947 *Economic Journal*, most typically reflect Austin's great strengths as an economist, perceptive human being, and elegant stylist.

As we noted, Austin was elected to a Fellowship in Sidney Sussex in 1931. From then on, the College was a central focus of his life, especially after Joan died in 1983 and Austin moved from their house in Grange Road to a flat opposite the College. Roger Andrew, a former Bursar of Sidney who was close to Austin, writes (personal communication): '[Austin's] enthusiasm for the College and his concern for it [are] known only to those within its framework. The ideal for College life is the City State of Plato in which like minds administer and further the affairs of the establishment. Austin filled this position admirably ... His philosophy was to guide and to bring those other members by persuasion to a similar belief'. Austin's daughter, Barbara Jeffrey, writes that (personal communication) 'he also felt it was important to college life that people should be able to get on well with one another'.

In his address at the Memorial Service for Austin in November 1993, Alan Hughes, Austin's colleague and an economics Fellow of Sidney, spoke of Austin's role as an active member of the Investments Committee responsible for the management of the stock market portfolio set up in the 1960s, of his many gifts of, for example, silver plate and carpets for public rooms and of the 'exceptionally generous bequest to Sidney to further education and research' (personal communication). He described Austin in retirement 'as a familiar figure in college, especially in the continuation of his life-long association with the chapel. His interest in sport...meant that any other fellow with a similar interest in following [horse racing and rugby] on TV would often find an agreeable companion in Austin', not least because of the wine he provided to offset the bitter reaction to an Oxford try on 'a gloomy mid-winter Tuesday' (personal communication).

8 Contemporaries' Impressions and Evaluations of Austin

Austin had a long life, he worked extraordinarily hard, and was associated with a breathtaking number of institutions in academia, government, and internationally. Of all these institutions he was, in his own words, a willing 'slave'. As with many of his generation, he found delegation difficult and this caused clashes and misunderstandings, sometimes leaving Austin feeling hurt

and unappreciated by other officers of the organisations for which he worked so hard and, overall, served so well.

For the Memoir of Austin which I wrote for the *Proceedings of the British Academy* (Harcourt 1997), I asked a selection of people from these and other institutions for their impressions and evaluations. What emerged is the respect and affection in which he was held in so many spheres: respect for his outstanding abilities, affection for him as a person even though his stature and personality were such that I do not think the persons concerned felt they were able to get really close to him, much as they may have wished to. I quote below from their responses.

Robin Matthews worked closely with Austin in the 1940s, 1950s, and 1960s in the Faculty and also on the *Economic Journal* when Matthews was Review Editor. He singled out (6 March 1994) Austin's contributions to economics, emphasising the *range* of topics to which he made original contributions. Though Austin did not keep up to date with the literature, he 'had a knack of identifying what was important'. He concluded that Austin was a most serious and optimistic economist who 'believed that economics was capable of doing good'.

Frank Hahn's views (6 June 1994) are complementary to those of Matthews:

Austin was a born "mandarin" ... impatient of theory which abstracted from the "real world". His aim was to improve the world whether it was the small world of Cambridge, the Indian subcontinent or the Royal Economic Society. His memoranda... were perfect instances of what such writings should be: lucid, precise and brief.

Austin was socially a cut above many of his more recent colleagues. He had enormous self-confidence, and spoke in upper-class Cambridge English. He was also apt to favour those he knew—especially in Cambridge—when it came to jobs and honours. This was not really a sign of the "old school tie" syndrome. He simply took it for granted that the best minds, and indeed the morally most reliable minds, were to be found in Cambridge. After that he would allow some merit to Oxford and London, but not much beyond that ... [A] failing, but one found it hard to blame him for being faithful to beliefs formed when England and its Universities were very different from what they are now.

Austin was long associated with the NIESR. Two former Directors, Bryan Hopkin and David Worswick, recollected Austin's role there and much else besides. Worswick tells an amusing tale of how, at Robert Hall's prompting, he concocted a 70-and-over rule to rid the Executive Committee of Austin and one other 'old man' (which soon took off Hall himself). To their credit, 'both departed gracefully... without enquiring too closely into the origin of the rule'.

Hopkin (5 June 1994) reported on Austin's massive contributions during Hopkin's time as Director (1952–1957). Austin 'took a detailed interest in all the work...going on, [gave] wise and informed counsel' and personal support to Hopkin. Austin was the ideal person to fill such a role because he knew and was respected by so many people, he criticised work incisively but gently, and was well behaved even in the most difficult circumstances.

What of American evaluations? When President of the IEA, Paul Samuelson 'was most content to have [Austin] run me and all in sight' (7 July 1994). He thought that, as an economist, Austin was original and lucid, that he had good judgement which was not affected by dislike or personality. He considered it remarkable that he 'never heard [Austin] utter a sour criticism of any of the Cambridge menagerie'. Bob Solow (7 June 1994) praised Austin's role in the IEA, highlighting the length of the conferences under Austin's guidance, which enabled serious discussion of papers, and that Austin's force of character made sure that authors wrote the papers that the conference needed. Solow liked Austin, not least for his plain speaking, which contrasted with '[a] lot of Cambridge conversation [which struck Solow] as a move in a game (whose rules and objectives [he did not] know)'.

I have tried to give due weight to Austin's contribution to development economics. Esra Bennathan knew of these at first hand. In a letter of 18 July 1992 to Cairncross, Bennathan mentions that after being interviewed by Austin for the civil service in 1961 he discovered that an 'admired colleague' at Birmingham regarded 'Austin with the utmost suspicion, a dangerous figure of the Establishment, a duplicitous nature hiding behind an ascetic and saintly face'. Bennathan's long experience of Austin was 'totally different'. His lengthy letter is concerned not only with Austin's crucial gifts as an economist but also with his practical Christianity, especially in helping academics in what became Bangladesh both to escape persecution and to build up their libraries and laboratories.

Bennathan summed up his idea of Austin's 'private and instinctive' approach to development issues: '[Austin] work[ed] through and for people ... [He]... measure[ed] his effectiveness by his effect upon them, their actions and their progress ... [Austin] nurture[ed], encourage[ed] and sponsored those he [thought] promising, without expecting too much'.

I.G. Patel (5 July 1955) knew Austin as a supervisor (1946–1949) and then 'in many capacities'—visits to India (sometimes as a family guest), IEA conferences, consultant to UNDP, Council of the RES. His first impression was 'of a very generous and rather shy and self-effacing person'; his final summing up: 'Generous, self-effacing and deeply committed'.

Hans Singer (4 October 1994) also paid tribute to Austin's generosity and first-rate intelligence. Austin was the secretary of 'a small committee at Cambridge' set up in the early 1930s to help two German refugee students of whom Singer was one. Though not Singer's PhD supervisor, Austin gave him 'invariably helpful' advice on some problems in his dissertation. Austin's 'empirical approach and clear language were a great help to a new arrival, bewildered...by the incomprehensible lectures and papers by Wittgenstein and Piero Sraffa [as well] by the intricacies of liquidity preference'. Always 'young Singer' to Austin, 'up to shortly before his death people...from Cambridge [carried] greetings from [Austin] to "young Singer"'.

I conclude with Susan Howson (3 October 1994): 'I have a great admiration, as well as love, for Austin, who always struck me as one of the most sane members of our profession'.

9 Conclusion

To the end of his life, Austin remained mentally rigorous and alert. He had a fine sense of humour which was often combined with sharp, even wicked end lines about his contemporaries, delivered with a twinkle. He enjoyed gossip and barbed, but not malicious comments in private, for he was, first and foremost a kindly man, who nevertheless was realistic about and comforted by the fact that foibles as well as achievements characterise the human condition.

Austin and Joan were proud and fond of their five grandchildren and had, especially after the arrival of grandchildren, excellent rapport with their daughters and their respective husbands, who in turn appreciated the love and support they could depend upon.

In May 1993, Austin had a bad fall and was taken to Addenbrookes Hospital in Cambridge. He died peacefully on the morning of 1 June, having heard some of his favourite Bible readings and prayers the night before.

As we noted, his colleagues regarded him as the role model par excellence for aspiring applied political economists. At the Memorial Service for Austin in Sidney Sussex Chapel on 20 November 1993, one reading was the parable of the talents. Some thought this a peculiar choice; but a close friend who knew Austin intimately thought it peculiarly appropriate because Austin could not abide those who did not use their talents to the full. For Austin, economics was a 'hands on' subject—the sole object of theory was for it to be applied to explanation and then to policy proposals: '[N]o economist is more dangerous than the pure theorist without practical experience and instinctive understanding of the real world that he is attempting to analyse, seeking precision in a world of

imprecision, in a world he does not understand' (Austin Robinson 1992: 221). His Christian upbringing, in which works were emphasised even more than faith, and his wartime experiences led him to a life of service to his discipline and to humanity, especially to those least able to help themselves, victims of both oppression and the malfunctionings of social systems.

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27

Piero Sraffa (1898–1983)

Alessandro Roncaglia

1 Introduction

Piero Sraffa is not only one of the greatest economists of the twentieth century, but also a major protagonist in the history of philosophy and political science.¹ Born in Turin, the only son of Angelo Sraffa, a well-known Professor of Commercial Law (Bocconi University opens onto the Milanese piazza that bears his name), Piero attended elementary school in Parma, lower secondary school in Milan, and upper secondary school and university in Turin, where he graduated in law in 1920, with a dissertation (under the supervision of Luigi Einaudi) on *Monetary Inflation in Italy During and After the War* (privately published, Sraffa 1920).

The dissertation includes some ideas which Sraffa was later to discuss with Keynes when they first met in August 1921 and which, in my interpretation, was to exert some influence on the latter (see below). Subsequently, Keynes took the young Italian under his wing and commissioned from him an article on the Italian banking system for the *Manchester Guardian Reconstruction Supplements*, which he was editing. The resulting article was so good that

¹ For a more extensive illustration of Sraffa's life and writings, a fuller bibliography, and a more detailed interpretation of his thought, let me refer the reader to Roncaglia (2009). Unavoidably, some points in this chapter are taken from that book.

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Keynes decided to publish it in the *Economic Journal* (Sraffa 1922a), requesting a shorter piece for the *Supplements* (Sraffa 1922b). The two articles reveal a command of institutional and technical aspects of banking, awareness of the interests at stake, and a lively interest in economic policy (indeed, in politics in general).

The second article, which shed light on the weakness of Italy's leading commercial banks and the doubtful legitimacy of some policy measures adopted by the Bank of Italy, irritated Mussolini, who demanded recantation, without obtaining it. Keynes invited Sraffa to Cambridge, but the British government refused admission to the UK, possibly under pressure from the Fascist authorities (as Keynes thought) or because of Sraffa's leftist leanings (as Sraffa himself tended to believe).

In the meantime, Sraffa had begun an academic career as economics lecturer at the University of Perugia (from 1923), rising to a full Professorship at the University of Cagliari in 1926. This was thanks mainly to his long 1925 article in Italian analysing and criticizing Marshallian theory, followed by a shorter one published in English in the *Economic Journal* in 1926, in which a summary of the critique was accompanied by the proposal of a theory of imperfect competition, which he soon abandoned but which was taken on and developed by Joan Robinson (1933) [1969] and others.

Confronted with the increasing authoritarianism of the Fascist regime (his friend Antonio Gramsci had been arrested in November 1926) and with a renewed invitation from Keynes, Sraffa moved to Cambridge in 1927, first as Lecturer, then as Director of Research, and finally as Librarian at the Marshall Library, the position most congenial to him, which he would retain for decades. His connections with King's College date back to the time of his arrival in Cambridge; however, in 1939 he became a Fellow of Trinity College, holding the position up to his death. In the last three decades of his life he resided in college rooms, in Nevile's Court, a point of attraction—together with his office at the Marshall Library—for Italians (including the recent President of the Italian Republic, Giorgio Napolitano) and for economists from all over the world.

Sraffa's research programme in economics, for which he provided most of the necessary theoretical material, was to accomplish nothing less than a revolution: a shift from the marginalist to a classical approach, modified in such a way as to take into account Keynes's contribution. He also had a significant influence in other fields, in particular through his friendship with

Gramsci and Ludwig Wittgenstein, two leading exponents of twentieth-century culture in the fields of political science and philosophy, respectively. In 1961, he received the Södeström Gold Medal from the Swedish Academy of Sciences, an honour shared with Keynes (who had been awarded the prize in 1939), and which constitutes the antecedent to the Nobel Prize in Economics.

Illustrating Sraffa's contribution to economics without considering his broader programme could prove misleading. Notwithstanding the abstract and apparently self-contained nature of his main work, *Production of Commodities by Means of Commodities* (1960), it cannot be understood fully without a grasp of Sraffa's world view and method,² in relation to which it will be worth considering what we know of his relationship with Gramsci and Wittgenstein. This aspect will be discussed in Section 2, together with Sraffa's contribution to the history of economic thought through his magisterial edition of Ricardo's *Works and Correspondence* (published in 11 volumes between 1951 and 1973) and his representation of the economy as a circular flow of production and consumption, rather than a one-way street leading from scarce resources to the satisfaction of economic agents' wants and desires. In Section 3, Sraffa's critique in the 1920s of the then mainstream Marshallian approach is presented as a preparatory step to *Production of Commodities by Means of Commodities*. Section 4 provides a brief outline of the fortunes of Sraffa's thought. Section 5 concludes.

2 Gramsci, Wittgenstein, and Ricardo

Sraffa met Antonio Gramsci in 1919 while both were university students in Turin. Without ever becoming a registered member of the Socialist or the Italian Communist Party (founded in 1921), Sraffa developed a strong friendship and intellectual relationship with the Communist leader, with long discussions and collaboration on the journal founded by Gramsci, *L'Ordine Nuovo*. When Gramsci was imprisoned in 1926, Sraffa became his main point of reference in the outside world (after a short while, through Gramsci's sister-in-law, Tatiana), getting him books and journals, searching for ways to obtain freedom without genuflecting to the Fascist regime, offering food for thought through Tatiana's letters, and thus acting as a stimulus to the writing of

²When in 1967, I asked Paolo Sylos Labini to supervise my degree dissertation on Sraffa, he requested that I first read Smith's *Wealth of Nations*, Ricardo's *Principles*, and Marx's *Capital*: no small assignment, but most useful advice.

the famous *Quaderni del Carcere* (*Prison Notebooks*, published posthumously, Gramsci 1975), which would exert a deep and wide influence on Western leftist thought in the post-Second World War period. Sraffa's influence on Gramsci, and Gramsci's influence on Sraffa (and, through him, on Wittgenstein), can still provoke heated debate; however, it would be beyond our scope to go into the issue here.³

Something more can be said about Sraffa's relationship with Ludwig Wittgenstein. The two met in Cambridge in 1929, where the Austrian philosopher had just moved, under the auspices of Bertrand Russell, who a few years earlier had taken care of the publication of Wittgenstein's celebrated book, *Tractatus Logico-Philosophicus* (Wittgenstein 1921 [1922]), considered by many as the high point of logical neopositivism. Wittgenstein's long and frequent discussions with Sraffa exerted a decisive influence on the Austrian philosopher and played an important part in his transition from the logical neopositivism of the *Tractatus* to the new position developed in *Philosophical Investigations*, published posthumously (Wittgenstein 1953).

Wittgenstein himself in the Preface to *Philosophical Investigations* stated: 'I am indebted to [the criticism] which a teacher of this university, Mr. P. Sraffa, for many years unceasingly practised on my thoughts. I am indebted to *this* stimulus for the most consequential ideas of this book' (ibid.: viii; italics in original). Wittgenstein had abandoned the idea, developed in the *Tractatus*, of language as a biunivocal representation of the world, with a connection between 'propositions' (constituting language) and 'facts' (constituting the world). In the *Philosophical Investigations* he developed a new theory of language and the relations between it and the world it should describe: a theory based on the idea that there is not just one type of language, but 'there are *countless* kinds: countless different types of use of what we call "symbols", "words", "sentences". And this multiplicity is not something fixed, given once for all; but new types of language, new language-games, as we may say, come into existence and others become obsolete and get forgotten' Wittgenstein (1953: 21; italics in original). This is the so-called theory of language games: theoretical models focusing attention on particular aspects of real language, presenting them as the general language of a group of people.

This methodological position can be generalized, as in opposition to the idea that reality can be represented through a general, all-encompassing model—such as, in economics, general equilibrium theory—and preference for the construction of specific theoretical bricks connected into a more gen-

³Let me recall only a recent paper by Amartya Sen (2003); for my own interpretation see Roncaglia (2009: 6–9 and 25–28).

eral world view by the requirement of conceptual consistency. In the case of economics, this means consistency with a representation of the economy as a circular flow of production and consumption, as in classical political economy, rather than as a one-way street from scarce resources to the satisfaction of economic agents' desires, as in the marginalist/neoclassical approach.

Many contemporary economists are convinced that there is only one correct way of looking at the economy, namely the scarcity view. This is a frequent source of misunderstanding in debates between mainstream and heterodox economists. So much was quite clear to Sraffa himself, since in the early stages of his research career he too considered equilibrium between supply and demand as the only rule of the game, to the extent that in his 1925 article, after demonstrating that within Marshall's approach both increasing and decreasing returns are logically unacceptable, he concluded that constant returns must be assumed (Sraffa 1925). According to philological reconstructions based on Sraffa's papers (preserved at Trinity College, Cambridge), it is now believed that the turning point came around 1930, when Sraffa reached the conclusion that no assumption whatever on returns is necessary, since the level of production may be assumed as given when analysing the central issue in value theory, namely determining the set of prices of production that guarantee reproduction of the circular flow and their relationship with income distribution. According to Sraffa's own reconstruction of classical political economy, this—the idea that equilibrium between supply and demand is *not* the fundamental pillar in classical value theory—is a crucial divide between the classical and the marginalist approach. Hence, a study of the classical conceptualization of the economy, as distinct from the marginalist conceptualization based on supply and demand equilibrium (i.e. the balancing of scarce resources with economic agents' preferences), is a prerequisite for a theoretical reconstruction of the classical approach.

Thus, Sraffa's meticulous work on his magnificent edition of Ricardo's *Works and Correspondence* is not solely the fruit of a passionate interest in the history of economic thought; it was also part of his effort to bring economics back from the marginalist to the classical path—and, as we shall see, in such a way as to make the latter compatible with his friend Keynes's own contributions. In fact, it was at Keynes's suggestion that the Royal Society entrust Sraffa with the task of editing Ricardo's writing in 1930, and it was Keynes who repeatedly defended Sraffa when he delayed completion of the work year after year, and who helped him in the years-long fruitful treasure hunt for lost Ricardian manuscripts (which led to many minor and two major discoveries, such as a chest of letters received by Ricardo from his correspondents, discovered in 1930, and a number of important letters from Ricardo to

James Mill and various manuscripts, including the essay on 'Absolute Value and Exchangeable Value' in 1943). But again, this is not the place to recount the story of this truly magnificent enterprise, the countless anecdotes about Sraffa's meticulousness and hard work, and the eager expectation of economists such as Einaudi or Schumpeter for the completion of the work.⁴

Let us summarize, from our post-1960 (i.e. with our knowledge of Sraffa's 1960 book) vantage point, Sraffa's interpretation of Ricardo. Sraffa stresses the importance of the notion of the surplus, or in other words the difference between the set of commodities produced and the set of the means of production and subsistence necessary to carry on production. In the circular flow of production and consumption, when the division of labour prevails, each productive unit at the end of the production process obtains a product (or, in joint production, a set of products) and, in order to start the production process again, needs to obtain from other sectors (other units of production) its means of production and the means of subsistence for its workers. Thus, there arises a web of exchanges between productive units and between sectors. In a market economy, these exchanges also constitute the way in which the surplus is distributed between productive units and sectors; Sraffa assumes, with implicit reference to a capitalist economy, that this happens on the basis of the simplifying assumption of a unique rate of profits (which corresponds to the assumption of free competition, that is, free movement of capital between the different sectors of activity in the economy).

When a wage rate equal to the subsistence wage is also assumed, such as to allow workers to acquire the means of subsistence necessary for their and their family's survival, and once rents are determined thanks to the 'Ricardian' theory of differential rent (which, in fact, Ricardo drew from Malthus, West, and Torrens), profits correspond to the part of the surplus not accruing to rents. If, as does Ricardo, we assume that all rents are devoted to luxury consumption and profits are entirely devoted to investment, the rate of profits (equal to the ratio between profits and capital advances) corresponds to the rate of accumulation (equal to the ratio of investments to capital advanced).

There is a problem, however: since profits are obtained as the difference between the value of the surplus (consisting of a set of heterogeneous commodities) and the amount of rents, and since capital advanced is also a set of heterogeneous commodities, we need to express in value terms such aggregates of commodities in order to determine the rate of profits and the rate of accumulation (hence, the rate of growth of the economy). This is no easy task, however, and—as Sraffa shows—Ricardo persevered in it up to the end

⁴ On this, let me refer the reader once again to Roncaglia (2009: 35–40).

of his life without, however, coming to the definitive solution, but only to approximate ones.

According to Sraffa's interpretation, a preliminary, approximate solution (to be found in Ricardo's 1815 *Essay on Corn*, but possibly devised one year earlier) consists in determination of the rate of profits as a ratio of two physical quantities of corn. This solution relies on a strong simplifying assumption, namely that there are only two commodities in the economy: corn (agricultural produce), utilized as a means of production in both sectors of the economy and as the only means of subsistence for the workers, and a manufactured commodity, which is only utilized as a means of production in its own sector. Thus, we may focus attention on the corn sector: once we have circumvented the problem of rent by focusing on no-rent land, subtracting from the amount of corn produced on this kind of land the amount of corn used as a means of production and for subsistence, we obtain profits as an amount of corn, while also capital advances are expressed as an amount of corn; the ratio between profits and capital advances, both magnitudes expressed in physical terms as amounts of corn, thus determines the rate of profits.

Malthus immediately objected to this solution that in no sector does any such situation occur, with a single commodity representing both the whole product and the whole of the means of production and subsistence. Thus, in the short time span of a couple of years, Ricardo turned to a different solution. In his *Principles of Political Economy and Taxation* (1817) he adopted a labour-contained theory of value, expressing both aggregate product and aggregate means of production and subsistence in terms of the labour directly or indirectly required for their production. Once again, this allowed him to determine the rate of profits as a ratio between two quantities, surplus produce and capital advances, both expressed in terms of a single magnitude, labour. Once again, Ricardo himself knew that this solution was not perfect, and in the very first chapter of the *Principles* indicated the limits of the labour theory of value, including the existence of different ratios between labour and capital advances in different sectors of the economy, different lengths of production period, and different durations of the various capital goods (this latter being the object of a specific critique by Torrens). As a result, the problem of value remained open, nor could the answer to what Marx (1894) called the problem of transformation—of labour values into prices of production—suggested in the posthumous Book 3 of *Capital* half a century later amount to a definitive solution. Finally, after years of searching, a solution was offered by Sraffa in his 1960 book, *Production of Commodities by Means of Commodities*, as we shall see in more detail below.

3 The Sraffian Revolution: Critique and Reconstruction

The cultural project Sraffa pursued, gradually defined as he worked on its various aspects, was—as mentioned above—a revolutionary one: to substitute the dominant approach to economics, the marginalist/neoclassical one based on the pillars of scarcity and preferences and on the equilibrium between supply and demand, with a return to the classical (Ricardian) approach in such a way as to make it compatible with Keynes's new findings on the active role played by money in modern capitalist economies and the possibility of persistent unemployment. To this project, Sraffa contributed a number of basic constitutive elements: first came a critique of the then dominant Marshallian stream of the neoclassical approach (born in Cambridge, but subsequently colonizing the rest of England and nearly the whole world); second, there was the edition of Ricardo's writings discussed in the previous section, and with it a reconstruction of the classical conceptual framework; third, we had a critique of the traditional marginalist theory of value and distribution; and fourth, a solution was proposed to the problem of value which, as we saw in the previous section, Ricardo and the Classical economists—Marx included—had left open.

Before considering these two latter elements, we present a brief overview of Sraffa's initial work on the critique of the Marshallian theory.

Sraffa's main contribution in this respect was a long article published in 1925 in Italian when he was 27-years-old. As we saw, he had been appointed to a Lecturership at Perugia University in 1923, and had chosen Marshall's *Principles* (1890) as his textbook, preferring it to Italian texts such as Pantaleoni's *Principii di Economia Pura* (1889), although the latter was an author he greatly respected. Sraffa's sharp, critical mind was thus brought to focus on that stream of marginalist economics—preferred as less abstract to the Walrasian stream of general equilibrium theory, well known in Italy through Pareto's writings, and considered useless for interpreting the real world.

Sraffa's 'destructive' criticisms—as Keynes (1930b: 79) considered them—concerned the two main aspects of Marshall's theory: the method of partial equilibria, by which supply and demand equilibria were analysed for individual industries and firms, and the notion of laws of returns to scale, which allowed for the construction of U-shaped supply curves.

The assumption of perfect competition implies small but not infinitesimal individual firm size, and hence increasing returns (decreasing costs) at first,

when the quantity produced grows starting from zero, followed by decreasing returns (increasing costs) for still higher levels of production, with an aggregation of supply curves for individual firms providing the supply curve for the industry, while competition ensures that the number of firms is such that each of them produces in equilibrium at the point of its minimum costs. The *ceteris paribus* assumption is also necessary to be able to focus on an individual industry or firm. However, Sraffa remarks, the reasons adduced for decreasing returns cannot in general be specific to individual firms. Nor, in fact, are they specific to individual industries: when there is a change in the total amount produced by the industry being considered, the reasons adduced to explain changes in its unit costs (such as the scarcity of a factor of production) in general also affect costs in other industries, and in general the effects will be of the same order of magnitude for various industries. As a result, the demand curve of the industry under consideration will shift, and the *ceteris paribus* condition will not hold, given changing prices in industries producing competing commodities.

Sraffa's 1925 article concludes with a suggestion framed in the context of the analytical method of supply and demand equilibrium, namely that constant returns be assumed. However, this suggestion is soon abandoned (although many mainstream analyses of Sraffa's 1960 book fail to recognize this point), reading it as half—the production side—of a general equilibrium model. Instead, in a subsequent article that Sraffa wrote at Keynes's request for publication in the *Economic Journal* (Sraffa 1926), a summary of his argument is accompanied by the suggestion to move in the direction of a theory of imperfect competition, where the individual firm may be confronted by a downward-sloping demand curve, so that (slightly) increasing returns may be admitted.

Following on from this, in a famous 1930 *Economic Journal* Symposium on Increasing Returns and the Representative Firm, Dennis Robertson (1930) tried to answer Sraffa's critique by distinguishing between the 'simplified' Marshall to whom Sraffa's critiques applied, and the 'real' Marshall—an evolutionary theorist relying on the idea of a representative firm caught in the middle of its development, hence characterized by increasing returns to scale, while the growth and decay process of the individual firm ensures the equilibrium of the industry—who was immune to Sraffa's criticism. Sraffa's (1930) answer to Robertson consists in pointing out the inconsistency of such a position, based on the awkward assumption of a life cycle of firms. By then, Sraffa had developed such total opposition to the marginalist structure of Marshall's thought that no mending was

considered possible: both logical consistency and realism are required, and missing these we have to look for other avenues: ‘Marshall’s theory...cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain ... I think... that [it] should be discarded’ (ibid.: 93).

The fact is that Sraffa had by then already taken the revolutionary road of total abandonment of the marginalist approach to return to the classical tradition. This is testified by the Sraffa Papers and is also hinted at by Sraffa himself in the Preface of his 1960 book, where he recalls having already shown a first draft of its main theoretical propositions to Keynes in 1928. We may also stress, in this context, that Keynes is the only economist whom Sraffa thanks in the Preface, attributing to him a crucial suggestion: ‘[H]e recommended that, if constant returns were *not* to be assumed, an emphatic warning to that effect should be given’ (Sraffa 1960: vi; italics in original).

Ironically, in a text celebrated for its conciseness, the point about constant returns is stressed three times, at the very beginning. The insistence is due to the fact that this point is decisive for interpretation of the book. It is in this context that Sraffa distinguishes between two kinds of readers, the marginalist- and the classical-leaning, and two aims simultaneously pursued in the book, a critique of the traditional marginalist approach and reconstruction of the classical one: ‘Anyone accustomed to think in terms of the equilibrium of demand and supply’, says Sraffa (ibid.: v), alluding to the marginalist readers—‘may be inclined, on reading these pages, to suppose that the argument rests on a tacit assumption of constant returns in all industries’. Such readers, embedded with the marginalist conceptualization of the economy and hence incapable of reasoning outside of the framework of the equilibrium between supply and demand, need first to be persuaded that there are crucial theoretical inconsistencies in their theory. Thus, let them assume constant returns so that by reading Sraffa’s price equations as part of a general equilibrium model they can discover the logical inconsistencies embedded in the traditional marginalist theory of value and distribution. Once such a theory—and the marginalist approach based on supply and demand equilibrium—is abandoned, the reader can usefully reread the book as a *positive* contribution, solving a problem which the classical approach had left open: in this reading, the assumption of constant returns can, indeed, must, be abandoned, since the framework of equilibrium between supply and demand is extraneous to the classical conceptual framework (more precisely, the classical economists refer to a role played by demand and supply in the determination of market prices, which are not considered as theoretical variables).

Let us briefly summarize Sraffa’s criticism of the traditional marginalist theory of value and distribution. We can focus on two chapters of Sraffa’s book:

Chapter 6, providing a critique of the Austrian notion of the average period of production, and Chapter 12, on the choice of techniques. The analysis conducted in these two chapters shows that one of the main pillars of the marginalist construct, namely the direct relationship between the real wage rate and the capitalist intensity of production processes, does not hold.

As for the first aspect, let us recall that the notion of the average period of production had been used by the Austrian School, from Böhm-Bawerk to Hayek, as a measure of the capitalist intensity of production, with the rate of profits determined by the demand and supply of capital. Sraffa shows that the average period of production depends on the rate of profits and that there is no precise law for their relationship. Thus it cannot be utilized for measuring the quantity of the factor of production ‘capital’ in the framework of the determination of the rate of profits interpreted as the price of such a factor.

As for the second aspect, namely the choice among alternative techniques when income distribution changes, Sraffa shows the possibility of the ‘reswitching of techniques’, namely the possibility that a given technique, after turning out to be the most profitable one, is superseded by another technique when the rate of profits increases, but then turns out again to be the most profitable one at still higher profit rates. The implication of this fact is the following. However the capitalist intensity (i.e. the ratio between the quantities utilized of the two factors of production, capital and labour) of the different techniques is measured, it is not true that the quantity of capital utilized in the economy decreases (and the quantity of labour increases) when the rate of profits—that is, the ‘price’ of the factor of production capital—increases (and, conversely, the quantity of labour employed increases when the wage rate, the price of the factor of production labour, decreases). In other words, the traditional marginalist theory of value and distribution falls apart as soon as it is recognized that there is more than one single commodity in the economy. Traditional macroeconomics, based on the idea that when there is unemployment a fall in the wage rate suffices to boost employment up to the full employment level, also falls apart, together with its pillar, the inverse relationship between the real wage rate and employment.

Let us now turn to the positive contribution made by the classical approach. The classical problem is that when commodities are at the same time products and means of production, within the conceptualization of the economy as a revolving circuit of production and consumption the price of any commodity cannot be determined independently of the others nor the complex of relative prices independently of the distribution of income between wages and profits. It is therefore necessary to consider simultaneously the interrelations running

on the production side between the various sectors of activity while also tackling income distribution and the determination of relative prices.

As a first step, Sraffa (1960: 3) shows that in a system of production with no surplus product, and where 'commodities are produced by separate industries and are exchanged for one another', at the end of the production period 'there is a unique set of exchange values which if adopted by the market restores the original distribution of the product and makes it possible for the process to be repeated'. The situation is more complex when the system under consideration is able to produce a surplus. In this case, 'the distribution of the surplus must be determined through the same mechanism and at the same time as are the prices of commodities' (ibid.: 6). Sraffa shows that, if the wage rate can exceed the subsistence level, the relative prices and one of the two distributive variables—wage rate or rate of profits—are jointly determined, once the technology and the other distributive variable are taken as given. This basic result is then extended to the case of joint production and, as a specific instance of this category, to the case in which fixed capital, and a scarce resource such as land, are utilized.

Attention is also devoted to the construction of an analytical tool, the 'standard commodity', and to proving the uniqueness of the underlying 'standard system'. The standard commodity has the property that the aggregate of its means of production does not change in value relative to the product when income distribution changes. Sraffa shows that this obtains when both product and means of production are but different quantities of the same composite commodity, constructed by adding up in different proportions the different sectors of the economy. Thus in the standard system the rate of profits can be determined, as in Ricardo's 'corn model', as a physical ratio.

The way in which Sraffa solves the classical problem implies its specification in various respects. First, prices of production are those prices which allow for the reproduction of the economy, given the technology, and for a given value of one of the distributive variables; as a result, prices of production change when income distribution changes, or when technology changes. Second, since constant returns are not assumed, the technology assumed as given corresponds to a given set of activity levels in the various sectors of the economy: in other words, what is considered in Sraffa's analysis is a sort of 'photograph' of the economy at a given moment in time. As such, the determination of prices of production and their relationship with income distribution is kept separate from the determination of technology and technological evolution, and indeed from the determination of income distribution and its changes over time as well as the determination of activity levels and

employment, and again their changes over time. Such ‘separation of issues’ is a characteristic of Sraffa’s method, and as we saw above it also emerged in his discussions with Wittgenstein, leading the Austrian philosopher to develop a theory of language games.⁵

4 The Debates in Capital Theory and the Sraffian Schools

The Sraffian revolution has thus been described in its constituent elements: method, critique of the marginalist approach, and reproposal in the new guise of the classical approach. How were the Sraffian ideas received?

Once again, we can try to answer this broad question by considering separate aspects. First, there have been a number of theoretical enquiries trying to reformulate Sraffa’s analysis in strict mathematical terms and developing some specific points within it. Second, there has been a series of debates on the solidity of Sraffa’s reinterpretation of Ricardo. In both streams, a series of important contributions have been made, but it is beyond our scope to consider them here.⁶

A third stream of discussion drew more attention worldwide, namely the two Cambridge debates on capital theory, running high in the 1960s and early 1970s but with few subsequent outbursts. In these debates, the so-called Anglo-Italian School (Pierangelo Garegnani, Luigi Pasinetti, Luigi Spaventa, and various others, including Nicholas Kaldor and Joan Robinson) confronted the Cambridge on the other side of the Atlantic (Paul Samuelson, Robert Solow, and others). The debates concerned the validity of Sraffa’s critiques; as it turned out, and as was recognized by Samuelson himself, such critiques held.⁷ But their relevance, it was said, was limited to the ‘aggregate’ version of the marginalist approach and did not affect general equilibrium theorizing (GET) of the Arrow–Debreu variety. The point is still a topic for debate: Walras’s original theory was indeed affected, and rescuing the Arrow–Debreu variety of GET means interpreting it in such a restrictive sense as to render it utterly useless in interpreting the real world.⁸ What is more, mainstream macroeconomics should

⁵ For a fuller illustration of this interpretation (first proposed in Roncaglia 1975), see Roncaglia (2009).

⁶ For brief accounts of these debates, the reader is referred, once again, to Roncaglia (1975, 2009), and the bibliographies contained therein. For a reconstruction of the whole history of economic thought on the lines suggested by Sraffa, see Roncaglia (2005).

⁷ For a survey of these debates, see Harcourt (1972).

⁸ Garegnani (1960) shows that Sraffa-type critiques apply to all the main exponents of the marginalist tradition, including Walras and Wicksell.

have been abandoned: in fact, it was rescued as ‘lowbrow theory’ (the appellation is Samuelson’s, while Frank Hahn preferred to speak of ‘lowbrow theorists’). In short, the Sraffian critiques did not receive a theoretical rebuttal but simply a political one: In the cultural climate of the Reagan–Thatcher revival of the free market, they were ignored together with their conclusions on the fallibility of the invisible hand of the market, unable to automatically re-establish full employment.

Lastly, there came a series of contributions working on the reconstruction of classical political economy. Elsewhere I have proposed a distinction between three different lines of development, respectively, christened the Marxian, the Ricardian, and the Smithian Schools (see Roncaglia 1991, 2009: Chapter 8). Here I shall illustrate these reconstruction projects only briefly.

The first, the main exponent of which is Pierangelo Garegnani, interprets Sraffa’s analysis of production prices and their relationship with income distribution as ‘the core’ of the intended reconstruction of classical political economy. Garegnani (1990: 124–125) proposes ‘a distinction between two fields of analysis: a field where general quantitative relations of sufficiently definite form can be postulated’—‘the core’—‘and another field where relations in the economy are so complex and variable according to circumstances, as to allow not for general quantitative relations of sufficiently definite form’, namely the rest of economic theory: ‘The relations pertaining to this second field had accordingly to be studied in their multiplicity and diversity according to circumstances’. Moreover, according to Garegnani (1981: 112), Sraffa’s analysis of ‘the core’ of economic theory retains the central aspects of Marx’s thought: ‘[T]he contingent nature of capitalism is demonstrated by Marx on the basis of an analytical nucleus consisting in what he often calls “the internal nexus of bourgeois economic relations”, that is, basically, the antagonistic relation between wages and profits’.

The Ricardian line of reconstruction retains two main characteristics of Ricardo’s analysis: his deductive method, and the connection between growth and income distribution. Luigi Pasinetti’s 1981 model and its book-length discussion represent a synthesis of this line of research. Pasinetti (1981: 25) aims at ‘a theory which remains neutral with respect to the institutional organization of society’ and focuses on the ‘primary and natural’ features of the economy, namely ‘the conditions under which it may grow and take advantage of exploiting all its potential possibilities’. The evolution of technology is considered exogenous, as well as the evolution of consumption, for which the subjective element of preferences is discarded in favour of a sectoral articulation of Engel’s Law. The assumption of full employment gives the model a normative nature: the method proposed consists ‘of singling out first the

fundamental structural dynamics which must take place and then of trying to facilitate them' (ibid.: 243–244).

The Smithian reconstruction, developed in a number of writings by Paolo Sylos Labini (e.g. 1984), attributes a central role to the political–institutional setting and to market forms in their interaction with the division of labour and the process of accumulation. The main object of economic enquiry is thus located in the 'wealth of nations' and the factors determining its evolution over time and in different countries. While Sraffa's contribution is decisive as a critique of the marginalist approach, as a reconstruction of the conceptual foundations of the classical approach and as the solution to a central problem which classical economists had been unable to solve, namely the determination of prices of production and their relationship with income distribution, this latter aspect cannot be considered exhaustive as economic theorizing, nor as constituting the basis of an all-encompassing model. There is an analytical separability of the different issues, to be tackled within 'different analytical areas' (Roncaglia 2009: 153), though within a common classical conceptual framework.

A testing ground for the three reconstruction projects is the relationship they establish between Sraffa's and Keynes's contributions. It is not inappropriate, therefore, to conclude this presentation by briefly recalling the relationship between the two main Cambridge economists of the twentieth century.

Sraffa was 15 years younger than Keynes, and they held quite different political views. This notwithstanding, their relationship in the economic field was very close, with what appears to have been a reciprocal influence. Let us briefly recall some elements in this respect (some of which have already been alluded to above).

The acute intelligence of the young Sraffa (23-years-old) must have impressed Keynes, already a well-known economist, at their first meeting in the summer of 1921. One of the ideas Sraffa had suggested in his degree dissertation (Sraffa 1920), namely the distinction between internal and external monetary stability, not to be found in Keynes's early writings, found a place in his *A Tract on Monetary Reform* (Keynes 1923 [1971]), the Italian edition of which Keynes entrusted to Sraffa. Keynes also asked him for a contribution on the Italian banking system, to be published in the *Manchester Guardian Reconstruction Supplements*, and when the Italian economist produced it, longer than requested and focused on a single episode, the quite recent (December 1921) bankruptcy of the Banca Italiana di Sconto, Keynes found it so good that he chose to publish it in the *Economic Journal* (Sraffa 1922a) instead. Sraffa was thus obliged to then write a shorter piece (Sraffa 1922b), in which the weakness of Italy's leading commercial banks was

highlighted and serious doubts were cast both on the correctness of their official accounts and on the expedient adopted by the Bank of Italy to support them. Faced with Mussolini's harsh reaction to this article,⁹ Keynes invited Sraffa to Cambridge, but—as noted—the British authorities did not allow entry into the UK, so Sraffa's move to Cambridge was postponed to 1927.

Subsequently, after Sraffa had published his 1925 Italian article, Keynes invited him to write another article in English on the same issues for the *Economic Journal* (Sraffa 1926). His support for the Italian economist after his move to Cambridge was also unflinching: it was Keynes who manoeuvred for the Royal Society to entrust Sraffa with the edition of Ricardo's writings, and during the Second World War had Sraffa freed from the internment camp where the Italian economist had been confined as an enemy alien, notwithstanding his lifelong anti-Fascism. In 1938 the two Cambridge economists, both passionate bibliophiles, together produced a precious edition of an anonymous 1740 pamphlet on Hume's *A Treatise on Human Nature*, providing compelling proof of Hume's authorship. Sraffa was one of the members of the so-called Cambridge Circus, advising Keynes in his transition from *A Treatise on Money* (Keynes 1930a [1973]) to *The General Theory* (Keynes 1936 [1973]); here Sraffa played a mainly critical role, and his preference for an endogenous theory of money was not heeded. Meanwhile, it was once again at Keynes's request that Sraffa intervene in the controversy with Hayek, producing a destructive criticism of Hayek's 1931 *Prices and Production* (Sraffa 1932). Though money and finance are conspicuous by their absence in *Production of Commodities by Means of Commodities* (apart from a cryptic aside on the monetary determination of the rate of profits Sraffa 1960: 33), it is clear that Sraffa was by no means unfamiliar with the money and finance field of enquiry and Keynes's related contributions; indeed, it was Keynes himself who stated, in a lapidary footnote to Hayek's paper and with Hayek's permission, that 'Mr. Sraffa has understood my theory accurately' (Keynes 1932: 249, fn. 2). Of course, there may have been differences on points of detail (and it is likely that Sraffa was not in favour of the short-period equilibrium framework upon which Keynes had cast his *General Theory*, possibly preferring the dynamic approach of the *Treatise on Money*), but it is sufficiently clear that there was quite a lot of common ground, especially on the critical point of the relevance of monetary and financial elements in the real economy.

Sraffa's hint in his 1960 book at the monetary determination of the rate of profit can in fact be read as a statement in the direction of the non-neutrality of money and finance and on the compatibility of such a thesis, crucial

⁹The story is told in Naldi (1998).

for the Keynesian approach, with the Sraffian analysis of the relationship between prices and income distribution. Once again, this theme cannot be tackled here; however, it is clear that the compatibility—indeed, the merging—of Keynes’s and Sraffa’s contributions is essential to the construction of a classical-Keynesian approach alternative to the marginalist one.¹⁰

5 Conclusion

If we consider the whole of his contributions, we may be entitled to affirm that Sraffa provided all the necessary ingredients for a revolution in economics: the rediscovery of the conceptual foundations of the classical (Ricardo’s) approach and the clarification of its opposition to the marginalist–neoclassical approach; the solution to the problem concerning the determination of prices and their relationship with income distribution that classical economists had left open; and the critique of the analytical foundations of the mainstreams of the marginalist tradition from the Austrian theory based on the notion of the average period of production and the Marshallian theory of the firm to the whole of modern macroeconomics founded as it is on an aggregate notion of capital and the inverse relationship between real wages and employment and to any theory of income distribution that considers the wage and the profit rate as scarcity-determined prices of the factors of production, labour, and capital. Sraffa’s analysis is also compatible with Keynes’s ideas, so that we might speak of a Keynes–Sraffa Cambridge tradition in economics.

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¹⁰For an attempt in this direction, see Roncaglia and Tonveronachi (2014).

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Maurice Herbert Dobb (1900–1976)

Hans G. Despain

1 Introduction

Maurice Herbert Dobb arrived in Cambridge as a 19-year-old in 1919. Originally he intended to study history, but just before leaving London, he encountered several unorthodox writers in Karl Marx, John A. Hobson, George Bernard Shaw, and William Morris. These readings inspired him to pursue political economy. Maynard Keynes encouraged Dobb to join his Political Economy Club, ‘an invitation-only’ (Shenk 2013a: 26), ‘markedly elitist’ (Marcuzzo et al. 2008: 573), ‘all-male gathering’ (Skidelsky 1994: 5)¹ society. As a member of ‘Keynes’ Club’ (ibid.: 287), Dobb would read a paper on Marx.²

While at Cambridge as an undergraduate, Dobb was involved with several students and political organizations including the University Socialist Society.³ Dobb’s political activity throughout his life provided him with an ‘order to his existence and meaning to his life’ (Shenk 2013a: 35). His political activity,

¹ Thus, excluding in particular, Joan Maurice (later Robinson) from participating.

² Dobb reports Keynes approved of the paper. Dobb (1978: 117) proclaimed that Keynes liked ‘unorthodoxy in the young, up to a point’.

³ For an impressive, and inspiring, account of Dobb’s early political activity see Shenk (2013a), especially Chapters 2 and 3.

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however, did tend to alienate him from his fellow undergraduate students at Cambridge. On one occasion students disrupted a meeting of the Union of Democratic Control and tossed the young pacifist socialist Dobb into the River Cam (*ibid.*: 24). Nonetheless, following his graduation, he was able to obtain a Studentship for Research at the London School of Economics (LSE). In 1922, while in London, he became a member of the British Communist Party and remained a member all his life.⁴

Dobb was a Fellow of Trinity College, Cambridge. In 1944, the University of London and the LSE attempted to lure Dobb away from Cambridge. In the end, his communism was too worrisome for the LSE (*ibid.*: 127). Thus, Dobb stayed on at Cambridge. He was elected to a University Readership in economics in 1959. He taught at Cambridge until his retirement in 1967. He received honorary degrees from Charles University of Prague, the University of Budapest, and Leicester University. After his retirement from Cambridge Dobb lived in the village of Fulbourn with his wife, Barbara, until he died on 17 August 1976 (Sen 1990).

Dobb's contributions to political economy were extraordinary. He was one of the most influential economists of his generation. He was a member of the Faculty of Economics at Cambridge from 1924 to 1967. Although many of his colleagues, such as Keynes, Joan Robinson, Michał Kalecki, Piero Sraffa, and Nicolas Kaldor, have many volumes written on their economic ideas and contributions, Dobb's work has been comparatively neglected.⁵ This neglect is all the more remarkable when the impact of his work is taken into account. Dobb's 12 published academic books range from economic theory, to economic history, development economics, studies of the Soviet political economy, critique of neoclassical theory and (neo)liberal policy, welfare economics, labor and wage theory, and history of economic thought. He published hundreds of articles, many academic and even more articles and pamphlets intended to bring political economy to the eyes, minds, and actions of a general audience. Dobb was impressively successful, balancing an academic career with political activism.

⁴In an early defense of his commitment to the Communist Party, Dobb stated that his party membership gave him 'a sense of participating in a world movement, and thinking of world events in realistic and up-to-date terms' in the necessary 'struggle against capitalism' (Dobb 1940a: 445).

⁵Dobb was the subject of Despain's PhD dissertation (Despain 2011). More recently, Timothy Shenk (2013a) has written a highly impressive intellectual biography of Dobb. Despain reviewed Shenk's book twice: from a Marxian perspective (Despain 2014a) and from the perspective of the history of economic ideas (Despain 2014b). In the author's opinion, Despain (2011) and Shenk (2013a) provide very different, but complementary, approaches to Dobb's legacy. Despain's focus is an institutionalist reading of Dobb's political economy, especially Dobb's theories on crisis and unstable capitalist development, and an attempt to model Dobb's philosophical approach to social science. Shenk's focus is an intellectual biography, and the dialectic between Dobb's political activity, intellectual output, history, and current events.

2 Classical Political Economy: A Theoretical Way Forward

Dobb's participation in the Political Economy Club, reserved for 'the best of Cambridge's aspiring economists' (Shenk 2013a: 26) as judged by Keynes, signaled his membership into Keynes's inner circle. Keynes would help to secure Dobb's Faculty position at Cambridge. This was significant because 'Only two of the early post-war candidates for the economics tripos—Austin Robinson and Maurice Dobb—got College teaching jobs in the 1920s' (Skidelsky 1994: 5). When Keynes visited the Soviet Union it was Dobb who would accompany him (Shenk 2013b, Skidelsky *ibid.*: 209). He wrote an insightful and praiseful review of Keynes's *A Treatise on Money* (Dobb 1930). However, by the early 1930s Dobb began to retreat into campus isolation (Marcuzzo et al. 2008: 582).

During this time, Dobb also began to drift away from Keynes's inner circle. Shenk speculates that part of the reason for this was to avoid the internal campus politics surrounding Keynes's repudiation of Marshall (Shenk 2013b).⁶ Dobb's voluntary academic isolation would mean that he was not directly involved in the Circus to discuss Keynes's ideas, which would lead to *The General Theory*. Whatever the reasons for Dobb's voluntary isolation, he began to concentrate on political activism and a rigorous and definitive study of classical political economy.

Dobb's studies of classical political economy would culminate in his celebrated *Political Economy and Capitalism* (Dobb 1937). Thus as Keynes was writing *The General Theory* (Keynes 1936 [1973]), denouncing classical political economy as merely (and mistakenly) part of the Marshallian tradition, Dobb was returning to classical political economy as a way forward. At the same time, Dobb would certainly agree with Keynes that marginal productivity theory was

⁶Shenk merely asserts this. It seems he might base this claim on the fact that Dobb indeed had sympathy for Marshall's theories. In fact, Dobb's first book *Capitalist Enterprise and Social Progress* (Dobb 1925) can be described as significantly Marshallian. Nonetheless, I believe Shenk's assertion to be doubtful and likely misleading. By the 1930s, as explained below, Dobb more than drifts away from Marshall in terms of theory. Moreover, Dobb is deeply embedded in a study of classical political economy generally and a rereading of Marx in particular. His drifting away from Keynes's inner circle was far more likely because he had disagreements directly with Keynes and the emerging 'Keynesians'. Of the Cambridge elite political economists, Dobb's strongest theoretical affiliations were with Sraffa (see Dobb 1961, 1970a) and Kalecki (see Dobb 1939a). I would speculate that politically within the department, the forces against this trio were overwhelming and an internal battle of ideas would have been futile. Moreover, Dobb, Sraffa, and Kalecki had far more disagreement with the 'bastard' Keynesians of the Samuelson variety than with the Post-Keynesians at Cambridge.

problematic. Likewise, both agreed that income and wealth could not be fully explained by the forces of supply and demand. However, Dobb insisted, in concert with classical political economy, that a theory of distribution was necessary *prior* to an analysis of market activity and the forces of supply and demand.

Dobb contended that classical political economy had important insights concerning the role of historical modes of (primitive) accumulation and institutionalized systems of power. Smith, for example, maintained that the determination of wages and profits were the function of power relations and legal rights (Smith's version of class struggle) (Dobb 1937: 134ff, 1973: 46–52). Likewise, Smith's theory of agricultural rent emphasized monopoly privilege (Dobb 1976). According to Smith the monopoly privilege concerning land ownership and agricultural rents was primarily a feudal relic (Dobb 1937: 73–78, 1973: 53).

Ricardo regarded the explanation of income distribution as *the* central issue of economics (Dobb 1976). Ricardo wrote that determining 'the laws which regulated this distribution is the principal problem in political economy' (Sraffa 1951–1973; quoted in Dobb 1937: 16, fn. 1). Ricardo maintained that wages and profits were less a function of supply and demand, and far more a function of land fertility, technological change, and historical 'habits of the country' by which he included 'those comforts which custom renders absolute necessities' (Ricardo 1951: 94, Dobb 1937: 46–48, 1973: 65–95).⁷

John Stuart Mill famously contended that the forces of supply and demand did determine the prices of consumption goods, but the costs of production

⁷Dobb and Sraffa edited together the "works" and "correspondence" of Ricardo (see Pollitt 1988). Sraffa began work on the project in 1930. By 1943, he had a 16-volume edition ready in proofs when a number of important letters between Ricardo and James Mill were found, along with other essays (Roncaglia 2009: 35–36). With mounting pressure from the Royal Economic Society and the publisher, in 1948 'Dobb came in to assist with the editorial work' (Sraffa 1951: x). The result was a ten-volume edition entitled *The Works and Correspondence of David Ricardo* published from 1951 to 1955, to be followed in 1973 by a painstakingly compiled volume of indexes. This edition is a highly cherished accomplishment in the history of economic thought. John Eatwell writes of this edition, that Sraffa's and Dobb's 'reconstruction of Ricardo's surplus theory, presented in but a few pages of the introduction to [the] edition of Ricardo's *Principles*, penetrated a hundred years of misunderstanding and distortion to create a vivid rationale for the structure and content of surplus theory, for the analytical role of the labor theory of value, and hence for the foundations of Marx's critical analysis of capitalist production' (Eatwell 1984: 111–116). Importantly, Sraffa and Dobb's volumes on Ricardo were a major contribution in the rediscovery of the theoretical system of classical political economy based on the surplus approach. This was especially important, given the dominant interpretation of Ricardo by Marshall who explained Ricardo to be a rather minor precursor of marginalism and supply and demand analysis. Jevons was even more dismissive, accusing Ricardo of diverting economics from the path of science (Roncaglia 2001: 451).

(wages, rents, and profits) were a primarily a function of institutional and political conditions (Dobb 1937: 43–46). Mill believed that developed countries most needed systems of ‘better distribution’ (Dobb 1973: 136). This is because for Mill there was a separation between the laws of production and the historical modes of distribution (ibid.: 26). The laws of production are ‘necessarily common to all industrial societies’ while the historical modes of distribution ‘assume a particular state of society’ (ibid.: 172). This separation meant that an increase in production, productivity, or growth did *not* necessarily increase the income and wealth of the general population (Dobb 1937: 43–46, 1973: 135–136).

For Dobb, the most important classical political economist was Marx (ibid.: 137–136; Dobb 1932, 1937: 55–78, 1942 [1955]: 178–204, 1943, 1947, 1970b). Marx provided the basic outline for the unstable, crisis-ridden, and stagnationist tendencies of the capitalist system (Dobb 1943: 22–27, 1937: 79–126). Marx argued that the determination of profits and wages was a function of class struggle (Dobb 1937: 93–103). Likewise, Marx argued that rent was primarily a function of monopoly power (Dobb 1937: 73–76, 1966a: 70–75). Thus, drawing on the tradition of classical political economy, Dobb argued that distribution is determined by the institutional physiology of society (Dobb 1951: 226–235), which are in turn a function ‘value’ (in its economic, social and philosophical meaning) (Dobb 1937: 1–33).

Dobb would later state that *Political Economy and Capitalism* (hereafter *PEC*) was too hastily written and not adequate for addressing Keynes’s critique of classical political economy (Dobb 1978: 119). However, ‘several generations of Dobb’s readers’ believed the book to be ‘the most trenchant critique of its day of the foundations of modern Western economic theory’ (McFarlane and Pollitt 1992: 150–151). The book would provide a primary theoretical basis for all of Dobb’s future work and for many generations of political economists. It is in *PEC* that Dobb first demonstrates that the classical political economists did not hold to a ‘subsistence’ theory of wages. Rather, wages in particular and income distribution in general are governed by institutional and historical conditions that are ontologically distinct from conditions affecting production, productivity, and growth. Therefore, production and distribution should be analyzed separately, to achieve a full understanding of institutional dynamics.

3 Intellectual Versatility and Influence

Dobb's students were attracted to his critique of mainstream theory and his unorthodox commitment to, and emphasis of, the historical and institutional context of economic activity and human behavior. Dobb inspired a new approach to history and a new approach to economic theory. His list of Cambridge students included Ronald Meek, Joan Robinson, Victor Kiernan, Eric Hobsbawm, and Nobel Prize Laureate Amartya Sen.

Sen would claim that Dobb was 'the closest to a guru that I've ever had' (Sen quoted in Shenk 2013a: 172). Sen would recount in his 1998 Nobel Address that in the tradition of Dobb's mentor, A.C. Pigou, Dobb himself taught a generation of Cambridge economists (neoclassic) welfare economics: 'The Marxist Maurice Dobb and the conservative neo-classist Dennis Robinson did joint seminars'. Dobb was 'one of the few [Cambridge economists, a department caught in an internal battle over Keynesian economics] who, to my delight, took welfare economics seriously'. Victor Kiernan (1987) would remember Dobb for his knowledge and expertise of Marxian economics. Likewise, Ronald Meek (1979: 61–62) would claim that *PEC* would for 'future historians and economic thought...date the emergence of Marxist economics as a really serious economic discipline'.

Indeed, one of Dobb's greatest strengths as a theorist was his penetrating knowledge of both neoclassical economics and Marxian political economy. He employed his knowledge of neoclassical economics to development incisive immanent critiques (e.g. Dobb 1949, 1969 [1955]: 104–117). He employed his knowledge of Marxian historical materialism to understand the historical emergence of capitalism (Dobb 1946: 33–221) and historical evolution of capitalism and its 'stages of development' (ibid.: 1–32, 222–393). He employed his knowledge of Marxian political economy to the economics of development in order to better understand modern society and the crisis tendencies of capitalism.

Dobb's contributions have a wide versatility, demonstrating his substantial breadth of knowledge in an impressive number of areas. Not only was he arguably *the* foremost scholar of his generation in Marxian political economy, but also his studies and knowledge of the political economy of the Soviet Union were the most renowned of any English-speaking economist. His *Russian Economic Development Since the Revolution* (Dobb 1928), and his *Soviet Economic Development Since 1917* (Dobb 1948), introduced generations of English-speaking economists to the history of the Soviet system.⁸

⁸ Dobb received great acclaim for both these books, from both the right and left. They were remarkable accomplishments.

Dobb further participated in the so-called calculation debate concerning the political economy of socialism (Dobb 1933, 1935, 1939b). His defense of socialism was inspired by his work in the economics of development. Dobb contended that capitalism was not the best system for a developing country. In fact, he argued that the primary tendency of capitalism is to prohibit and undermine economic growth (Dobb 1967: 89–107). Dobb also held a deep passion, interest, and knowledge of the history of economic thought. The last book that he would publish was his highly celebrated *Theories of Value and Distribution since Adam Smith* (Dobb 1973). Above all else Dobb is most well known for his *Studies in the Development of Capitalism* (Dobb 1946) (hereafter *Studies*). The basis of *Studies* grew out of his PhD dissertation, supervised by Edwin Cannan, concerning the history and theory of the capitalist ‘undertaker’ or entrepreneurship. His dissertation itself was the basis of his first book *Capitalist Enterprise and Social Progress* (Dobb 1925) (hereafter *CESP*). He later complained that *CESP* was ‘an unsuccessful and jejune attempt to combine the notion of [Marxian] exploitation with the theory of Marshall’ (Dobb 1978: 117).

Regardless of his own overly severe criticism, the historical sections of *CESP* would provide Dobb with the core historical insights of *Studies*. Moreover, it was *CESP* where he first emphasized the importance of methodology and philosophy of science, and the necessity for an institutional approach to the study of political economy. These three themes or theoretical motifs of *history, methodology and philosophy of science, and institutional analysis*, would become the theoretical pillars for all of Dobb’s future studies in political economy.

4 The Entrepreneurial Myth and the Instability of Capitalism

Dobb’s institutional analysis is well illustrated in *CESP*, especially in the development of his penetrating critique of the ‘entrepreneurial myth’ (Dobb 1925: 3–5). He contended that capitalistic development and growth, alluded to above, along with capitalistic *entrepreneurship* (in conjunction with a non-interventionist state) becomes highly problematic. In short, according to Dobb capitalist entrepreneurialism has strong contradictory tendencies toward *both* overproduction *and* underproduction.

It is in Dobb’s first book, *CESP*, where he begins to develop these highly contradictory tendencies of capitalism (see *ibid.*: 379–387 for overproduction tendencies, and 157–174 and 361–377 for underproduction and stagnationist

tendencies of capitalism). Dobb points out that there is a difference between the *social* entrepreneurial function and the particular agent or agency that fulfills this social function (ibid.: 44ff). In capitalism the agents who fulfill and carry out the social entrepreneurial function are private, profit-motivated, individual entrepreneurs, or what Dobb called 'capitalist undertakers' (ibid.: 4–15). Thus, unique to capitalism is that a key social function, and arguably *the* key socioeconomic function (i.e. the social entrepreneurial function), is performed by profit-motivated private capitalist undertakers. In other modes of production, the social entrepreneurial function is instituted as a public good and carried out by an agent motivated by some sort of social interest.

The entrepreneurial myth itself is simply that capitalist undertakers are seen to be necessary for a vibrant economy, technological change, and growth. Dobb disputes this myth on two grounds. First, capitalist undertaking is historically and institutionally contingent (Dobb 1924 [1955]: 8). Second, and more importantly, capitalist undertaking makes socioeconomic crises immanent (Dobb 1925: 383–387).

The institutionalization of capitalist undertaking, in both its industrial and financial forms, occurs essentially through the process of primitive accumulation (Dobb 1946: 199). There are several historical currents and causes (ibid.: 221–254); nonetheless the important historical results are the monopolization of social resources and new class differentiation (Dobb 1925: 130–142). These results allow for particular differential advantage of the new monopoly class to 'claim a larger share of the income of the community than their fellows' (ibid.: 125).

According to Dobb this sharp new inequality disrupts the 'index of utility' (ibid.: 160) (note the Marshallian language). Every financial gain of an individual undertaker increases the inequality and lowers the marginal utility of money for the undertaker. Hence, the undertaker is in a more advantageous position to face the risk and uncertainty that accompany entrepreneurial activity and discourage others with higher marginal utility of money from taking risks, and encourages them to accept the capitalistic working conditions. The institutional presence of capitalist undertaking tends to augment inequalities whereby 'the rich tend to get richer, and the poor conversely to get poorer' (Dobb 1924 [1955]: 13) and inequality generates political instability. Worse still, argued Dobb, capitalist undertaking manifests immanence for socioeconomic miscalculation and maladjustment, that is, *crisis*. Here, Dobb argues that privatized undertaking requires calculation for the quantity supplied of other capitalist producers: 'Short of a system of espionage or the frank publication of business secrets and intentions, one can know scarcely anything at all' regarding the quantity supplied of one's competitors (Dobb 1925: 379). Thus, in *ignorance* of

the production plans of others, capitalist undertakers carry out investment and production in a veil of *uncertainty*, introducing a significant *risk* of financial loss.

Dobb contended that in the absence of sound calculation, oversupply becomes a strategic tendency. He writes, ‘There is abundant field for miscalculation; here, where basis for sound calculation is absent, emotional influences (business optimism, etc.) enter in’ (Dobb 1924 [1955]: 14). Thus, more than a decade before Keynes’s notion of ‘animal spirits’, Dobb underscored the importance of emotional influences of the investing classes for the determination of the levels of employment, output, and the (mal)adjustment of society due to the miscalculations of capitalist undertakers. Dobb explains there is a general tendency for the capitalist undertaking class to make miscalculations in the same direction (Dobb 1925: 380, 1937: 275). These universal miscalculations create ‘a sharp conflict between business anticipations and the actual facts’ (Dobb 1925: 384). The ‘actual facts’ manifest ‘severe maladjustment and wastage...over-capitalisation and over-production’ (Dobb 1924 [1955]: 14), ‘in a word, a crisis’ (Dobb 1925: 384).

The effect of *ignorance*, *uncertainty*, and *risk* upon the class of capitalist undertakers becomes devastating for society. The tendency toward oversupply will lead undertakers to curtail production and ‘to turn away as many [productive] resources and as much labour as they can and “the bonds which unite different enterprises will become channels through which the depression will spread to other enterprises”’ (ibid.: 386). This quote clearly demonstrates Dobb’s anticipation of Keynes’s theory of *effective demand* well before *The General Theory* as well as anticipating the rise to prominence of the multiplier.

A crisis will transform any previous optimism among undertakers ‘to pessimism and timidity ... The sudden disappointment will sap the strength of the capitalist spirit—destroy the undertaker’s self-confidence and his incentive to brave uncertainty’ (ibid.: 384). Thus, crises tend to be self-reinforcing due to a lack of effective demand and undertakers’ ‘pessimism and timidity’. In an economy dominated by capitalist undertaking, ‘[o]ptimism and pessimism alike will act as a rapidly spreading epidemic’. Boom and bust, in a word, crisis is immanent when the entrepreneurial function is in the hands of individual capitalist undertakers.

Dobb contends in his early work that there are tendencies within a capitalistic economy toward both political instability and economic inequality. The former because of the tendency toward severe inequality and the latter because of *immanence* of crisis due to ignorance, uncertainty, and risk. Let us emphasize Dobb’s employment of a Marshallian theoretical framework in *CESP*. Dobb correctly recognizes this to be limiting to the understanding of

social being. He began to understand a Marshallian theoretical framework, and neoclassical economics more generally fails to explain the distribution of income and wealth. As explained above, Dobb believed that returning to classical political economy would provide a theoretical road forward.⁹

By 1937, Dobb began to theorize crisis as not only immanent, but also *necessary* and *permanent*. In other words, he did not believe that Keynesian interventionism could overcome the crisis tendencies of capitalism.¹⁰ On the *necessity* and *permanence* of crisis Dobb drew heavily from Marx. However, Marx's theory of crisis was far from complete. Moreover, capitalism had changed since the time of Marx (Dobb 1957).

Drawing from the insights of volume II of Marx's *Capital*, the Marxian schemas of reproduction would become the foundation of Dobb's theory of crisis. Marx's incomplete analysis fails to develop an explicit theory of crisis. For Dobb the leading instability characteristic of capitalism is the social entrepreneurial function in the hands of private individual capitalist undertakers. Capitalist development does not result from any thought-out aggregate plan. In capitalism 'it just happens—accidentally as it were—as the result of a large number of autonomous individual decisions each of them taken in ignorance of other and parallel decisions, on the basis of market data *plus* guesswork or "expectations" as to future movements' (Dobb 1967: 56; italics in original). The significance of this observation is that there is, both, by definition and institutional design, 'anarchy of production'. This simply means that production decisions are made 'atomistically' and are supposed to be coordinated by the mechanism of price movements in markets (Dobb 1962: 30). According to neoclassical economists, capitalist production operates without regulation as long as there is sufficient competition (Dobb 1937: 37).

However, since the time of Marx's *Capital* oligopolization had become more pronounced (Dobb 1966b). Moreover, Dobb underscored the rise of big government and its intervention as well as the rise of big labor.¹¹ The rise of big business manifests in overcapacity utilization (Dobb 1927: 145–148) as a

⁹Dobb and Keynes each rejected marginal productivity theory. They both emphasized that the marginal productivity theory of wages was fundamentally misleading. However, Dobb and Keynes pose quite different critiques. More striking is that Dobb emphasizes an inadequate theory of entrepreneurial profits and the tendency toward industrial monopolization. Keynes's theory would underscore the importance of the determination of the rate of interest and the role of money.

¹⁰Dobb defends this position in his last book (Dobb 1973: 226–246), but was lecturing on the 'impossibility' of capitalist reform as early as the mid-1920s (Shenk 2013a: 64).

¹¹Indeed, in the mid-1920s, Keynes asked Dobb to contribute a book on *Wages* to the Cambridge Economic Handbooks series, of which Keynes was the general editor. In this book Dobb clearly anticipates John Kenneth Galbraith's *Theory of Countervailing Power*, to explain both the rise of big business and oligopolization as a competitive strategy and the emergence of big labor and big government as a related historical process. Dobb, however, gives these tendencies and processes no special name.

strategic weapon to use against rivals (Dobb 1925: 157–176) and union activity (Dobb 1927: 177–198, 1940b). In twentieth-century capitalism, Dobb argues, overproduction had become the general tendency of the system. He contends that the overproduction tendencies are ‘familiar’ to all economists, examples ‘consist in the chaotic duplication of railway facilities, the frequent overlapping of public utility services, the mushroom growth of shopping and entertainment facilities’ (Dobb 1937: 279).

The problem of overproduction is not necessarily a problem of underconsumption (*ibid.*: 87ff). Rather overproduction creates uneven growth between Department I (capital goods) and Department II (consumption goods), what Marx called a ‘crisis of disproportionality’. Marx assumes throughout volume II of *Capital* that balanced growth is achieved between departments, and thus is formally possible.¹² However, Dobb never tired of pointing out, there is *no* ‘actual tendency in capitalist society for these abstract conditions [of balanced growth] to be fulfilled—on the contrary, they were only observed “by an accident”’ (Dobb 1937: 102, 1942 [1955]: 196, 1973: 163). Economic crisis is the process of system adjustment. In this sense crisis is *necessary* for reproduction (Dobb 1937: 111, 1962: 69, 1973: 161–162). Thus, in counter-distinction to many mainstream theories, Dobb did not consider crisis as an abnormal occurrence, but the normal functioning of capitalism (Dobb 1959a). Instead, crisis is a *necessary* equilibrating force of capitalism when disproportionalities arise between departments and sectors.

Dobb also de-emphasizes the fall in the rate of profit as a primary cause of crisis (Dobb 1959b). He believed that technological change and the power of oligopolies could reverse the tendency of the fall in the rate of profit (Dobb 1966b). To be sure, a fall in the rate of profit was, according to Dobb, very important for understanding the structural dynamics of modern capitalism and the behavior of oligopolistic firms (Dobb 1937: 108–110). Certainly, a fall in the rate of profit could add to a crisis and contribute to secular stagnation (Dobb 1959b). However, Dobb believed the crisis tendencies of capitalism manifested regardless of whether the rate of profit is falling or rising. In short, a fall in the rate of profit is not *necessary* for the manifestation of crisis.

Dobb held that no policy could reform capitalism and overcome its structural tendency toward overproduction, disproportionalities, and crisis (Dobb 1966a: 27–38). In other words, (neo)liberal policy to curtail monopoly

¹²Marx’s schemas provided the basis for G.A. Feldman’s Soviet model of growth (see Domar 1957: 225–231). Marx’s schemas also anticipated the work of Léon Walras, and provide the basis of balanced economic growth reminiscent of the later models of Roy Harrod (1939) and Robert Solow (1956). Dobb was far less interested in the conditions of ‘balanced’ growth, but recognized its importance. He was more interested in explaining the conditions of instability, crisis, and stagnation.

powers, remove price rigidities, ease capital mobility, restrict labor union activity, and so on would fail. This is because even if they are implemented they would increase the accumulation process, and generate overproduction and disproportionalities. Indeed, according to Dobb, the more that (neo) liberal policies are put in place, the more that fluctuations would sharpen (Dobb 1924 [1955]: 13–14, 1949 [1955]: 105). In this sense, crises are a *permanent* feature of capitalist organization. Thus, Dobb establishes, much as Marx had before him (Clarke 1994), that there is an *immanence* of, *necessity* for, and *permanent* structural tendencies toward crisis in capitalist economies and when the entrepreneurial function is carried out by individual capitalist undertakers, motivated by profit.

5 Underdevelopment and the Virtues of Planning

The structural crisis tendencies of modern capitalism were compounded, according to Dobb, by the systematic tendencies toward structural underinvestment (Dobb 1937: 279) and underdevelopment (Dobb 1951, 1960). The structural underinvestment and underdevelopment tendencies of capitalism become especially problematic and acute in the so-called emerging economies (Dobb 1967: 71–88). In lectures developed at the Delhi School of Economics in India, ‘Dobb discussed some of the central issues of development planning for an economy with unemployed or underutilised labour’ (Sen 1990). Later these lectures were published as *Some Aspects of Economic Development* (Dobb 1951). Dobb maintained that if ignorance, uncertainty, and risk were too acute, this ‘may so inhibit investment decisions as to arrest growth entirely’ (Dobb 1960: 8). Underinvestment is ‘doubtless more important than we are generally aware since [it is] not brought to our notice as are the results of overinvestment’ (Dobb 1937: 279).

Dobb argued that industrially underdeveloped countries most often need state intervention for the initiation and successful development of a technological ‘take-off’ (Dobb 1960: 73). This is primarily because, according to Dobb, investment devoted to the capital sector (Marx’s Department I) tends to lag investment in other sectors (e.g. consumption, luxury, etc.) (ibid.: 66). The industrial ‘take-off’ fails to materialize because of the persistent problem of disproportionality. Most underdeveloped countries that attempt a capitalist path of development ‘are characterized by large reserves of [actual and disguised] unemployed’ (Dobb 1967: 32), disproportionalities, stagnation, and political instability.

In the absence of state intervention, the ‘planless economy ruled by the market individual *entrepreneurs*’ (Dobb 1960: 75; italics in original) tends to only provide moderate, or low, levels of industrial development in the capital sector, unless there are ‘short bursts of Schumpeterian optimism on the part of particular *entrepreneurs*, such as those that engaged’ in the nineteenth-century North American ‘railroad-race’ or there is industrial development ‘promoted by war’ (ibid.: 74; italics in original). In the absence of these atypical ‘bursts’, capitalism tends toward *stagnation*. Agricultural economies that employ the capitalist model tend to remain industrially underdeveloped (Dobb 1967: 71–89). The ‘[l]ack of the will or the incentive or the means to invest (or some mixture of all three) is apparently the crux of the prevailing stagnation’ (ibid.: 32) in the underdeveloped world attempting to industrialize via the capitalist road.

Let us summarize the two primary Dobbian insights we have developed above. First, Dobb argued that capitalism is contradictory and crisis-ridden. Second, capitalism is a radically unreliable system for industrial development. Notice that Dobb does not argue against markets per se.¹³ Rather it is the institutional role of particular *historical* agents and a particular *historical* social entity. More specifically, as explained above, Dobb underscores the contradictory mechanisms involved with capitalist undertaking.¹⁴ Second, Dobb is concerned with the oligopolization¹⁵ of the economy and the rise of

¹³Dobb’s position was that markets for consumption goods were relatively benign and consistent with socialism. Decentralization is superior in the consumption goods sector. Markets for the resources of production are where planning must take precedence. Dobb begins to develop this position during the early 1930s in his contributions to the ‘calculation debate’ (Dobb 1933, 1935, 1939b) and elaborates further in the 1960s and 1970s (Dobb 1960: 29–47, 1966a: 39–56, 1969: 121–152, 1970c, d, 1974). Shenk 2013a unfortunately says not once, but twice, that according to Dobb ‘socialism made markets work’ (ibid.: 56, 89). This is highly misleading. Rather, Dobb’s position was that planning in markets for resources would make society and the economy more stable. The reason for this is that planning would decrease the degree of monopolization of social resources, reduce the problems of excess capacity and stagnation, diminish the tendencies toward underproduction and overproduction, and therefore lessen the likelihood of disproportionalities between the consumption goods sector (Department II) and capital goods sector (Department I).

¹⁴By underscoring the contradictions generated by capitalist undertaking, it may seem that Dobb has become somewhat historically dated, in that financialization has superseded the role of the traditional capitalist entrepreneur. Undoubtedly, financialization has increased the importance of finance and financiers as economic agents, and demoted the importance of traditional entrepreneurs. Nonetheless, Dobb’s critique of the role of traditional entrepreneurs, that is, ‘capitalist undertaking’, can be directly applied to a critique of finance. Indeed, contemporary finance has compounded the problems outlined by Dobb. It should also be added that according to Dobb ‘the problem of industrialisation is essentially not a financial one, but a problem of *economic organisation*’ (Dobb 1951: 37; italics in original).

¹⁵Dobb’s political economy positions are highly consistent with the *monopoly capital* theory of Paul Baran and Paul Sweezy (1966) (see Dobb 1966b). The key motifs of the latter are ‘oligopolization’, ‘excess capacity’, and ‘stagnation’. Dobb was theorizing these same historical tendencies, and the systematic contradictions these phenomena generate, as early as 1925.

economic power in manipulating (so-called) market activity¹⁶ and economic and political outcomes.¹⁷

As outlined above, Dobb's contributions to political economy are massive. So far we have not even begun to address the contributions that he is most renowned for, that is, his research in economic history. His most celebrated work by far is his *Studies in the Development of Capitalism* (Dobb 1946). Granted, Dobb is not known for doing original historical analysis in his economic histories. Rather, his love for the subject of history was primarily a methodological entry point for theorizing about political economy.

6 Dobb's Philosophical Ontology and Methodology

Dobb did not theorize using abstract mathematical modeling. His philosophical ontology began with historical observations and stylized historical facts. This philosophical and methodological approach was crucial, according to Dobb, for understanding the internal articulation of an economy, or mode of production. With some emphasis, Dobb maintained that the lack of a historical understanding of social institutions, or an absence of social institutions characteristic of mathematical modeling, contributed to the 'mystification about the essential nature of capitalist society' (Dobb 1946: 32).¹⁸

¹⁶On the manipulation of market activity Dobb was, of course, heavily influenced by Sraffa (1926), Robinson (1933) and Kaldor (1934) at Cambridge; Dobb was further influenced by Harrod (1934) who was at Oxford (Harrod had spent some time at Cambridge in the early 1920s), and the American-based economists Chamberlin (1933) and Triffin (1940). Nonetheless, Dobb was writing of 'economic frictions' and monopoly power as early as 1924 (Dobb 1924 [1955]). Indeed, it can be argued that *CESP* (Dobb 1925) is a book especially concerning imperfect and monopolistic competition (which Dobb calls 'monopoly advantage' and 'effects of monopoly'). The evidence is that Dobb's work was a major influence on the development of the theory of imperfect and monopolistic competition.

¹⁷In this context, two points should be underscored. In Dobb's historical studies of the rise of capitalism, monopoly power and political manipulation of market outcomes is the primary characteristic. Thus, market corruption is characteristic of the system and not merely a function of temporary 'economic frictions' (Dobb 1924 [1955]: 7–15, 1937: 186–222). These so-called frictions are permanent constitutive features of the system. Monopoly-like power alters 'the nature of the equilibrating forces' (Dobb 1937: 187) and gives rise to tendencies toward disequilibrium (Dobb 1942 [1955]: 178–204), crisis (Dobb 1937: 79–126), and at the international level, various forms of market manipulation and 'imperialism' (Dobb 1937: 223–269). In this sense, Dobb is highly consistent with Polanyi's (1944) point that a self-regulating market system is an illicit utopia. Dobb anticipates the historical tendencies toward political corruption, or what James Galbraith (2008) has recently dubbed 'the predator state'.

¹⁸Shenk (2013a: 185) captures Dobb's position well when he writes: 'The bulk of [Dobb's] colleagues... had been seduced by promises of formal sophistication into contriving models of dazzling complexity whose premises had become increasingly detached from the actual mechanics of economic life. It was a savvy way to get tenure, but an awful mode of economic analysis. The triumph of a cult of mathematics had left the discipline vulnerable to what the book's subtitle [Dobb 1969] labeled "a commonsense cri-

Dobb's methodological achievement is impressive, although underappreciated. Amartya Sen has underscored 'Dobb's deep concern for descriptive richness' (Sen 1990: 33). Cambridge economist Tony Lawson (1997) has praised Dobb's methodological insights in general and underscored his *process of abstraction* in particular. Indeed, Dobb's philosophical ontology anticipates and shares many commonalities with Lawson's support for critical realism (Despain 2011: 495–528 models Dobb's [implicit] philosophical underpinnings and methodology).

Dobb insisted, with Marx, that social science requires *a process of abstraction*. As Marx wrote, 'in the analysis of economic forms neither microscopes nor chemical reagents are of assistance. The power of abstraction must replace both' (Marx 1976: 90). In its most simple formulation, the *process of abstraction* is the emphasis on certain aspects of something to the (momentary) demphasis of other aspects (Lawson 1997: 227).

This process of abstraction is central to all science. When a science attempts to achieve a more precise refinement and comprehensiveness, 'abstraction is required' (Dobb 1937: 4). Human beings must employ the process of abstraction to comprehend any moment of reality: 'Our minds can no more swallow the world whole at one sitting than can our stomachs' (Ollman 1993: 24). Space does not allow us to unfold the details of Dobb's philosophical ontology (see Despain 2011: 495–528).¹⁹ Suffice it to say that Dobb was a precursor to, and developed a position quite consistent with, Lawson's critical realist philosophy of science.²⁰

Dobb's methodological and philosophical achievements are substantial. He rejected the empiricism and the logical positivism that were in vogue dur-

tique." No amount of mathematical fireworks, he charged, could redeem models based on shaky logic and cartoonish assumptions about human behavior'.

¹⁹Despain (2011) argues that Dobb's process of abstraction rests on five basic theses, one of which is more methodological and the other four more ontological. First, the 'theory thesis': Theory is necessary to both scientific activity and historical analysis alike. The second is the 'material thesis': Ideas and motives of human beings are conditioned by practical and material experience. The third is the 'internal articulation thesis': Societies are structured and differentiated sets of social relations. Fourth is the 'historical thesis': Social relations are transitory; hence so, too, is theory. Finally, the fifth is the 'agency thesis': All human action potentially has epoch-making effects.

²⁰This is not at all surprising when we consider that Lawson argues for important philosophical affinities between critical realism and Post-Keynesianism (Lawson 1994, 1999). Certainly, Dobb was an important Cambridge Post-Keynesian very familiar with the methodological disputes between Post-Keynesianism and other Keynesians, and between Post-Keynesianism and economics more generally. Likewise, many theorists have drawn attention to the affinities between critical realism and Marxism (see the articles in Brown et al. 2002). Thus, Dobb's strong affiliations to Post-Keynesianism and Marxism would suggest on the surface that we would expect positions consistent with critical realism.

ing the first 70 years of the twentieth century.²¹ He further insisted that the boundary lines between social sciences such as history, economics, sociology, anthropology, and psychology were arbitrary and obscured human problems and an understanding of historical conditions and social being more generally (Dobb 1946: 32, 1951: 230). Dobb's groundbreaking path, especially in *Studies*, influenced and inspired many historical theorists to overcome these boundaries. He most certainly should be seen as an initiator of the convergence of history and social theory. It is these achievements that maintain Dobb as a rich source of theoretical and political-economic insights. Dobb recognized, as did Marshall, that classical political economy did not suffer from the same *ahistorical* ontological pitfalls of neoclassical economics and the empiricist tendencies of the new emerging econometrics.

Dobb demonstrated that classical political economy, especially Marxism, beheld a multitude of ontological and theoretical virtues that had been absented following the 'Jevonian revolution' (Dobb 1973: 166–210) and neo-classical developments (Dobb 1969: 3–116). He was painfully aware that his attempt at an ontological shift would be met with resistance and skepticism. In the 'Preface' to *Studies* Dobb writes:

A work of this kind, which is concerned with generalizing about historical development on the basis of material already collected and arranged by other hands, runs a grave danger of falling between two stools, and of displeasing both the economist, who often has little time for history, and the historian, who may dismiss it as insufficiently grounded in the first-hand knowledge that comes from actual field-work. To the economist the author may appear as an irrelevant wanderer from his proper territory, and to the historian as an intruding amateur. Of this danger and of his own imperfect equipment for the task the author has, at least, not been unaware. He has, nevertheless, been encouraged to persevere by the obstinate belief that economic analysis only makes sense and can only bear fruit if it is joined to a study of historical development, and that the economist concerned with present-day problems has certain questions of his own to put to historical data. He has been fortified by the conviction that a study of Capitalism, in its origins and growth, so much neglected by economists (other than those of a Marxist persuasion), is an essential foundation for any realistic system of economics (Dobb 1946: vii).

In 1946 perhaps it was merely an 'obstinate belief' to maintain that 'any realistic system of economics' depends upon an appropriate convergence of

²¹ In fact, Dobb's primary complaint against Robinson's interpretation of Marx (Robinson 1942) was that she was overly reductionist. Her reading of Marx was dogmatically as a logical positivist (see Kerr 2007).

history and theory as ‘an essential foundation’ of social science. However, 70-odd years since the publication of *Studies*, Dobb’s methodological ‘wanderings’ and philosophical stubbornness have now found substantial intellectual warrant within the disciplines of history, social theory, political economy, and philosophy.²²

7 British Marxian Economic Historians

For Dobb the warrant for the convergence of history, social theory, political economy, and philosophy is in the power of the particular sociohistorical analysis to illuminate the actual historical processes of human experience (Dobb 1951: 235). Much of the historical processes that had been dim behind the shadow cast by an empiricist methodology have now been illuminated by the historians directly influenced by Dobb. Collectively, they are known as the Communist Party Historians Group; their paradigm metaphor is ‘history from below’.

It was Dobb’s *Studies* that had directly inspired this tradition of historians (see Hobsbawm 1979: 23; Hill 1950: 315; Hilton 1947: 29–30; Kaye 1984, 1992; Schwarz 1982). Initially, the first generation of this group had come together in England in 1945 to discuss the second edition of A.L. Morton’s *A People’s History of England* (original published in 1938). The group included some of the twentieth century’s most prominent names in Marxian (socio-) historiography. Besides Dobb, regular attendees included Christopher Hill, Rodney Hilton, Eric Hobsbawm, Victor Kiernan, George Rudé, Dorothy Thompson, E.P. Thompson, and Dona Torr.

All of the members of the Group were also members of the Communist Party of Great Britain. Dobb’s *Studies* (Dobb 1946), published within a year of the Group’s formation, became a great intellectual interest to many of the Group’s members (see, for example, Hilton 1947; Hill 1947; Hobsbawm 1979). Especially significant to the participating members was Dobb’s ‘stages of development’ analysis that he employed in *Studies*.

In *Studies*, chapters two through six can be divided into roughly five distinct stages of development analyzed by Dobb. In each stage of development, Dobb’s

²²The more prominent historians combining history and social history include Barrington Moore, Jr., Reinhard Benedix, Alvin Goulder, Daniel Bell, Fernand Braudel, Perry Anderson, Charles Lemert, and Immanuel Wallerstein. As Anthony Giddens (1979: 230) correctly asserts, ‘There simply are no logical or even methodological distinctions between social sciences and history – appropriately conceived’. Dobb should certainly be understood as inspiring and helping to bring about a convergence between history, social theory, political economy, and philosophy.

analysis is driven methodologically by primacy given to the ‘pathological’ or crisis moments of history. The first stage is the ‘crisis of feudalism’ in the late thirteenth and early fourteenth centuries. The second stage is the *bourgeois revolution* of the seventeenth century. While the second stage is a reactionary response in an attempt to ‘save’ feudalism, and hence remains part of the development of feudalism, stage three is a movement away from feudalism. More specifically, stage three is the aftermath of the English bourgeois revolution during relatively the same time period as the second stage. The third stage initiated the rise of *industrial capital* and historically constitutes a prelude to capitalist development, and a more rapid deterioration of the feudal mode of production. The fourth stage is the social historical formation, or ‘making’ of a *proletariat class* dependent solely on wages for their livelihood. With the formation of a proletariat class, *and only after* the formation of the proletariat class, the conditions are set for the so-called industrial revolution in the late eighteenth and early nineteenth centuries, which is the fifth (Dobbian) stage of development.

As mentioned above, each stage of economic development more or less corresponds to a chapter of Dobb’s *Studies*. Further, each stage would become a research agenda for, in order of their respective stages, Hilton, Hill, Thompson, and Hobsbawm. Hilton would take up the research of crisis and class conflict in thirteenth- and fourteenth-century (English) feudalism (stage one), Hill would focus his intellectual efforts on sixteenth- and seventeenth-century England (stages two and three), Thompson would write one of the most celebrated monographs in Marxian historiography concerning *The Making of the English Working Class* (to invoke the title of his book) (stage four), and Hobsbawm would research the industrial revolution²³ of England and the (more political) French Revolution during the late eighteenth and early nineteenth centuries (stage five).

8 *Studies in the Development of Capitalism*

It was Dobb’s *Studies* that provided the basic framework for these historians (see Hobsbawm 1979: 38). Dobb’s influence also inspired a methodology and specific interpretation of Marx and Engel’s historical materialism, an interpretation quite at odds with the orthodox deterministic versions of historical materialism that ruled the era (e.g. Plekhanov’s *The Development of the Monist*

²³ Dobb, Hobsbawm, and Thompson were in agreement that the notion of an ‘industrial revolution’ was a word too many. There was an industrial ‘take-off’ that occurred only after the centuries-long process of primitive accumulation. The process of primitive accumulation was as much a process of dispossession of the masses as it was the enrichment of the landlords and ‘merchant element’ (Dobb 1946: 221–254; Thompson 1963: 182–447).

View of History, originally published in Russian in 1894 and later defended by Gerry Cohen in his *Marx's Theory of History: A Defence*, published in 1978). A study of the writings of these Marxian historians will further enrich the methodological insights and ontological orientation of a Dobbian approach to social history. Although the historical periods of research of these historians are distinct, and the methodological emphases and biases may vary, it is suggested that these historians constitute a theoretical tradition (Kaye 1984; Despain 2011: 157–262).

Broadly, Dobb sets out to accomplish two aims in *Studies*. First, he endeavored to critique the ‘commercialization model’ of capitalism and better understand the dynamism of feudalism. Second, he wanted to better understand the crisis and stagnationist tendencies of contemporary capitalism by better understanding its historical genesis and institutional development.²⁴

According to the commercialization model, the extension of long-distance trade, then the development of domestic exchange, and consequently, the rise of domestic markets are the primary dissolvents of feudal relations of production. Moreover, trade and markets are seen as alien or external forces operating outside of the internal structure of both feudal production and feudal exploitation. In turn, markets and money are seen as the catalyst for the emancipation of feudal peasantry from serfdom. The presence of markets and money, furthermore, are seen to have unleashed improvements in technology from the fetters of feudalism (Dobb 1946: 37–38). In that these forces are viewed as alien and external to the internal structure of feudalism, the internal social constitution of feudal production itself is viewed as (more or less) stable, save for the external pressures of markets and money. Moreover, in that the commercialization model assumes that feudalism was characterized by its particular stability, this implicitly suggests that the feudal mode of production tended to be technologically stagnant.

According to the commercialization model then, not only are markets and money exchange the ‘historical destroyers’ of feudal relations of production, but they also further engender the capitalist relations of production. Monopoly merchants, or the mercantile element of feudalism, in this view, are crowned as the principal begetter of emancipation of human beings from the shackles of feudal institutions.

Dobb’s dissatisfaction with the commercialization model is threefold: (1) the internal articulation is under-analyzed, hence the economic dynamism

²⁴ Significant attention has correctly been given to Dobb’s chapters on feudalism and especially the mechanisms of ‘transition’ from feudalism to capitalism. (The original debate is published in Hilton (ed.) 1976; also see Despain 2011: 110–156.) Nonetheless, nearly half of *Studies* is concerned with capitalism and approximately a third of the book with contemporary capitalism. These highly insightful pages have been relatively neglected and underappreciated.

of feudalism remains obscure; (2) it is misleading on several accounts with respect to the transition from feudalism to capitalism; and (3) it overestimates the revolutionary role of the feudal mercantile element on the one hand, while on the other hand, it underestimates the conservative role of merchant capital in sustaining the social relations of feudal production.

In spite of the tremendous attention that the transition from feudalism to capitalism received following the publication of *Studies*, it was not Dobb's primary concern for writing the book. Without exaggeration, the main intention of *Studies* was to understand the stages of economic development, growth, and crisis of capitalism. To do this, Dobb believed that he first needed to understand the transition that took place. First, in a contrastive (or scientific) sense, the structural dynamic of capitalism could be analyzed from, and compared with, the differences of the structural dynamic of feudalism. Second, Dobb believed that history is always present. The idea of history always being present refers to the fact that social structure, modes of production, and institutions always evolve from those of the past. Contemporary institutions are always rooted in previous institutions, and in part, constitute them.

For Dobb, history is a necessary endeavor to understand contemporary social being. Also, it was necessary to understand the institutions, the evolutionary development, and the contemporary forms of social being. Dobb's approach to contemporary social being was not only historical, but also structural, institutional, divided into stages (of development), and orientated around the concept of agency. Dobb's notion of social class includes all of these methodological motifs.

Social class is structural in that it is determined and defined by the mode of production. Social class is institutional in that it is mediated by the institutional forms. Stages of economic development (contingently) modify social class (and sociopolitical alliances). Finally, social class is the predominant aspect that determines political motives and social action. It is in this sense that for Dobb social class is the primary category for understanding the reproduction of any social arrangement or mode of production. The social class of an individual determines the ways in which a person flourishes in (or is enabled by) and suffers in (or is constrained by) a society.

Dobb not only turned to history to help understand contemporary social class and social being, but he also believed that a return to classical political economy, and especially the work of Marx, was of the utmost importance. The return to Marx was important for Dobb in that it was Marx who first emphasized both class struggle analysis and *economic crises*. Crises, of course, *do not* explain transformation or 'transition' from one system to a new one. Crises produce the opportunity for transformation, but only the actual historical agents

can do the transforming. Crises have an additional importance to social science, which Dobb was anxious to understand. Methodologically, crises allow a theorist analytical access to the ways in which a system is either reproduced or transformed by means of understanding the historical episodes when a system fails to reproduce itself.

It is indeed striking to discover that Dobb's main aim in *Studies* was to develop a theory for the self-regulation and reproduction of a capitalist economy. For Dobb to understand the political economy of twentieth-century capitalism, it was necessary to understand its institutions and history, and its historical emergence. It was to this aim that the historical chapters of *Studies* concerning feudalism and precapitalism were intended. What is remarkable is that these chapters contributed to the degree that they did for understanding feudalism and its mode of self-regulation, reproduction, and eventual transformation.

Dobb applies his historical reflections on the roots of capitalist institutions to the analysis and theoretical construction of twentieth-century capitalism. In short, *Studies* was a story of capitalism, first and foremost, with an aim to understand its stagnation, instability, and crisis tendencies in the twentieth century.

9 Conclusion

Dobb's intellectual achievements are extraordinary. He contributed significantly to our understanding of feudalism, economic development, socialist planning, Soviet history and development, history of economic thought, the historical story of capitalism, imperialism, labor economics, welfare economics, crisis theory, and to the dynamic of contemporary political economy. His relative neglect is surprising. It is shocking when it is recalled that he was a premier, if not *the* premier Marxian political economist of his generation and a Faculty member of the foremost political-economic institution of the era in Cambridge University.

Dobb's academic work remains a wealth of insight for aspiring political economists, historians, and social theorists. The great financial crisis of 2007–2008 and the European collapse of 2009–2010 have inspired numerous economists to rethink the instability (see, for example, Reinhart and Rogoff 2009; Galbraith 2014; Eichengreen 2015) and stagnationist tendencies (see Gordon 2016; Summers 2013; Despain 2015) of capitalist organization. An excellent place for a young, aspiring political economist to gain an understanding of the problems of capitalist organization and its alternatives would be to read Maurice Herbert Dobb.

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29

Frank P. Ramsey (1903–1930)

Pedro G. Duarte

1 Introduction

‘An enthusiast for the public welfare and for the discovery of mathematical truth’. With these words, the 20-year-old Frank Ramsey described himself in a talk he delivered on January 1924 (Ramsey 1924 [1991]: 311). While certainly indicating a self-image and identity that are historically constructed, these words serve the purpose here of indicating the wide range of Ramsey’s interests and contributions. Despite Ramsey’s short publishing career, that lasted for only eight years, ‘in a series of brilliant essays [he] laid the foundations of several new and flourishing theories in philosophy, mathematics and economics’, as Nils-Eric Sahlin (1990: ix) wrote.¹ From logic to the foundations of mathematics, to combinatorics, to philosophy and probability, and to economics, Ramsey is venerated as having produced profound and original contributions.

In economics, Ramsey published two major papers in the *Economic Journal*, on taxation in 1927 and optimal savings in 1928, under John Maynard Keynes’s editorship. After the Second World War, Ramsey became a sacred predecessor in four important fields of economics (Duarte 2009a: 445–446, 2009b: 165–169). First, his criticism of Keynes’s *A Treatise on Probability* (Keynes 1921 [1973]), published in 1922 and developed further in 1926

¹ See similar words by D.H. Mellor in Ramsey (1990: xi).

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(Ramsey 1922a, 1926), put forward ideas on subjective probability that in the 1950s became important to the subjective decision theory and expected utility literature.² Second, his 1928 paper received growing attention in the economic growth literature of the 1960s, with the works of David Cass and Tjalling Koopmans, leading to the emergence of the so-called Ramsey-Cass-Koopmans model of optimal growth and later related to the economics of conservation and exhaustible resources (cf. Duarte 2009b; Erreygers 2009). Third, his 1927 contribution to taxation became a central reference in the public finance literature. William Baumol and David Bradford (1970: 277–280) borrowed a general storyline from Paul Samuelson (1964: 95) which placed Ramsey's mathematical proof as a critical development in the history of the literature on departures from marginal cost pricing. This interpretation made its way into Peter Diamond and James Mirrlees's (1971a, b) articles which, according to Samuelson (1982 [1986]: 98, fn. 9), are 'the spring from which all modern discussion flow'.³ Fourth, with public finance ideas later becoming important to discussions on optimal economic policies, Ramsey (1927) assumed also the role of a distinctive precursor in monetary economics, with economists nowadays having adopted the language of solving a 'Ramsey problem' to characterize an optimal monetary policy.

This canonization of Ramsey in the post-Second World War economics literature came together with a view that he employed a mathematics too advanced for his peers to be able to appreciate his contributions (Duarte 2009b, 2010). Therefore, the story goes, roughly three decades had to elapse for economists to have the appropriate mathematical knowledge to finally comprehend Ramsey's ideas. Such narrative has a Romantic mould typical of the historical accounts of mathematicians from the nineteenth century: contrary to eighteenth-century mathematicians, who were important public figures at a time when mathematics aimed at understanding the natural world, mathematicians of the following century were seen as lonely individuals worried about logical rigor and internal consistency of their field (Alexander 2006). The tragic, Romantic narratives of mathematicians, as Alexander (ibid.) discussed, are well represented in the popular book, *Men of Mathematics*, published in 1937 by the mathematician and science fiction writer E.T. Bell.

² See Heukelom (2014: Chapter 2) for a historical discussion of the development of this literature. See also Soifer (2009: 284–246). Mellor (2005) discusses whether the subjective decision theory that incorporated Ramsey's ideas should be interpreted as a positive or a normative theory.

³ See Duarte (2010) for a historical discussion on the canonization of Ramsey's 1927 article in the public finance literature of the post-war period.

While the mythical view on Ramsey certainly poses important historical questions of how, and with whose help, his contributions secured a reputable place in post-war economics, the view of Ramsey as someone ahead of his time and an outsider to economics was clearly articulated by Keynes in the obituary he published in 1930 in the *Economic Journal* after the death of his young friend:

When he did descend from his accustomed stony heights, he still lived without effort in a rarer atmosphere than most economists care to breathe, and handled the technical apparatus of our science with the easy grace of one accustomed to something far more difficult ... [His 1928 paper] is, I think, one of the most remarkable contributions to mathematical economics ever made ... The article is terribly difficult reading for an economist, but it is not difficult to appreciate how scientific and aesthetic qualities are combined in it together (Keynes 1933 [1972]: 335–336).

The philosopher G.E. Moore, much admired by Ramsey (Duarte 2009a: 455, fn. 24), also praised Ramsey as someone who ‘seemed to me to combine very exceptional brilliance with very great soundness of judgment in philosophy. He was an extraordinarily clear thinker ... He had, moreover, an exceptional power of drawing conclusions from a complicated set of facts’ (Moore quoted in Ramsey 1931 [1960]: vii–viii).

Roy Harrod was at Oxford and went to the University of Cambridge in the autumn of 1922 to interact with Keynes when he became a close friend of Ramsey. Harrod (1951) described Ramsey very affectionately, as ‘a man of extreme brilliance and precocity’ (ibid.: 320) and the one who, ‘more than any others of the post-war vintage, seemed to embody the intellectual and personal ideas that were cherished in Cambridge at the opening of the [twentieth] century’ (ibid.: 398).⁴

It is true that the early and sudden death of Ramsey, roughly a month prior to his 27th birthday, ‘make[s] it hard to disentangle what he was really like [and what his relationship with other people was like] from the shocked exclamations of grief and praise that naturally followed his death’, as his sister wrote in her memoir (Paul 2012: 223). Indeed, Keynes had expressed in two letters to his

⁴ Goldsworthy Lowes Dickinson, a Kingsman, also saw Ramsey as a typical Cambridge man, someone who is ‘unworldly without being saintly; unambitious without being inactive, warm-hearted without being sentimental. Through good report and ill such men work on, following the light of truth as they see it; able to be sceptical without being paralyzed; content to know what is noble and to reserve judgement on what is not. The world could never be driven by such men, for the springs of action lie deep in ignorance and madness. But it is they who are the beacon in the tempest and they are more, not less, needed now than ever before’ (Dickinson quoted in Martin (1966 [1969]: 109), and used by Keynes in the obituary of C.P. Sanger he published in 1930 in the *Economic Journal*).

wife, Lydia Lopokova, immediately after learning about Ramsey's death that 'it is so terrible when a young person dies', and that Frank 'was in his way the greatest genius in [King's] college, and such a dear creature besides' (Keynes quoted in *ibid.*: 268). Nonetheless, during his lifetime, Ramsey was admired by and the friend of such Cambridge luminaries as Moore, Keynes, Bertrand Russell, Ludwig Wittgenstein, Piero Sraffa, the geneticist, mathematician, and later leading psychiatrist Lionel Penrose, the literary critic I.A. Richards, the English linguist and polymath C.K. Ogden, and the mathematician-turned-philosopher Richard Braithwaite, a close friend of Ramsey who edited the first posthumous collection of his papers (Ramsey 1931 [1960]). Braithwaite wrote in an obituary for *The Cambridge Review*: 'By his second year [Frank] was accepted as the arbiter of good reasoning on every subject. For 8 years, if an abstruse point arose in philosophy, psychology or economics, the question was: "What does Frank Ramsey think of it?"' (Braithwaite quoted in Paul 2012: 273).

Ramsey was also the friend of James and Alix Strachey, who later published the English translation of Sigmund Freud's complete works. James was the youngest brother of Lytton Strachey, an eminent biographer and one of the founders of the Bloomsbury Group, which congregated intellectuals and artists, including Keynes, and had an important impact on British culture during the first decades of the twentieth century. Several members of this group were former Cambridge students and some were members of one of the most distinguished societies in Cambridge's intellectual life, the Cambridge Conversazione Society, or the Apostles. As Donald Moggridge (1992: 66) argued, the importance of the Society was due to its longevity and 'the intellectual importance of many, although far from all, of its members'. Keynes, Russell, Moore, Wittgenstein, Braithwaite, Lytton Strachey, E.M. Forster, Leonard Woolf, Roger Fry, Lionel Penrose, and Frank Ramsey were some of the distinguished members of the Apostles.⁵ Ramsey mixed with the Strachey brothers and was acquainted with other Bloomsbury figures such as Virginia Woolf, Forster, Fry, and also Frances Partridge and Dora Carrington (Paul 2012: 112, 140). Virginia described Ramsey as 'something like a Darwin, broad, thick, powerful, and a great mathematician, and clumsy to boot. Honest I should say, a true Apostle' (Duarte 2009a: 454).

Ramsey was thus inarguably immersed in the Cambridge milieu of the 1920s. He interacted closely with leading economists of the first half of the

⁵The society was founded in 1820, and Paul Levy (1979 [1989]: 300–311) lists the members elected up to the First World War. Similarly, William Lubenow (1998: 413–432) provides a biographical directory of the Apostles of this period. See also Richard Deacon (1985).

twentieth century, such as Keynes, A.C. Pigou, and Harrod (Duarte 2009a), and he was canonized immediately after his death by Cambridge notables like Keynes and Moore. However, his two major contributions to economics only secured him a central place in the discipline after the Second World War, when Ramsey's ideas crossed the Atlantic and were nurtured by economists in the USA.⁶ Therefore, Ramsey and his works provide an interesting window both into the Cambridge environment of the 1920s and to the transformation of economics in the post-war period. In the next section, I discuss in detail Ramsey's life and the University of Cambridge in the 1920s. I then summarize Ramsey's contributions to economics in Section 3, and briefly mention elements of the rediscovery of Ramsey in post-war American economics in Section 4. Section 5 concludes the chapter.

2 The Cambridge Road to Genius

Frank Plumpton Ramsey was the first child of Arthur Stanley Ramsey and Agnes Mary Wilson, born yellow with jaundice on 22 February 1903. The family was completed with the birth of Michael (1904), Bridget (1907), and Margaret (1918). His brother Michael occupied the highest post in the Church of England, being the Archbishop of Canterbury from 1961 to 1974, and Margaret became an economist at St. Hilda's College and also a Fellow of Lady Margaret Hall, Oxford.⁷

Agnes was born in 1875 and married Arthur in 1902. She went in 1896 to St. Hugh's Hall, Oxford, to study history. From there she went to teach history at East Putney High School, giving up teaching after two years. She nurtured strong interests in socialism and feminism since her high school years in Bristol. Before the First World War, she was highly interested in the women's movement, in its peaceful wing, working for the 'suffragists', having the support of Arthur. She then opposed the war and did social work to improve the lot of the unemployed, being also instrumental in organizing free milk in British schools. She had a permanent association with the Labour Party, a political outlook later shared by Frank. She died in 1927, two years before her eldest son, when the car driven by Arthur had an accident (Paul 2012: 14–21). It seems that Frank was

⁶It is quite telling to the centrality of Ramsey in post-war economics that the University of Cambridge has no professorship named after either Keynes or Marshall or Pigou or other distinguished Cambridge economists, but it does have one named after Ramsey. However, this Professorship appeared only in 1994 for the tenure of Sir Partha Dasgupta. In 2011, it was permanently retitled the Frank Ramsey Professorship of Economics (Cambridge University Reporter 2011).

⁷The detailed information on Ramsey's family background comes mostly from Paul (2012).

overdependent on his mother, but he was also the child who was most able to put up with Arthur's harsh treatment of Agnes and he developed a 'serene and affectionate relationship' with him, according to Paul (*ibid.*: 13).

Arthur was born in 1867 and had trouble entering college. After being rejected in entrance exams of four colleges, he got a scholarship to go to Magdalene College, Cambridge, where he read mathematics and succeeded very well in the Mathematical Tripos: sixth in the whole University in Part I and third in Part II. It was common at the time that Cambridge colleges appointed one of their own alumni to their Fellowship, but Magdalene already had a mathematics Fellow. Arthur then got a job in a Scottish public school, where he spent six years. Then, in 1897, he became a Fellow of Magdalene, being instrumental in changing the then poor intellectual state of the College: according to him, Magdalene, which had no intellectual test for admission, 'was a refuge for boys who could not get in elsewhere' (*ibid.*: 6). Arthur was bursar, president (i.e. vice-Master), and acting Master (1917–1920) at Magdalene, where he stayed until his death in 1954, although he had stopped teaching in 1934 (*ibid.*: 5–14). As evidence of the improvement in the College's academic status during Arthur's lifetime, Paul (*ibid.*: 11) wrote that '[i]n Arthur's last year, five of his six finalists were placed in the first class. Several of his pupils obtained distinctions in the tripos (a rare honour) and themselves became university teachers of mathematics'.

Arthur was apparently a good teacher, and he specialized in applied mathematics (i.e. classical mechanics), writing several highly praised and used textbooks. In 1894, he wrote with George Richardson a book for the 'Association for the Improvement of Geometrical Teaching', entitled *Modern Plane Geometry*. In 1913, he became coauthor of *A Treatise on Hydromechanics*, originally published in 1859 by the Cambridge mathematician William Henry Besant as a single volume; Besant turned it into two volumes in 1882 but did not manage to publish the second volume, thus inviting Arthur in 1904 to be responsible for Part II on 'Hydrodynamics'.⁸ However, as Arthur wrote in the Preface to the first edition (of the two-volume book), he found it 'desirable to write a new book *ab initio*' (Arthur Ramsey 1935 [1960]: v). In 1914, Arthur published the revised and enlarged fifth edition of Besant's *A Treatise on Dynamics*. More interesting is to note that he also wrote several textbooks aimed at stu-

⁸ Besant was a Fellow and mathematical Lecturer at St. John's College, Cambridge, and died in 1917. In 1850, he was first in the second part of the Mathematical Tripos (i.e. Senior Wrangler), and became Fellow of the Royal Astronomical Society in 1871. 'He gained a great reputation as a mathematical coach' and served as examiner for the Mathematical Tripos in 1856, 1857, and 1885 (Royal Astronomical Society 1918: 241). Based on his experience of preparing Cambridge students for the tripos, Besant wrote in 1863 the textbook *Elementary Hydrostatics* containing mathematical exercises of the kind found in the examination.

dents preparing for Part I of the Mathematical Tripos at Cambridge, many of them based on his own lecture notes: *Elementary Geometrical Optics* (1914), *Dynamics* (1929), *Statics* (1934), *Hydrostatics* (1936), and *Electricity and Magnetism: An Introduction to the Mathematical Theory* (1937).⁹ What these last books reveal very clearly is what E. Roy Weintraub (2002: Chapter 1) discusses, that the mathematics taught in Cambridge at the time was disconnected from developments occurring on the Continent in the field of Analysis: in nineteenth-century England and still in Arthur's textbooks, 'mathematics was [...] defined [...] by a set of tricks and details, based on Newton, which were linked to applied physics and mechanics' (ibid.: 14). Arthur Ramsey's books were among the mathematics books read by his son (Paul 2012: 47).¹⁰

Applied mathematics was taken to task in Cambridge after the First World War, and it was understood then as seriously out of date. The Trinity Mathematical Society debated this point and a motion was approved to revise it. Ramsey took part in this discussion, as recorded in this motion from 1922 (ibid.: 11):

That in the opinion of this house applied mathematics should be instantly and radically revised. F.P. Ramsey said that applied mathematics had afforded no stimulus to advance since the middle of the eighteenth century. All the recent advances in mathematical physics had been made by pure mathematicians. With regard to applied mathematics as a subject of study, he was of the opinion that it would be better to do away with it in the university, as it was merely a collection of standardized puzzles.

It is not surprising that the young Ramsey would take this position in 1922 as his interests and later works in mathematics came from his familiarity with the 'symbolic school' of mathematics, as defined by the works of Russell (in his *The Principles of Mathematics*, of 1903, and the three-volume *Principia Mathematica*, published with A.N. Whitehead in 1910, 1912, and 1913), following Gottlob Frege, and of Louis Couturat, a French mathematician born in 1868. For these authors, the foundations of mathematics lay in logic, and Couturat believed that symbolic logic was the way to advance mathematics and philosophy. In 1918, at the age of 15, Frank wanted to expedite his learning

⁹His 1929, 1934, and 1936 books had as a subtitle 'a text-book for the use of first year students at the universities and for the higher divisions in schools'. Arthur Ramsey also wrote a textbook for Master's students of mathematics entitled *An Introduction to the Theory of Newtonian Attraction* (1940). As evidence of the importance of his books, Paul (2012: 10) wrote that Arthur 'used to boast that the 100,000 copies of them that were sold in his lifetime, if stretched out, would have reached to the moon'. Several titles were recently reprinted by Cambridge University Press, featuring in the catalog of 'books of enduring scholarly value', in the series 'Cambridge Library Collection'.

¹⁰At Winchester, in 1918, Frank even attended his father's lectures on dynamics (ibid.: 49).

of German and read a German translation of Couturat's book—*Les Principes des Mathématiques: avec un Appendices ur la Philosophie des Mathématiques de Kant* (1905)—which he understood to be 'founded on B. Russell (*Principles of Mathematics, vol. 1*)' (Ramsey quoted in Paul 2012: 46). In Cambridge in 1920–1921, he attended Russell's lectures, traveled to London with his close friend Ogden to talk to Russell, discussed with the latter concepts of *Principia Mathematica*, and attended Moore's lectures, including one on logic for which he read the introduction of Russell and Whitehead's *Principia*. He also reported to his father that he read some work of the British mathematician G.H. Hardy and he did not like it much (ibid.: 46, 81–82, 95, 98).¹¹

This shows how Ramsey was familiar with a different kind of mathematics, not applied mechanics, that was beginning to appear in England, in which discussions on the foundations of mathematics were typical. This was exactly Ramsey's concerns in the work he did for his Fellowship dissertation, which generated the 1925 article 'The Foundations of Mathematics', originally published in the *Proceedings of the London Mathematical Society* (and reprinted in Ramsey (1931 [1960]: Chapter 1). In an entry to the fourteenth edition of the *Encyclopedia Britannica*, Ramsey (1929) discussed the paradoxes emerging from Russell and Whitehead's theory of types, and the use they made of the Axiom of Reducibility to circumvent them. Such an axiom was 'generally considered unpalatable and unsatisfactory' (ibid.: 84), being criticized by the 'intuitionist school' of Brouwer and Weyl, and by the 'formalist school' of Hilbert. Meanwhile, Wittgenstein, who interacted with Frege and wanted to study with him, was also critical of *Principia Mathematica* as he developed in his book *Tractatus Logico-Philosophicus*, which Ramsey helped translate into English in 1922. Ramsey wanted to show that 'using Wittgenstein's work the system of Principia Mathematica can be reconstructed so that the unsatisfactory Axiom of Reducibility is no longer required. Thus classical mathematics, interpreted as one with formal logic, may yet be rehabilitated' (ibid.).¹²

¹¹ Once again, in the January of 1924, Frank visited Russell in London to discuss *Principia* (ibid.: 111), and in 1925, he reviewed the second edition of this book for *Nature* and *Mind*. As Weintraub (2002: 16–17) has argued, 'Modern mathematical ideas in England, as shared concerns of the larger world mathematical community, made their appearance with Hardy and Littlewood in the second decade of the twentieth century'.

¹² In 1926, Ramsey went to Oxford to deliver a paper entitled 'Mathematical Logic' (reprinted in Ramsey 1931 [1960]: Chapter 2). Hardy, who had left Cambridge for Oxford in 1919, returning to his home institution in 1931, was in the audience, perhaps an indication that he was familiar with Ramsey's 1925 article. Upon his return to Cambridge, Ramsey wrote to his wife, Lettice, and explained that he disagreed with Brouwer's denial of the law of excluded middle which states that 'every proposition is either true or false'. He also described his disagreement with Hilbert's proposition that in arithmetic numbers are just 'marks on paper constructed out of the marks 1 and +' (Ramsey quoted in Paul 2012: 227–228). He wrote: 'I cannot persuade myself that I do not know for certain that the "law of excluded middle" is true'. With respect to Hilbert's position, he said that statements like 'I have two dogs', rendered as 'There are x

It is clear that Ramsey's work on mathematics is a product of his family background and of the Cambridge circles he mixed with. This was also the case for many other aspects of his life. In particular, he had two friends of the family who helped him in many ways, C.K. Ogden and I.A. Richards. Both studied at Magdalene College, Cambridge, where Arthur Ramsey worked and taught: Ogden got a First Class in Part I of the Classical Tripos in 1910, and Richards, who went to study history but turned to moral sciences, secured a First Class in Part I of the Moral Sciences Tripos in 1915—after interrupting his studies several times because of tuberculosis attacks (Scott 2004 [2009]; Storer 2004 [2008]). Ogden became known for his Benthamite principles of linguistic reforms (including a 'Panoptic eliminator' of words) and his scheme of Basic English (an international language comprising 850 words) developed with his friend Richards (cf. Franke 2008: 197–201), who was Moore's student. Together, Ogden and Richards published in 1923 the book *The Meaning of Meaning*, unfavorably reviewed by the young Ramsey (Paul 2012: 80–81).¹³

Ogden, who first met Ramsey studying at Winchester, a leading British public school, in the Easter of 1920, and Richards were important supporters of his: in his second term at Cambridge, in 1921, they took him to Moore's lectures on metaphysics and made him a member of another important Cambridge society, the Cambridge Heretics (Paul 2012: 90).¹⁴ At a time when religion was still important in determining social values and customs in Britain, with chapel attendance being mandatory at Magdalene (up to the First World War no other Cambridge college any longer had it), Ogden was a chief founder of the Heretics in 1909.¹⁵ While it started as a movement against compulsory college chapel,

and y , which are my dogs and are not identical with one another', involve the idea of existence and thus are not 'marks on paper' (ibid.: 228).

¹³Ogden, Richards, and the artist James Woods had published in 1922 the book *The Foundations of Aesthetics*. Ogden went on to become one of the leading Bentham scholars of his time, delivering in 1932 the Bentham Centenary Lecture at University College London (Franke 2008: 199). In 1919, Richards was one of the freelance lecturers recruited by the Cambridge English School preparing students for the newly created English Tripos. In 1922, he was appointed Lecturer of moral sciences and English at Magdalene, a position he held until 1939 when he moved to Harvard University (returning to England only in 1974).

¹⁴Ramsey became treasurer of the Heretics, succeeding Lettice Cautley Baker. He and Lettice first saw each other in 1921 but had no further contact. At the end of 1924, they would meet again, at a gathering of the Moral Sciences Club (Duarte 2009a: 448, fn. 8). In August 1925, they got married (roughly two weeks after Keynes and Lydia). They had two children, Jane and Sarah. Lettice studied psychology at Cambridge, and in the 1930s, she became a distinguished photographer, establishing the firm Ramsey & Muspratt that 'became renowned for its portraits of the Cambridge elite' (Stansky and Abrahams 2012: 143).

¹⁵Compulsory chapel attendance at Magdalene was abandoned only in 1921, when students were no longer subjected to sanctions and were merely expected to attend at college chapel (Franke 2008: 35).

the goal of this undergraduate society was ‘to promote discussion on problems of religion, philosophy, and art’, requiring that its members ‘reject traditional a priori methods of approaching religious questions’, as recorded in the Society’s laws (Franke 2008: 44).¹⁶ In contrast to many other Cambridge societies, the Heretics, despite having a formal membership body, opened its meetings to the public and allowed membership from women colleges, Girton and Newnham (the latter being the College of Alix Strachey née Sargent-Florence, Lettice Baker, and Frances Partridge). As Franke (ibid.: 20; italics in original) argues, heresy here meant synthesis, or ‘the desire not so much to remain *opposed* to orthodoxy but to *subsume* it. Reconciliation and synthesis were the predominant modes of transgression of the [Edwardian] age’.¹⁷ Before Ramsey’s membership, speakers at the Heretics included such notables as Russell, Moore, George Bernard Shaw, the statistician R.A. Fisher, the Hungarian poet Ferenc Békássy, and the mathematician G.H. Hardy (Franke 2008: appendix). As honorary members of the Society, we have Moore, Hardy, the painter Lowes Dickinson, the historian G.M. Trevelyan, and Keynes. In 1921, the Society inaugurated its Economics Section that lasted until 1927; according to Philip Sargent Florence, it was ‘heretical in criticizing theory based entirely on the assumption of a rational economic man’ (Sargent Florence quoted in ibid.: 92).

Ogden was president of the Heretics Society from 1911 to 1924, and in 1912 he started *The Cambridge Magazine*, turning it into a vehicle for his own ideas (writing under various pseudonyms) and for publishing extracts and reports of Society meetings (ibid.: 57). His role as editor was important, and in 1910 he started as general editor for Kegan Paul of the successful book series ‘The International Library of Psychology, Philosophy and Scientific Method’ (see Scott 2004 [2009]; Franke 2008: 78–79).¹⁸ This series became known for publishing important psychological and philosophical books, including many titles of authors associated with the Vienna Circle. It was in this series that Richards published in 1931 the first collection of Ramsey’s papers, and it was for this series that in the autumn of 1920 Ogden invited Ramsey to translate into English Wittgenstein’s *Tractatus Logico-Philosophicus*. It was also for Ogden’s *Cambridge Magazine* that in 1922 Ramsey published two impor-

¹⁶ Those who ‘while in sympathy with the general principle of open discussion, are not entirely free’ can only be associates, and not regular members. Franke (2008: 56) wrote that by ‘1913 over 200 undergraduates had joined the Heretics, representing between five and ten percent of the student population’.

¹⁷ Franke (2008: 28–29) goes on to make the case that the Heretics was a society that ‘touched a nerve in the culture of the times’ and that it facilitated the dialogue and widened ‘the sphere of mutual influence between the Apostles and Bloomsbury’.

¹⁸ Ogden edited other book series for Kegan Paul, such as ‘The History of Civilisation’ and ‘To-day and To-morrow’. In 1920, he helped found the psychological journal *Psyche* (originally entitled *Psychic Research Quarterly*), becoming its editor in 1922 with the demise of *The Cambridge Magazine*.

tant articles: one criticizing Major Douglas's credit theory and another being a review of Keynes's *Treatise on Probability*, it carrying the most weight for the book's author (see references in Duarte 2009a: 455, fn. 25).¹⁹

Before coming to Cambridge, Ramsey had German lessons at Winchester in April and May of 1920, and he was reading Ernst Mach 'on the analysis of sensations' and interested in his book *The Science of Mechanics* (Paul 2012: 46). He was encouraged by Ogden to go on with it and expedited his learning by reading German books side by side with their English translations, acquiring knowledge sufficient for enlisting to the German school prize. However, Richards helped spread a story that Ramsey learned German in roughly 10 days after reading Mach's *Analysis of Sensations* with the help of a German dictionary and grammar, reinforcing the aura of genius of the young mathematician. This story made its way into a 1978 BBC radio broadcast by D.H. Mellor, a Cambridge philosopher and editor of several volumes on Ramsey, which was later turned into a series of lectures and an article published in 1995 (Mellor 1995); the broadcast was echoed by Sahlin (1990: 222–223). This story entered economics with the help of Paul Samuelson, who substituted Kant's *Critique of Pure Reason* for Mach's book in the story (Duarte 2009a: 450–451).

Ramsey accepted Ogden's invitation and, at the age of 18, as a student at Trinity College, Cambridge, translated Wittgenstein's *Tractatus*, and a year later, after finishing the project, he wrote a review of it for *Mind*, a leading journal of psychology and philosophy edited by Moore. The attempt to publish the *Tractatus* related back to Wittgenstein's years as a student at Cambridge before the First World War: it involved Russell, and, after being rejected by Cambridge University Press, it also involved Wilhelm Ostwald (who published it in a German periodical) and Ogden in his Kegan Paul Library, where it was finally published.²⁰ Ramsey's translation pleased Wittgenstein, and it was the starting point of a close and sometimes turbulent relationship between them. In 1923, after finishing his final examinations, Ramsey went to Austria and discussed with Wittgenstein every line of the *Tractatus*. At this time, Wittgenstein said to Ramsey that he wanted to get a BA degree from Cambridge with his book as his thesis.²¹ Ramsey and Keynes started a long campaign to bring Wittgenstein back to Cambridge and make him resume his

¹⁹Brady and Arthmar (2012) dissent from this view and suggest that Ramsey had only a partial reading of the *Treatise on Probability*.

²⁰See Monk (1990: Chapter 9) for a detailed account and Duarte (2009a: 451–452) for additional references.

²¹In 1914, Wittgenstein, who had already quarreled with Russell, had asked Moore to visit him in Norway when he asked if he could get a BA degree with the notes on logic he dictated to the latter. The university requirements mentioned by Moore infuriated Wittgenstein: the two argued and only renewed their friendship in 1929 when Wittgenstein returned to Cambridge (Levy 1979 [1989]: 272).

philosophical work. Only in 1929 was Wittgenstein awarded a PhD with the *Tractatus* as his dissertation, having Ramsey as his official adviser and Russell and Moore as examiners (Duarte 2009a: 452–453).

Richards and Ogden, and Russell and Moore, were not the only ones who supported Ramsey in many ways. At the start of his undergraduate life, Ramsey became a close friend of Kingsley Martin, later journalist and editor of *New Statesman*, who was a Quaker. Through Martin, Ramsey was introduced to a group of Quakers who became very close to him and who ‘formed the nucleus of the university Labor Club’ (Paul 2012: 90). This included Lionel Penrose and Richard Braithwaite, both of whom, like Ramsey, became Apostles.²² Braithwaite was one year ahead of Ramsey, reading mathematics at King’s but mainly interested in philosophy. He had known Keynes since 1919 (Moggridge 1992: 121) and introduced Ramsey to him in January 1921, when Keynes’s *Treatise on Probability* was about to be published. In the winter of the same year, Braithwaite proposed Ramsey for membership of the Apostles: in October, Frank was elected unanimously, as required by the Society. Election was for life, with the obligation to attend every meeting, but members could ‘take wings’ and become ‘angels’, that is, honorary and not obliged to attend the meetings. At the end of 1925, Ramsey ‘took wings’ (Paul 2012: 111, 221), but remained active in the Apostles until 1929 (Levy 1979 [1989]: 65, 270). Besides being Apostles, both Braithwaite and Ramsey were invited to Keynes’s Political Economy Club (Paul 2012: 91), which was not a very exclusive club, but it was the one through which ‘Keynes came to know the best of each generation of Cambridge economists irrespective of College’ (Moggridge 1992: 190).²³

While the pre-war Apostles were much under the spell of Moore’s *Principia Ethica*, the First World War brought with it a new intellectual configuration which favored changing the values of pre-war society—Martin’s *Father Figures* (1966 [1969]) is an exemplar of this.²⁴ A post-war vintage of Apostles, including Braithwaite and Ramsey, abandoned Moore and favored a psychological view of ethics (Paul 2012: 114–116). The post-World War I era in Cambridge, and in Britain more generally, was a time of two other particular influences: socialism and psychoanalysis.

²² Frank shared the political outlook of his mother, Agnes, who was associated with the Labour Party: ‘While he always remained left-wing, [after his undergraduate days] he...stopped taking an active part in politics’ (Paul 2012: 21).

²³ Ramsey liked discussions and took part in several Cambridge societies. In addition to the ones already mentioned, he attended the Moral Sciences Club, the Cambridge University Socialist Society, and Magpie & Stump, a Trinity debating society.

²⁴ The book’s subtitle is ‘How a child of Victorian dissent saw the certainties of liberal progress shattered by the 1914 war and found new convictions in socialism and the writings of his radical contemporaries’.

The sizeable working-class movement that had arisen in almost all European countries by 1914 witnessed its socialist branch joining governments. In the case of Britain, the labor movement was based on integrating the working class within the capitalist state. After the war, the European left was split between reformism and revolutionism, and working class unrest and revolutionary potential escalated (Sassoon 1996 [2010]: Chapter 2). At Winchester, before entering Cambridge, Ramsey excelled at mathematics and was much interested in socialism and economics, which made his enthusiasm for religion wane. In 1920, he recorded in his diary 45 books he had read, with about half of them being in those two subjects: he read Lenin, Kautsky, Marx's *Das Kapital*, Sidney and Beatrice Webb's *A History of Trade Unionism*, J.A. Hobson's *Industrial System*, John Stuart Mill, and Marshall's *Industry and Trade*. The year before, Ramsey was most enthusiastic about the Bolsheviks, sympathetic to guild socialism, and he even opposed a motion at Winchester for armed intervention in Russia (Paul 2012: 55, 64).²⁵ He wrote an essay on progress where he cited Mill and discussed the two 'reasonable standards' for a 'fair division of wealth': (1) 'men should receive shares according to their social utility', and (2) 'they should receive equally, provided they do their share of the world's work'—he favored the latter. On either standard, it follows that rent and interest ought to be socialized, 'except the interest on a man's own savings, which could be allowed for his lifetime' (Ramsey quoted in Paul 2012: 66).

In July 1920, Ramsey left Winchester and entered Cambridge, and in his first term, he turned his interests to philosophy, this due to Ogden's influence (and later Moore's), who, according to Paul (2012: 72), inculcated in him liberal values. Nonetheless, Ramsey's concern for improving 'the lot of his fellow men' accompanied him throughout his life and was clearly present in the first article he read to the Apostles, in 1921, in which he criticized Russell's and Hardy's defense of pure mathematics against the charge that it was useless for the 'task of alleviating the suffering of humanity' (Ramsey 1921 [1991]: 292). The same was the case in another Ramsey paper to the Apostles which I quoted at the opening of this chapter (Ramsey 1924 [1991]).

At Cambridge, Ramsey became keenly interested in psychoanalysis. In the autumn of 1922, he was psychoanalyzed by Edward Glover in London, and about this time, he was reading Freud. Through his close Cambridge

²⁵Guild socialism was a multifarious political movement in Britain, strongest after the First World War, associated with G.D.H. Cole and supported by Russell. The idea was that of workers electing, through their factories and industries, a 'guild congress' that would coexist with parliament. A national guild would control industry (see Hutchinson and Burkitt 1997: Chapter 1).

friends, this interest grew even more: Penrose, Adrian Bishop, Margaret and Geoffrey Pyke, James and Alix Strachey, and Sebastian Sprott were analyzed (Paul 2012: 189; see also Forrester 2004).²⁶ The Stracheys and Penrose went to Vienna to be psychoanalyzed: the Stracheys went in 1920 and were psychoanalyzed by Freud for one year, and Penrose went there in 1923 encouraged by John Rickman, a Quaker who was analyzed by Freud and became his first Cambridge follower (someone whom Ramsey also met). Glover encouraged Ramsey to go to Vienna to be analyzed, which happened during the six months Ramsey spent in Austria in 1924 after his graduation as a Wrangler, when he received a Fellowship at King's.²⁷ His analyst was Theodor Reik, the second name suggested by Glover (Paul 2012: Chapter 12). In 1925, Ramsey, Penrose, Rickman, James Strachey, and a few other Cambridge graduates (half of whom were Apostles) set up a short-lived little club to discuss psychoanalysis, the Cambridge Psychoanalytic Group (Forrester 2004: 4–5). Ramsey was so much into this theory that James Strachey wrote of one meeting (quoted in Paul 2012: 193):

[Frank] seemed on the whole to accept [psychoanalysis], but thought the theory very muddled ... He is thinking of devoting himself to laying down the foundations of psychology. All I can say is that if he does we shan't understand them. He seems quite to contemplate, in his curious way, playing the Newton to Freud's Copernicus.

In the paper read to the Apostles in 1924, involving an imaginary conversation with Mill, Ramsey deprecated Mill's outdated utilitarian psychology and praised Freud and his followers for advancing psychology as scientists 'observing facts and inventing theories to fit them' (Ramsey 1924 [1991]: 306). Even Keynes, in 1925, published a pseudonymous letter in the journal *Nation and Athenaeum* commenting on a controversy over psychoanalysis, in which he described Freud 'as one of the great disturbing, innovating geniuses of our age, that is to say as a sort of devil' (Keynes 1982: 393).

If the intellectual efflorescence of Cambridge in the 1920s had Ramsey as an important actor, this configuration could no longer continue with the tragic death of the young Cambridge mathematician on 19 January 1930. In

²⁶In 1922, Sprott tried to arrange a lecture for Freud at Cambridge, but the latter never visited the University (Forrester 2004: 1–2).

²⁷Ramsey got the highest class in Part II of the Mathematical Tripos in 1923, making him a Wrangler. However, contrary to what was previously believed (including Duarte 2009a), he was not first, that is, Senior Wrangler (top candidate), to the dismay of his father (Paul 2012: 140). Ramsey's move from Trinity to King's (something not very common at the time) probably had Keynes's support (cf. *ibid.*: 448). Ramsey later became Lecturer and Director of Studies in mathematics at King's.

November of the preceding year, he contracted flu and later jaundice, but this raised little concern because at the time, people understood that jaundice was not life-threatening, although it could last a long time. In early January, he went to Guy's Hospital in London for an exploratory operation to see if there was any stone that needed to be removed. The operation revealed his liver in a very serious condition, and he did not recover from the surgery, to the stupefaction of his family and Cambridge friends.

3 Ramsey's Economics

Ramsey published only three articles in economics: as an undergraduate student of 18 years of age, he published a critique of Major Douglas's credit theory for Ogden's *Cambridge Magazine* in 1922; then in 1927 and 1928, he published two important articles in the *Economic Journal*, the first on taxation and the second on optimal saving.

Major Clifford Hugh Douglas did not complete his studies at Cambridge and became a member of the Institute of Electrical Engineers. Together with Alfred Richard Orage and others, he developed from the late 1910s (a period when the UK was off the gold standard) a theory of guild socialism known as the 'A + B theorem' in which credit had a central role. Major Douglas saw money as a ticket system that granted the right to its owner participation in the economy, allowing the goods produced to be distributed to consumers as they see fit. Additionally, and central to his ideas, he conceived production as a multistage process in time, with firms distributing two types of payments: (A) payments to other firms related to the purchase of raw materials and to other costs; (B) payments to individuals in the form of wages, salaries, and dividends (Hutchinson and Burkitt 1997: Chapter 2). The prices of goods reflect both 'A' payments made in the past and 'B' (thus 'A + B'), and in a given period of time, 'B' cannot buy 'A + B' (though over time there is no lack of purchasing power). Therefore, extra money in the form of credit should be injected into the economic system every now and then. His argument was also made in terms of selling prices being inferior to cost prices of goods due to a lack of purchasing power.

According to Soifer (2009: 293), Ogden was the one who persuaded Ramsey to analyze Douglas's proposals. Douglas's writings are far from clear, and Ramsey opened his review article suggesting to those interested in understanding his ideas to read W. Allen Young's *Dividends for All* (Ramsey 1922b [1968]: 335). Young pointed to a flaw in Douglas's theory, with an argument that in general terms involved the possibility of an existing firm producing intermediary goods

and distributing wages and dividends that could absorb the surplus of all other firms. Ramsey set himself the goal of exploring this flaw in Douglas's theory more carefully. Unlike Young's static argument about the existence of purchasing power somewhere in the economy at a given point in time, and contrary to the understanding of Hutchinson and Burkitt (1997: 84), Ramsey went on to make a dynamic argument very much in line with Douglas's stress on production as a multistage process in time. Ramsey (1922b [1968]: 336) made 'a strong and simple argument for supposing that the [selling price to cost price] ratio does not differ appreciably from unity': this ratio is equal to one in a 'stationary state', which is a state 'in which production goes on at an unchanging rate and prices, wages and the national wealth never alter' (ibid.). In this state, 'the distribution of purchasing power by all [intermediary] factories proceeds at rate A', which together with wages and dividends through B add up to an aggregate purchasing power of $A + B$ —'which equals the rate of flow of cost prices of consumable goods' (ibid.: 337).

Finally, Ramsey closes his analysis with two arguments. The first is that 'the present state is not very far from being stationary' (ibid.), thus implying that the ratio of selling to cost prices does not differ greatly from unity. But he understood that this was not a general case. He then used integral calculus, and integration by parts, to study this ratio 'under much wider conditions, which allow for changes in the quantity of production, in the rate of wages, in the productivity of labor, and in the national wealth' (ibid.). With a dynamic analysis, he first showed that under certain conditions the ratio of selling to cost prices is unitary (ibid.: 337–339) and then considered even more general conditions. In this case, he showed two possibilities for the ratio to be less than one but they are 'obvious to common sense and are clearly irrelevant to Major Douglas' contention that "just price" is today a quarter of cost price' (ibid.: 340).

The two papers that Ramsey published in the *Economic Journal* were the ones to secure him, eventually, a distinguished place in the economics profession. Contrary to the view that he was mostly a mathematician distracted by economists to help them with certain mathematical problems, I have argued elsewhere that those two papers were part of what we might call Ramsey's research agenda in economics, which emerged from his close interaction with Pigou (Duarte 2009a). As part of a Cambridge habit of senior Fellows asking other economist Fellows to help them with their research, Pigou involved Ramsey in his work that culminated with the publication of his 1928 book *A Study in Public Finance*, with a concern about tax exemption on savings.²⁸ Out of this concern, Ramsey first wrote an article setting up a static utilitar-

²⁸ Pigou asked for Ramsey's help in preparing the third edition of his book *Economics of Welfare*, published in 1929, as he had done earlier with the young Keynes, who helped revise his 1912 *Wealth and Welfare* (Duarte 2009a: 459). See Gaspard (2003) for a discussion of Ramsey (1928).

ian framework for characterizing the optimal tax rates for a government in need of raising revenues at the same time that it does not have lump-sum taxes available. Ramsey (1927) simply postulated that there exists a net utility function of producing and consuming quantities of n goods in the economy (denoted by x): $u = F(x_1 \dots x_n)$. He then showed that uniform taxation is generally suboptimal because the tax rates that maximize this function are those that reduce the production of all taxed commodities in the same proportion with respect to the benchmark of prices equal to marginal costs. Not surprisingly, Pigou was the first to cite Ramsey (1927) in his 1928 book, stressing the elasticity version of Ramsey's result already enunciated by the latter: inelastic commodities, either for supply or for demand, should be taxed more than elastic ones.

As a reaction to Pigou's 'treatment of saving as a use of income with its own elasticity of demand' (Ramsey quoted in Duarte 2009a: 463) in the 1928 book, Ramsey (1928) built on his previous article to discuss the taxation of savings in an intertemporal utilitarian framework. He first characterized the choice of savings that maximizes the utility of consumption net of the disutility of labor subject to the economy's resource constraint (which specifies that expenditures on consumption and investment exhaust aggregate output). In the first part of his article, he discusses the savings problem of a society without discounting future utilities on ethical grounds of intergenerational justice. Thus, in order to have a well-defined optimization problem, Ramsey postulated the existence of an upper bound to net utility and therefore minimized the distance over time of the actual utility to this bliss level:

$$\text{Min}_{x_t, a_t} \int_{t=0}^{\infty} \{B - [U(x_t) - V(a_t)]\} dt \quad \text{s.t.} : \frac{dc_t}{dt} + x_t = f(a_t, c_t)$$

where B is the bliss level, and x_t , a_t , and c_t are respectively consumption, labor, and the stock of capital.

The solution to this problem specifies that 'the saving rate multiplied by marginal utility of consumption should always equal bliss minus actual rate of utility enjoyed' (ibid.: 547).²⁹ Keynes suggested a marginal approach (of equating cost and benefit at the margin of saving one extra unit of money today) as an alternative way of deriving Ramsey's mathematical result, as Ramsey acknowledged in the article—a result nowadays referred to as the

²⁹ Later in the article, Ramsey considers the optimization problem of an individual who discounts future utility and shows how this result has to be adapted accordingly.

‘Keynes-Ramsey rule’. Once again, Pigou was the first to cite Ramsey (1928) in the second edition of his public finance book, published in 1929.

However, the published version of the 1928 paper has no section analyzing taxation. After Ramsey submitted the article to the *Economic Journal*, Keynes asked him to cut the section on taxation out of the optimal savings article as the mathematical analysis was ‘too involved in comparison with the conclusions which were feeble’ (Ramsey quoted in Duarte 2009a: 463). Ramsey agreed with Keynes and accepted his suggestion. The draft of the original section survives and was published in 2009 (Duarte 2009c).

What is clear from the few contributions by Ramsey to economics is both how his research agenda emerged out of the Cambridge milieu of his time and how concerned he was with using economics to obtain general results useful to improving human well-being. For the two later articles, Pigou was instrumental in posing the questions, providing an analysis against which Ramsey reacted, and in citing the articles in an important book of the interwar period. Keynes was important, too. He had Ramsey as an interlocutor to try out his ideas and to consult about articles submitted to the *Economic Journal* (Duarte 2009a: 455–456). Moreover, he helped shape the argument of Ramsey’s 1928 article and stressed that rendering sound results should limit the complexity of mathematical analysis in economics.

4 Crossing the Atlantic

In mathematics, some of Ramsey’s contributions were acknowledged promptly by Paul Erdős and George Szekeres (Graham and Spencer 1990: 114–115), leading to the creation of a field in the intersecting areas of combinatorics, number theory, geometry, topology, and measure theory, known as ‘Ramsey theory’ (mathematicians also talk about ‘Ramsey numbers’; cf. Soifer 2009, 2011). In contrast, as I mentioned earlier, in economics, Ramsey is presented as a sleeping giant who was awakened only in the 1950s when economists finally acquired the necessary mathematical tools to comprehend his ideas—a process that was an integral part of the Americanization and dominance of neoclassicism in economics (Morgan and Rutherford 1998). Claims like this tend to miss important aspects of the canonization of authors in a given community.

Ramsey’s 1927 and 1928 articles were discussed before the 1950s to a limited extent when compared to the boom in citations that occurred from the mid-1950s onwards (Duarte 2009b, 2010). The ‘rediscovery’ of Ramsey meant

extending his analyses, with new mathematical tools, such as Hamiltonians or new theories of welfare economics, and placing them at the core of the neoclassical economics of the post-war period. This is exactly what Michael Intriligator's description in the late 1960s of optimal growth literature reveals: this literature 'carries the imprint: "Economics by Ramsey; Mathematics by Pontryagin"' (Intriligator quoted in Duarte 2009b: 173).

In order to better understand the canonization of Ramsey in post-war economics, we have to recognize that, in terms of citations to him in economics articles available on JSTOR, there were three major waves of 'rediscoveries' (Duarte 2009b: 168–189): a wave of references to Ramsey in the subjective probability and expected utility literature of the 1950s and early 1960s; another wave on economic growth starting in the mid-1950s; and a final wave from the late 1960s on public economics. Furthermore, Paul Samuelson was a leading author citing Ramsey in all of them and an influential teacher of several authors who later contributed either to the optimal growth literature or to the public finance discussion on taxation, or to both (such as Peter Diamond and Joseph Stiglitz). Samuelson feared dying early and loved telling his students stories about economists, particularly the one, as already noted, that Ramsey learned German in a week, promoting the romantic view of the precocious mathematician who made seminal contributions and died in the prime of life (Duarte 2010: 150–153).

For post-war economists, several of whom were connected to Cambridge, USA, Ramsey is the precursor of a microfounded model of growth in which the lifetime utility of a representative agent is discounted. In public finance, he is also the precursor of a representative agent theory that was unfortunately in the mould of the old welfare economics with interpersonal comparison of utilities.³⁰ Clearly, the mathematical tool used by Ramsey (1928), namely the calculus of variations, was not the major factor explaining its attractiveness to post-war economists: the American mathematicians Griffith Evans and Charles Roos were contemporaries of Ramsey who employed the same method, albeit not in a utilitarian framework, and who were marginalized in post-war economics (Weintraub 2002: Chapter 2). The growing importance of Ramsey has much to do with the communities of economists who transformed economics in the post-war period.

³⁰ It is unclear whether Ramsey had a notion of a representative agent. I argue that he might have had one (Duarte 2010: 126–130).

5 Conclusion

Ramsey was a mathematician much involved with important economists of his time such as Pigou, Keynes, and Harrod. He was immersed in the Cambridge milieu of the 1920s and took part in discussions not only on the role and foundations of mathematics but also on improving the lot of his fellow men and socialism, on utilitarianism and psychoanalysis, and on taxation and savings. His untimely death moulded the tragic mathematician view that he acquired in the many areas he contributed to. Ramsey is sometimes portrayed as a sleeping giant whose two major articles in economics became central to neoclassical analyses of taxation and economic growth. It is thus clear that Ramsey is indeed a very rich historical figure who offers us interesting windows on both the Cambridge economics of the 1920s and on the important transformation that took place in economics after the Second World War.

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Joan Violet Robinson (1903–1983)

Prue Kerr

1 Introduction

Joan Robinson was a distinguished economist at the forefront of major developments in modern economic theory from the 1930s and for the next 50 years. She was as passionate about social injustices as she was about intellectual honesty. She was born into an upper-middle-class family, the third child in a family of four daughters and one son. Her father was Major General Sir Frederick Maurice, a professional soldier and, later, a well-regarded military historian. Her mother was Helen Margaret Marsh, from a family with many ties to Cambridge, including her father, Frederick Marsh, who became Professor of Surgery and Master of Downing College at Cambridge. The family favoured education for women, and Joan was educated at St Paul's Girls' School in London and then Girton College, Cambridge.

While at school her father became involved in a very public argument with the Prime Minister, Lloyd George, over troop requirements for the anticipated German attack on the Western Front. George ignored the military advice and

I am much indebted to Geoff Harcourt and have drawn extensively from our joint biography of Joan Robinson (Harcourt and Kerr 2009), and to David Kelly, both of whom made this chapter possible. I am also grateful to the Economics Department of the University of Adelaide, South Australia, for very generously providing me access to all their facilities, a space to work, and some colleagues to confront. I have also drawn on earlier research carried out in the Modern Archives Centre of King's College, Cambridge, and thank the staff for their guidance through the extensive collection.

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deceived parliament and the people, with the outcome being disastrous losses of soldiers' lives. The official blame for this was put on military decisions. Joan's father was absolutely loyal to his men, and this blame came on top of the tragic losses. In early 1918, he published a letter with his evidence in the daily papers, exposing the government for its deception. The Maurice Debate in the House of Commons became sensational. Although he was vindicated, he had to leave the army. It had a major impact on Joan, then aged 14, as she became known as 'Maurice's daughter', and was thrust, as she put it, from her life in her mind to a life in reality.

She began her studies in economics at Girton in 1922. It was unusual at that time for women to continue their education at university level, and the few women students of economics were permitted to attend the same lectures as the men. (While the men dressed in academic gowns, the women were to wear day dresses and hats! (Turner 1989)). Lectures in economic theory were based on Marshall's *Principles* as interpreted and adapted by his successor as Professor of Political Economy, A.C. Pigou. Joan would also have attended lectures by Maynard Keynes, on 'Realistic Monetary Problems'. Her other lecturers included Dennis Robertson and Gerald Shove, who also based their thinking around Marshallian theory.

The Cambridge approach to economics at this time was a combination of the inductive and deductive. First one had to identify and establish through empirical study, the economic problem, and then develop a theory which was logically sustainable and appropriate in its method and conditions for understanding the problem. These were seen to be the directions for economics to be awarded the sought-after status of a science and were consistent with Marshall's 'deductive political economy guided by observation' (Neville Keynes quoted in Whitaker 1987: 353).

Robinson completed the Cambridge Economics Tripos in 1925 with a 2.I, a disappointment to her after being the only student to receive Class 1 in the Intercollegiate Examination in Economics at the end of the first year of study. The actual degree was only awarded in 1948 when the University recognised women graduates.

In 1926, she married Austin Robinson. He had graduated from the Economics Tripos in 1922 with First Class Honours and was now a Fellow of Corpus Christi College. Two months later, they sailed for Bombay for Austin's appointment as tutor to the son of the recently deceased Maharaja of Gwalior. Although the appointment was offered for eight to 10 years, they stayed there for only two years. In this time, Austin and Joan drafted substantial portions of a book on optimum population for India given the existence of diminishing, constant, and increasing returns to industry; it

remained unfinished. Joan also reviewed fiction in local newspapers. On their return to Cambridge in 1929, Austin was appointed to a University Lectureship in the Faculty of Economics in 1931 elected to a Fellowship at Sidney Sussex College. Joan developed an informal relationship with the Faculty of Economics and Politics attending some lectures and taking some college supervisions. She was familiar with the prevailing Marshallian ideas on value theory from her undergraduate lecturers and her Girton supervisor, Marjorie Tappan-Holland, but uncomfortable with the authority these assumed. She was soon developing what would become lifetime friendships with critics of Marshall's hold on economics. There were two in particular: Piero Sraffa, who became a personal friend, and while he remained reluctant to read her work, he was nevertheless a continuing source of inspiration, especially for his knowledge of classical political economy and of Marx; and Richard Kahn, always a close personal friend, who became her 'sounding board' and an astute critic whom she trusted and respected.

In 1936, she met Michał Kalecki, a Polish economist visiting Cambridge, whose Marxist-inspired approach to a theory of employment was based on analytical propositions parallel with those of Keynes. He remained an important intellectual influence in Robinson's subsequent development and became a lifelong friend. She also met Roy Harrod, who had been in Cambridge in 1921–1922 and had since maintained contact with Keynes, James Meade, who was visiting Cambridge from Oxford in 1930–1931, and Nicholas Kaldor, at the London School of Economics (LSE) during the 1930s, and who, after a more formal friendship as they met at joint seminars or discussions, when he came to Cambridge in 1940, became a colleague, sometimes a rival, and friend.

In 1931, albeit with some controversy, Joan was allowed to give occasional lectures. In 1934, she was appointed to a Faculty position of University Assistant Lecturer for one year and, in 1937, to a permanent place as a University Lecturer. By this time, she had publications in top journals and an internationally regarded book to recommend her, quite apart from her success as a lecturer and supervisor—'the most exciting and brilliant lecturer' (Turner 1989: 45). She had also just given birth to her second daughter. Keynes and Pigou both supported her appointment. She was appointed Reader in 1949 and, finally, Professor in 1965, when Austin retired from his Chair.

From her family and school experience to the introduction to academic life, all of these people contributed to the young Joan Robinson's intellectual development and confidence. Within this context, she was encouraged to explore her potential for challenging the status quo. She was a sensitive and shy person despite her public presence, her passion frequently expressed in her confrontational style; these, coupled with her incisive mind, meant she

was rarely ignored. From this early stage in her career, she had the important support of intellectual engagement and respect and of friendships, as well as professionally strategic support, from Austin, Keynes, Kahn, and Sraffa, among others. From the outset of her career, she did not want to be known as a woman economist.

2 Against Marshall: Realism of Assumptions and Internal Logic: *Economics of Imperfect Competition*

Robinson chose to study economics because she believed it could provide ways to better the lives of those victims of the (generally capitalist) economic system. Indeed, she saw it as the responsibility of economists to act on their knowledge. Her social commitment initially took the form of confronting the prevailing authority of Marshallian economics in Cambridge. Neither Marshall nor Pigou disregarded the surrounding social issues, as both made their concerns explicit; the problems arose from the interpretation of these issues within a theoretical system from which policy could be formed. Marshall's critics presented this system as relying on a method and analyses that had no relevance to the existing capitalist, industrialising, and monetary economies. More fundamentally, Sraffa was already pursuing a critique of Cambridge Marshallian economics, begun as a young academic in Italy, during 1923–1925 (Sraffa 1925), and launched in England by Sraffa in 1926. Now in Cambridge at Keynes's invitation, as a Lecturer, he claimed to have destroyed Marshall's analysis of costs and of returns to scale based on its method of partial, or 'particular', equilibrium (Marshall 1890 [1920]: 669; Sraffa 1926), which required *ceteris paribus* conditions.

Robinson attended Sraffa's lectures on 'Advanced Theory of Value' at the end of the 1920s (Sraffa began lecturing in Cambridge in 1927 before taking the post of Marshall Librarian in 1930). Sraffa had demonstrated that inconsistencies arose in the internal logic of the partial equilibrium analysis, in particular, how to reconcile the coexistence of falling supply price and perfect competition in a partial equilibrium method. For Marshall's analysis to be valid, it required the assumption of independence of supply and demand conditions. As a method, when the partial equilibrium result was generalised to draw conclusions about a system as a whole, the assumption of *ceteris paribus* made no sense. This was the principal theme of Sraffa's 1926 paper and a symposium in the 1930 *Economic Journal* (suggested by Keynes, an editor) on the representative firm and increasing returns, rejecting Marshall's

and Pigou's versions of value theory. Sraffa interpreted Pigou's response to be taking increasing and decreasing returns from their separate roles in classical theory, where the theory of production and costs was objective while the theory of value was subjective, and combining them into symmetrical roles in the one theory of value. This obscured their respective contexts, one representing a historical process while the other described the emergence of a new class of income, rent, as marginal land was drawn into use. But the assumption of perfect competition enabled this condition of independence of the supply and demand functions to be met. However, it was inconsistent with a falling supply price. Robinson's entry into this divisive (in and beyond Cambridge) controversy questioned the impact of this simplifying assumption. Sraffa suggested that the market be represented as comprising a set of mini-monopolies operating as competitors with downward-sloping demand curves. Her response to Sraffa's critical analysis was to replace the assumption of firms operating along the precepts of perfect competition as the basis of value theory with more relevant imperfect-market structures and more realistic or objective concepts.

The Economics of Imperfect Competition (EIC) (Robinson 1933a) examined what happened to price and output in response to changes on the demand side of the market and then on the supply side (in Marshall's long period) when the assumption of perfect competition was replaced by alternative market structures. She introduced the concept of marginal revenue (MR—a recently identified concept, inspired by one of Austin's pupils, Charles Gifford, and made operational through Joan's collaboration with Austin and Kahn) together with marginal costs (MC), and these were the basic tools with which she systematically explored the outcomes of introducing imperfect markets into existing value theory, so remaining within the marginalist method rejected by Sraffa. This formed the body of her first book.

In *EIC*, Robinson had in mind the proposition that it was demand conditions rather than rising costs which affected firms' sales. Profit-maximising prices and quantities are determined by the intersections of their long-period MC and MR curves. For example, if the demand in the imperfectly competitive firm or industry increases, the equilibrium price will be the same, higher, or lower depending on the value of the price elasticity of demand at that new equilibrium position. Similarly, for costs: how do average costs (AC) respond as new firms are attracted into an abnormally profitable industry? On the supply side, this higher demand 'is more likely to lower the average cost curves of firms than when the market is perfect. It therefore appears...to be highly probable that falling supply price is a result of imperfect competition' (ibid.: 544; also quoted in Harcourt and Kerr 2009: 17–18.) She demonstrates with

diagrams the equilibrium for this firm, where $MR = MC$, but price is greater than AC . The tangency condition is $P = AC$, which means AC must be falling. But 'while the presumption that average costs to the firm will be falling is far stronger than those critics suggest, the presumption that supply price will be falling is far weaker than they suppose' (Robinson, *ibid.*). This would support her proposition that downward-sloping demand curves rather than upward-sloping cost curves were the ultimate constraint on both current and future output. Her analyses also applied to intermediate products and to services of factors of production, for example, where the buyers had a monopsony and the sellers an oligopoly. Also, imperfect competition challenged the idea of a clearly defined industry with the likely presence of differentiated products. Robinson effectively reconceptualised Marshallian value theory into a theory of output of a single commodity or firm.

The publication of *EIC* established Joan Robinson's status as a member of the Marshallian guild at Cambridge and secured her an international reputation. At the same time as *EIC* was published, Chamberlin (1933) published in the USA his monopolistic competition version of an analysis of imperfect competition. His public and continuing rivalry, trying to establish that his was the superior product, left Robinson bemused but nevertheless able to move on to new interests and ways of thinking. Her ambitions were to find solutions and disseminate their implications, not her own fame.

Yet it was a book she left behind because of its Marshallian properties and methods, and when it was reissued in a second edition in 1969, she wrote a new, highly critical Preface to prepare the reader for its original intellectual context. Although the economy of the 1920s and 1930s was very different, in detail at least, to that of the late twentieth and early twenty-first centuries, much of the existing mainstream theory remains based in a perfect competition framework and its assumptions. Robinson's critique shifted its focus onto the posing of the static neoclassical partial equilibrium theory as a theory about growth and distribution.

3 *Economics Is a Serious Subject (ESS): Fact and Value: Science and Ideology*

In 1932, having delivered the manuscript for *EIC*, Robinson wrote a pamphlet exploring the methods of economists, classifying her colleagues according to their approaches. Her pamphlet, *Economics Is a Serious Subject: The Apologia of an Economist to the Mathematician, the Scientist and the Plain Man* (Robinson 1932), was not written for academic recognition but more for provocation and a

broader audience. Robinson's absorption of the seductive language and structure of the local logical positivists was consistent with an equally pervasive faith in the primacy of science in advancing knowledge and the subordinate, subjectivity of research with political or ideological connections. The issue of whether economics was value based was a stock question at this time. Robinson sought to bring the two aspects of knowledge together, while maintaining a distinction between 'facts' and 'values' (*ibid.*: 4).

As she saw it, economic problems generally take the form of 'facts'. Facts were consistent with axiomatic assumptions. Such objectivity enabled political economy to be 'scientific'. Values and ideology introduced the formless role of the subjective. All economic theory implied some method, but it was at the time taken to be uncontroversial. Robinson, however, was considering the implications for economic theory of its method, recognising how any economic theory, in this case neoclassical partial equilibrium theory, was developed within the structures pre-empted by its method. Furthermore, questions can also arise out of the theory itself. 'Perfect competition' was a contemporary example. Perfect competition was clearly an analytical device, but if it were replaced by imperfect market features and rules, what were the results?

Her method, at this stage, was to examine the assumptions used to characterise a real situation, classifying these as either real or analytically tractable, with an apparent trade-off between the two.¹ The economist will reveal their ideological slant in their choice of assumptions. If this ideological presence could be contained in the assumptions, the actual logical theorising could be value-free, potentially 'scientific'. For Robinson in the 1930s, the adequacy of a theory thereby became a matter of the realism of its assumptions and the logical coherence of its arguments based on these assumptions and the appropriateness of the method for the required aim.

Science can prescribe various forms, or methods, requiring precisely defined concepts and ways of representing them. Statistical studies and mathematical models were, and are still, the most commonly used in economics. Robinson did not dismiss mathematical arguments; indeed, she recognised a role for this method; most of her own work was in the form of highly simplified (*sic*) deductive models using abstract concepts. Her own mathematical limits may have discouraged her from exploring problems along this route, and, besides, with a scope limited by the degree of abstraction necessary for this type of formal logic, mathematics may not have

¹ The apparent trade-off reappears as, or was suggestive of, a later decision, post-war, between having the status of science and having a relevant theory when Robinson was coming round more to the view of an inseparability of values from 'scientific' endeavours.

been suitable for her uses. On the other hand, she relied on statistical results from empirical studies by trusted others for many of her purposes. The very establishment of the problem itself entailed choosing concepts which could represent the actual issue meaningfully and measurably. With this objective, statistical work could be confirming a framework and its relevance. It was the ill-considered adoption of mathematics and statistics to understand everything that she was and remained wary of. This includes attempts to criticise her analytical arguments by empirical and statistical methods, which have missed the point of her advice to readers to be cautious against interpreting logical-time models as being applicable to actual situations. She makes this clear: ‘Certainly it is not legitimate to set up a highly abstract model and then draw from it conclusions applicable to actual problems’ (Robinson 1962a: Preface). Remarks about assumptions, values, and methods appear throughout her work and are a particular subject for discussion in her more wide-reaching books of Robinson (1962b, 1966a, 1970, 1971), a period during which the abstract concepts being used by her antagonists seemed particularly problematic.

4 Keynes

In 1930, Sraffa organised a group of young economists to discuss Keynes’s *Treatise on Money* (Keynes 1930 [1971]) (*TM*): the Cambridge Circus. The group comprised Sraffa, Austin and Joan Robinson, Kahn, and James Meade. Keynes’s *TM* was a theory of money and the determination of the general price level in Keynes’s eyes, but members of the Circus also saw it as possessing the germ of a new theory of (total) output and employment and as being about the long period. Keynes subsequently developed the argument in *The General Theory* (*GT*) (Keynes 1936 [1973]), and although the theory remained partly dependent on a Marshallian basis, Keynes showed, in a radical departure from Marshall, how effective demand, investment spending in particular, was the major determinant of levels of employment and output. Furthermore, investment, by increasing employment and incomes, generated its own saving, the reverse of neoclassical causality. Most importantly, he demonstrated the possibility, indeed probability, of sustained unemployment over the long run, that is, the long-period equilibrium outcome was consistent with ‘normal’ profits and wages and unemployment. He developed his analysis in a short-period context in which capacities of plant and of labour were fixed and his question concerned the influence of change in the level of effective demand on the utilisation of these resources and on incomes. He did

comment on the possibility of there being no unique long-run equilibrium as its determinants might themselves change the nature of their response to the short-period outcomes so introducing an idea of an endogenous growth process, a shifting equilibrium, which Robinson later gave form to as an iterative path in her growth models and, more broadly, as a way to introduce history into the analysis. Kaldor (1934) was also developing an idea of path dependence and ‘cumulative causation’.

Keynes had asked Robinson to read his manuscript for the *GT* as he wrote it (replacing his usual discussant in Kahn, who during this time, 1933–1934, was in the USA). In 1933, she published two articles which reported on Keynes’s current thinking and defended his ideas as he responded to the feedback from the Circus; Robinson (1933b) published in the first issue of the new journal, *Review of Economic Studies*, jointly edited by the younger generation at the LSE and Cambridge, and Robinson (1933c). By the time the *GT* was published in 1936, she felt confident that she was in a position to make its radical ideas more widely known and understood, anticipating those aspects of its argument which would be most politically as well as analytically problematic. It was a bold move to leave behind her recently acquired authority in the field of imperfect competition for a new venture into aggregate issues of unemployment and a role of apprentice. In 1937, she published two books: *An Introduction to the Theory of Employment* (Robinson 1937a), and *Essays in the Theory of Employment* (Robinson 1937b). The former was addressing a non-specialist (although informed) audience and aimed to provide a simplified introduction to Keynes’s main arguments, thereby familiarising a wider public with the rationale behind a new policy orientation. The *Essays* represented the first impact of Keynes’s new theory on other related—theoretical and actual—issues. Around this time, Kalecki had arrived in Cambridge and Robinson reset her understanding of the theory of effective demand into a Kaleckian–Marxist context (Targetti and Kinda-Hass 1982).

5 Marx

In the early war years, Robinson wrote her book on Marx, in which she sought ‘to alert my bourgeois colleagues to the existence of penetrating and important ideas in *Capital* that they ought not to continue to neglect’ (Robinson 1977: 50). This included translating selected elements of Keynes’s analysis into relevant elements of Marx (Robinson 1942). Her views on Marx were influenced by her discussions with Dobb and Kalecki, if not always in the ways they were intended. Both Keynes and Kalecki provided the analytical

grounds for a new theory of output and employment. Most controversially, they argued that there was no tendency within the capitalist economy to move it towards full employment. But Kalecki went further, and adapting Marx's schema of reproduction, he demonstrated that the capitalist process contained its own contradictory forces which would send it away from full employment and stability. These fundamental forces arise out of the nature of exploitation of labour and the production and appropriation of the surplus product. They create an inherent tendency for underconsumption and stagnation, which further stifles 'animal spirits' and dampens investment, leaving potential unrealised surplus. Kalecki brought together a theory of the trade cycle within a longer-run perspective, so placing these problems into the actual structure of his analysis.

His account provided a more insightful basis for Robinson's moral attitude towards the injustices of capitalism, for her own subsequent theory of growth and distribution than did Keynes's Marshallian framework. Robinson's adaptation of Marx's classical schema clearly illustrated the necessary relationships in a two-sector economy of wage and profits income, and consumption and investment expenditure, on which to introduce 'real' features and build a progressively more complex theory. With given productivity of labour producing wage goods, and given real wage rates, the schema illustrated the conditions that the consumption goods sector had to produce sufficient to cover its own wages as well as the wages in the investment goods sector and that aggregate saving match aggregate investment (Kalecki 1936, 1939). The different savings propensities of the respective income groups made redistribution of income a process of achieving target investment aims. This redistribution takes place when the real value of given money wages in the wage goods sector is reduced by imposing a higher markup on its prices, the resultant surplus making it possible to expand the investment goods sector.

Robinson's analysis, based on the ideas of Marx, Kalecki, and Keynes, directly addressed the problem of sustained unemployment. She introduced a distinction (Robinson 1937b) between unemployment that occurs due to lack of effective demand relative to the existing capital stock, Keynesian unemployment, and that caused by the capacity of the capital stock, a Marxian problem, which she terms 'disguised unemployment' or 'underemployment'. She introduced the latter as being a characteristic particularly of undeveloped countries, requiring different policy responses to those of the Keynesian case. The distinction was adopted into the literature on development and continues to be used, both empirically and theoretically (Robinson 1936, 1937b, 1973). She also discussed (Robinson 1943) the political role of unemployment in maintaining a 'disciplined' workforce, an issue similarly examined at that time by Kalecki (1943). Through her understanding of Marx and of Kalecki, she

both broadened the scope of what economists should comment upon and gave her own concepts more depth and her analysis more insight.

The unrelenting presence of mass unemployment throughout the 1930s was seriously questioning both the relevance, and the validity of the analysis, of the neoclassical status quo. Sraffa's (1926, 1930) critique of Marshallian value theory had not been satisfactorily answered and the practice of laissez-faire policies and institutions had obviously failed to provide its promised prosperity. If the activities of the private sector were not reconciled with the public interest by the unhindered mechanism of free markets, some alternative economic system was needed. Robinson's analysis, based on the ideas of Marx, Kalecki, and Keynes, directly addressed the problem of sustained unemployment.

6 Socialist Democracy

The Labour Party, along with other Left groups in the 1930s, was exploring the idea of democratic socialist planning as the basis for a new economic order. This would displace the risky dependence of the economy on animal spirits. The outbreak of the Second World War resulted in a level of state interference into the normal, peacetime economic activity far beyond that of the previous war, so bringing the political possibility of introducing a socialist economic order to Britain much closer. Robinson was a keen advocate of socialist planning, and she presented her ideas in her non-academic activities during the war.

Robinson's pamphlets, articles in the press, and talks at political meetings and on the BBC, during the war years and the following decades, are less known within academic circles (although some are published in subsequent volumes of her *Collected Economic Papers (C.E.P.)*) (Robinson 1951–1980). Her familiar terrain of analytical questions, framed in abstractions and rigorously argued, was replaced, with similar passion, by descriptive explanations of real issues and potential responses, in ways which had to be politically as well as economically feasible. She was writing for a general public and addressed their experiences, as she understood them, of ongoing political and economic issues and of those threatening their post-war future. These included unemployment, labour organisations, monopoly power, and inflation. She gave an understanding and a voice to those unfamiliar with the language of economics, suggesting the possibility of their own participation in change.

She introduced a discussion about the possibilities of a post-war democratic socialist economy among whose immediate goals was full employment of labour and resources. The most reliable route to take to achieve full

employment and a higher standard of living was a strategic mixture of private and public provision organised through some form of centralised planning. She recognised that 'planning' had become a slogan and warned that it only made sense when its terms of reference, its objectives and conditions, were clear. It would require a significant reorganisation of the relevant industry and public support practices from serving a largely uncontrolled economy to serving one with a higher degree of intervention. The possibilities of State planning were also of particular interest to Kalecki, and Meade too supported this approach for the British economy.

In her various pamphlets, Robinson proposed, as a fundamental part of implementing planning decisions, the establishment of a National Investment Board (NIB). The government, through the NIB, could make decisions about what to invest in and selectively contract out production to the private sector. This would encourage private investment in competitive markets but give State control over infrastructure, natural monopolies, or provision of essential services for which market distribution was inappropriate or there was little profit incentive. In *EIC*, she had concluded that there are some industries in the economy which, to be efficient, needed to be run collectively and under public ownership, natural monopolies being an obvious one. The NIB would enable the State to direct the activities of selected industry groups, taking into account their production interdependencies and their resources needs. The level and composition of effective demand would largely become the outcomes of deliberate and co-ordinated directives which could be based on objectives other than profit alone. It could undertake long-term planning and projects, co-ordinating their ongoing needs with shorter-run production processes. While still deciding what to invest in, the NIB could, in accord with its objectives, contract out its production to the private sector. The institution of the NIB would promote greater certainty through its planning of strategic parts of the economy while still leaving the market to determine consumer choices and also, by its competitive nature, encourage invention and innovation.

A major tool for the NIB was the price system. Pricing policy would be used to establish the desired income distribution between wages and profits: this will impact on resource allocation between sectors of consumer and investment goods and services, and between agricultural and industrial sectors. It could also be used, with incomes policies, to maintain real wages and control the anticipated post-war inflation accompanying full employment and shortages of necessities. The NIB would consider, given its objectives within the broader plan of the government, which prices to control and which to leave to the market, and which measures to use to implement control over

prices of different categories of commodities. A case-by-case approach would better achieve the broader human welfare objective of the State than applying a general rule.

The NIB would include in its loans necessary conditions to be met for employees, including the participation of workers in decisions about working conditions and wages and shares in productivity gains. Wages needed to acknowledge the nature of the work itself and of the needs of the workers. At the same time, Robinson argued for a cheap money policy; the Treasury and the Bank of England tended to set the interest rate with political issues in mind. Sustaining low interest rates could encourage investment in projects with long-run time horizons, typical of infrastructure needs. But using interest rates as a short-term device to manipulate spending and control inflation was not, she argued, effective.

The potential scope for the NIB was closely dependent on the national budget, and she proposed a budget structure developed from Keynes's proposals (Keynes 1933 [1972]) and consistent with his later organisation of the national accounts (Keynes 1945 [1980]).² Robinson's budget proposed three separate accounts: an Ordinary Budget, a Social Insurance or Beveridge Budget, and a Capital or Loan Budget, which distinguished between those investments with direct cash returns and those with a social return. The primary goal was full employment, and the means to achieve this lay most directly within the scope of the Ordinary and Social Budgets. These would be in deficit or surplus acting counter-cyclically as unemployment moved with the changes in short-period economic activity; their long-period balance would centre around the budget required to achieve this goal. It was a static argument, and she had not yet explored the implications of dynamic analysis and the associated double role of investment which were central to her post-war thinking (although Domar (1944) argued that a succession of budget deficits, or deficit financing, in a growing economy would generate only limited debt-to-income ratios and tax burdens).

Within this political material, Robinson also extended Keynes's theory to consider Britain's international presence. Full employment had implications for the post-war balance of payments, for both exports and imports and for holdings of foreign currencies. The war left Britain with heavy external debts.

² In his Memorandum, Keynes (1945) [1980] revised his idea of a 'semi-independent statutory authority', an NIB, instead retaining the authority for these decisions with the Chancellor of the Exchequer and the Treasury. He reasoned that 'with modern developments of policy, decisions on such matters [related to the Public Capital Budget overall] have become so much a part of the Government's economic programme as a whole that they should not be dissociated from the Chancellor of the Exchequer as the responsible Minister' (ibid.: 408).

Chronic debt, as also chronic surplus, would put pressure on the exchange rate and then on domestic inflation or deflation. Britain needed to persuade its trading partners to also pursue full employment policies so encouraging markets to expand for British exports. It also needed foreign resources, including financial investment. Such external co-operation, to be supported by new agreements and dedicated international institutions, would enable each country to develop its own domestic plans and Britain to move towards external balance. Britain's vulnerability to the decisions of the USA about trade and finance, including the adoption of the US dollar as the world's leading currency, led Robinson to suggest short-run bilateral agreements with trading partners and even State control of foreign trade at least for the period of post-war rebuilding. She saw capital controls as essential, as did Keynes. Adopting *laissez-faire* in the currency markets at the end of the war, she argued, would encourage speculation and destabilise real capital investment. The changing presence of post-war power, evident in the decisions made at Bretton Woods, oversaw the creation of international institutions designed to manage global difficulties in trade and finance.

When she published these pieces, Robinson had already contributed to international trade theory, arguing against the orthodox doctrine of free trade, in her *Essays* (Robinson 1937b). Here, she systematically works through the logical implications for a central orthodox idea of a single equilibrium exchange rate, of applying Keynesian principles in the context of prevailing domestic and trade and exchange rate practices. She demonstrates that such a unique equilibrium rate does not exist and there are many such rates, each consistent with a different set of conditions (Robinson 1937c: 134–155). In 1944, she extended her critique of orthodox theory in her review of an official US report on its balance of payments in the period between the two world wars. Free trade supposedly gave the best outcomes for all. It was clear that this was not the case when the USA, having expanded its international and domestic activity after the First World War and through the 1920s to dominate the economic activity of the rest of the world, then found itself in deficit (Robinson 1944). Clearly, the corrective mechanisms expected by free trade were not happening—due to either or both errors in its logic or in its applicability—and as the USA tried to slow down its imports, its trading partners, and indirectly the rest of the world, also collapsed. Robinson continued to contribute to the theory of international trade during the 1940s and the discussions at Bretton Woods.

Maurice Dobb had written to Robinson in January 1941 referring to the 'slant that theory has on reality' (Joan Violet Robinson Papers, King's College Archive Centre, Cambridge University: JVR/vii/120/11–13). These

pamphlets and public talks reveal her own slant on theory by allowing her to give thought to what her ‘ideal’ society might need. They also reveal her interpretation of the realities and threats facing the general population and of the actual situations framing any feasible policy approaches. At the same time, she maintained a flow of theoretical publications in academic journals. She continued after the war to apply her analytical skills to writing lucid and topical economic and political commentary in magazines as well as to speaking to wider audiences.

In her Inaugural Lecture in 1965, *The New Mercantilism: An Inaugural Lecture* (Robinson 1966b), she took advantage of her mostly well-educated audience to prompt their thoughts to consider, from a Keynesian and critic of neoclassical theoretical point of view, the pervasive idea that the ideal practice of global economic relations was international free trade. She argued that while a single country, with the right conditions (such as global power over finance), could benefit from it, the world economy did not. In a subsequent article (Robinson 1973), she approached the orthodox doctrine of free trade in the light of the results of the capital theory debates, prefacing her argument with the following: ‘There is no branch of economics in which there is a wider gap between orthodox doctrine and actual problems than in the theory of international trade’ (ibid.: 14). She dismisses the orthodox approach on the grounds of its reliance on an invalid theory based around ‘equilibrium’ and of its irrelevance. She put together her ideas in a set of four lectures given at Manchester University (Robinson 1974, 1979a), demonstrating how Ricardo’s arguments had been subverted by ‘modern’ trade theorists (i.e. including Marshall) to justify relying on comparisons of equilibrium positions of full employment outcomes, and then, as if in a separate world, to introduce Keynes’s analysis linking the trade balance to the domestic level of employment. Keynes’s arguments were essential to understanding why foreign investment could not be examined at such a general level; the nature of that investment and the form in which it is undertaken, together with the many implications of international finance, cannot be explored with the concepts and method of the modern theory of international trade.

7 Growth and Distribution

The Accumulation of Capital (Robinson 1956) was the outcome of a long-held ambition to develop Keynes’s *GT* into a long-run theory of growth. Robinson had published an early version of her thoughts about Keynes in the long run in 1937 (see Robinson 1937a, b), but the post-war demand for reconstruction

of Britain and Europe presented an urgent challenge to theorists as well as policy makers. Robinson's post-war economic analysis was shaped by the classical concerns with growth and the distribution of income between wages and profits (returned to the analytical domain by Sraffa (1951) in his Introduction to Ricardo's *Principles*). She also saw Keynes as having introduced history into the discussion of an analysis of growth with his suggestion of 'shifting equilibria', and the possibility of moving on from the static neoclassical method. Her ultimate goal was a theory of growth and distribution which would replace the neoclassical method of comparing equilibrium positions under different conditions with one which traced the process of moving through out-of-equilibrium situations. First, the basic logical requirements had to be made clear, and the classical schema provided a simple framework from which the implications of changing its assumptions and the forms of its relationships, extending to a Kaleckian-Keynesian theory of effective demand, could be explored. Ultimately, such a theory might lead into a description of endogeneities or path dependence, a dynamic system in which accumulation can be discontinuous and can impact on its own determinants.

Robinson developed her own theory drawing on Harrod's (1948) categories of growth. She had studied the proofs of Harrod's 1948 volume with Kahn and Sraffa (while on holiday in the Dolomites) and published a review (Robinson 1949) of his book. Harrod's method of achieving 'dynamic' analysis was to focus on relationships between rates of change of selected instrumental variables rather than between their levels. He identified distinct categories of possible growth: g_n , the natural rate of growth, reflecting a supply potential and determined by Harrod exogenously, by growth of the labour force and of labour productivity; g_w , the warranted rate of growth at which investment decisions led to just the outcomes that were expected so that no adjustments need be made; g_e , the expected rate of growth corresponding to the expected rate of profits; and g_a , the actual rate of growth. Robinson adapted these categories, each one based on her particular hypotheses relating successive adjustments of rates of profits and accumulation depending on both investors' expectations of future rates of profits and on productivity growth and real wages growth. Her 'banana' diagram illustrated possible outcomes of these. Her starting point was to clarify the conditions for steady growth, and then systematically introduce variations to these to trace their outcomes for distribution and growth. By taking account of the (initially) exogenous determinants of g_n , and of the effects of Keynesian animal spirits on effective demand and on g_w , she creates a set of 'Golden Ages'.

A Golden Age exists when there is growth with neutral technical progress,

without any change in the time pattern of production, the competitive mechanism [is] working freely, population growing...at a steady rate and accumulation going on fast enough to supply productive capacity for all available labour... [In these conditions] the rate of profit tends to be constant and the level of real wages to rise with output per man. There are then no internal contradictions in the system ... Provided that...entrepreneurs have faith in the future and desire to accumulate at the same proportional rate as they have been doing over the past, there is no impediment to prevent them from continuing to do so (Robinson 1956: 99; see also Harcourt and Kerr 2009: 88).

All expectations are realised; g_w prevails and is equal to g_n . Robinson then proceeds, systematically, to set out the different outcomes of different sets of conditions for variations of this equilibrium Golden Age.

Integral to understanding the process of accumulation is recognising the double-sided relationship between the rate of profits and accumulation. Investment, as well as its role in effective demand, at the same time adds to capacity: it has both ‘the relation between the rate of profit *caused* by the rate of accumulation and the rate of accumulation which that rate of profit will *induce*’ (Robinson 1962a: 48; italics in original). Kalecki and also Keynes had observed this interdependency, and Robinson developed it to clarify the responses between accumulation of the capital stock and the distribution of income. Her ‘banana diagram’ (ibid.) illustrates this interdependency in a short-period model, in which the ‘central mechanism...is the desire of firms to accumulate, and [this decision] is influenced by the expected rate of profit’ (ibid.: 47). If, for example, the rate of accumulation is higher than that justified by the associated rate of profits, planned investment will be curtailed and accumulation decline. This creates excess capacity in the sector producing machines (for both its own reproduction and producing wage goods) and investment plans are revised downward in response to lower expected profits. Expected and actual profitability are linked through Keynes’s concept of animal spirits to planned accumulation. Here, Robinson is describing a process, a sequence of responses to risky decisions in changing circumstances.

Her approach was subsequently developed by Donald Harris (1975, 1978) who introduced into Robinson’s theories of accumulation and rate of profits and growth, a production and distribution relationship, between the wage rate and the rate of profits, determined by technical factors and the current state of the class war. Technical changes associated with accumulation

will shift the relationship and possibly also affect the wage rate. In terms of production, the historical and cultural elements of the workplace as well as the choice of technique and productivity growth create a potential surplus. The outcome of this relationship between the (given, money) wage rate and the maximum rate of profits consistent with it is then introduced to the relationships between effective demand, or investment, and the distribution of income between wages and profits. The expected rate of profits will be associated with a desired rate of accumulation: an animal spirits function accommodates the more nebulous elements of uncertainty and risk upon which such investment decisions largely rest, and the necessary relationship between saving and investment is effected through a redistribution of income as the margin on the price of wage goods increases and the real wage falls. Harris demonstrated Robinson's analysis of the potential for crises in the interaction between the rate of accumulation of the capital stock and the distribution of income in historical time.

The process of technological change and choice of technique is an unavoidable part of the investment decision to maintain or expand productive capacity; new investment could incorporate new methods of production. Wilfred Salter, a Cambridge graduate student in the 1950s, described a production process with a changing composition of coexisting machines of different vintages (Salter 1960). He proposed a sequence of investment decisions, whereby the new techniques are introduced only when the existing vintages no longer cover their variable costs and when the latest best-practice machines cover their expected total costs, including the normal rate of profits (assuming competitive conditions).

Harrod had treated labour productivity as given for each long period and g_n as exogenous. Robinson argued that each new investment decision could potentially introduce new techniques embodying increases in labour productivity, thereby increasing g_n . Robinson's (1956) analysis was on the basis of comparisons of different outcomes for different Golden Age conditions. At any moment in time, there is an array of methods available involving different combinations of labour and equipment, which are associated with different expected rates of profit at given levels of costs and prices. To maintain employment, real wages must grow with productivity increases and capacity changes; the real wage must also be responsive to surpluses or shortfalls in available labour.

Robinson's endeavour to introduce the roles and impact of technical change led her into a further critique, of the neoclassical concept of 'capital' and of

the theory of distribution (Robinson 1953–1954). Her article infuriated the mainstream and started a major controversy³ between Cambridge (UK) Keynesians on the one hand and Cambridge (MIT-US) Neo-Keynesians and neoclassical economic theory on the other. The neoclassical approach meant that ‘the rate of return on investment itself enters into the calculation of the cost of any particular outfit of capital goods. For this reason an unambiguous measure of the cost of capital goods requires a definite and universally applicable expected rate of profit’ (Robinson 1956: 103). In order to provide a valid explanation of the distribution of income between wages and profits, in terms of marginal products, a neoclassical aggregate production function required a meaning for and a unit of measurement of ‘capital’ which was independent of prices and distribution of income itself. Her own idea of ‘capital’ recognised its different forms and roles as both finance and actual machines, and was also influenced by Marx and Keynes; the significance of capital lay in its relationships to labour. How, then, should ‘capital’ be conceptualised?

In her article (Robinson 1953–1954), she offered a possible way to deal with the measurability of capital, as real capital—capital in terms of labour time—which gave it a meaning within the aggregate production function as a ‘factor of production’ and a measure in wage units. To be a measure required overall economy equilibrium (Robinson’s) of realised expectations, so that the rate of profits is the same as the rate of interest, and no changes are made to decisions involving capital in either form. The resulting (ex ante) ‘pseudo production function’ showed a set of techniques ordered by successively lower positions of net output per unit of labour, and lower real wage rates associated with higher rates of profits. Switch points between techniques occur at a level of real wages at which the rates of profits are the same for adjacent techniques and at the ruling prices; the values of capital required for each technique are in the same ratio to each other as net profit per labour. A backward switch implies a possibility that the same technique, having been the most profitable over a set of values of wages and profits, could be reverted to at different rates of profits. This means that the production function is not a valid way of representing the neoclassical vision of

³ In her 1975 article for the *Quarterly Journal of Economics* entitled ‘The Unimportance of Reswitching’, Robinson states that ‘The story of what is known as the debate over the reswitching of techniques is a sad example of how controversies arise between contestants who confront the conclusions of their arguments without first examining their respective assumptions. How is it possible to have a controversy over a purely logical point?’ (ibid.: 32).

prices as indexes of scarcity. Similarly, capital-reversing, where a less productive, less capital-intensive technique may also be associated with a lower rate of profits, refutes the existence of a neoclassical downward-sloping demand curve for capital.⁴ The language used in this discussion around the aggregate production function suggests that a process is taking place when only comparisons of different positions can be made. Increasingly, Robinson saw this ongoing episode of theoretical and often personal and political hostility as being of little consequence and, indeed, a distraction from the development of an adequate theory of capitalist growth.

Moving on from the frustration of the (continuing) capital theory debates, Robinson's critique of neoclassical theory returned to her earlier criterion of relevance, almost taking for granted her own sufficient refutation of its internal logic and the implications of Sraffa's 1960 *Production of Commodities by Means of Commodities*.

For Robinson, 'relevance' was directed at replacing a method that was static with one that was about process, and it indicated the need to consider what happens with the passage of time, how the history of a situation is crucial for understanding how—with its present context of institutions and practices, a mixture of political, social, and economic, of institutions embodying cultural tradition and class structure—the processes of growth and distribution occur. Her analyses of growth theory had concentrated on theories in 'logical' time where relevance was directed to assumptions. Now, by the end of the 1960s and through the 1970s, relevance was more about method, and she wanted to address theories in 'historical' time.

Logical time could compare different equilibrium outcomes; it was static. Theories in historical time could (aspire to) trace a process of more complex changes which have no connection to 'equilibrium'. She argued that the historical approach inspires more interesting and pertinent questions because it arises out of causal hypotheses. Introducing such 'relevance' means the theory may not be 'scientific' if less precisely defined and measurable factors—non-scientific—were to be included in the causal list.

Robinson's own use of models in logical time was intended as a crucial prologue to her more complex historical theories. Such models enabled the purely deductive outcomes of systematically varying the model's conditions, to be established prior to introducing 'real' features. Their solutions provided sets of simultaneously compatible equilibrium values and so carried no causal status. She constantly cautioned her readers to the limits of such abstract

⁴A comprehensive and pellucid account of the many associated issues and proposed solutions and analyses, of the participating authors' ingenuity and resilience in response to criticisms of logic, up to the early 1970s, is to be found in Harcourt (1972) and more recently discussed in a symposium in the journal of the *Union of Radical Political Economy* (2014).

methods and their interpretation (see above, pp. 679–680; also see Robinson 1962a: 87, Conclusion).

Robinson was also highly critical of the associated, neoclassical use of equilibrium, as a concept and as an analytical device. Her own use of ‘equilibrium’ in her Golden Age models referred to a position or rate of growth at which an economy remains if all expectations have been satisfied and there is no reason to change the conditions or (investment) decisions by which it got there. She is careful to make the purely abstract nature of ‘equilibrium’ clear: if the economy is in ‘equilibrium’, then it must have always been there; it must have a composition of means of production perfectly suited to meeting the demands for its output so that stocks do not have to adjust between periods; the present is fully determined by its ‘history’ and to be there means its past must be exactly replicated in its present and its future. Logically, the economy could not move from one quantitative configuration of stock to another without having had a different history. Her claim of the irrelevance of the static method itself included the associated role of ‘equilibrium’ in neoclassical theory as an analytical device, of affirming that static method through the closure conditions it required. For Robinson, there was no role for equilibrium in historical analysis. The neoclassical equilibrium proposed *tâtonnement* as the causal process of reaching this point, which Robinson dismissed. Her critique was not that there were interferences with this process reaching its equilibrium and for which ‘corrections’ could be introduced and considered theoretically; it was that her ‘vision’ of the economy and the concepts and reasoning she ultimately chose to represent it were quite different.

Robinson’s analyses in the 1970s were shifting from a long-period ‘generalisation’ of Keynes in the 1950s and 1960s to accommodating the presence of more short-period factors, familiar to her and in different ways, from Kahn and Kalecki, in which the economy experienced cycles as well as long-period trends. In 1952, she had explored the likelihood of a capitalist economy following a long-period growth trend through cycles of short-period (dis) equilibrium—in the sense that short-period off-trend behaviour caused no changes to be made to expectations and investment. The independence or interdependence and indeed recognition of these as two phases of economic activity have provoked controversy in both empirical and analytical studies, and in the respective claims of theories about trends and cycles. Robinson’s early stand was to treat the two as quite separable, but she became more ambivalent and inclined to follow the Kaleckian-Marxian implications of her existing analysis. The actual structure of Kalecki’s analysis made cyclical models part of the ‘natural order’ of capitalism. On the other hand, Pierangelo Garegnani, who was crucial in developing Sraffa’s classical concerns and method, and who supported Robinson in her rejection of neoclassical equi-

librium method, completely took issue with her over the status of the long period in economic methodology and whether there actually was a centre of gravitation towards which the capitalist economy was drawn.

Garegnani argued (as did Dick Goodwin initially) that the long period was the only meaningful context for understanding growth and distribution; there was no relevant, systematic short-period behaviour. Robinson, however, identified the existence of (asymmetric) cycles and initially gave independent status to the theories of the cycle and of the trend, later identifying links between the two processes. Ultimately, she seemed to agree more with Kalecki's (1968) and Goodwin's (1953, 1982) conclusion that trend and cycle are 'indissolubly mixed'. Like Robinson, they based their arguments on a Marxist understanding of contradictory forces in capitalism, demonstrating in an aggregate model the inevitability of cycles disrupting any trend, and the intersectoral constraints imposed in a disaggregated model of production interdependencies; such 'shocks' themselves are a product of capitalistic societies, and the trend has no independent existence; it was an *ex post* statistical artefact.

That both trend and cycle are integral parts of the same process of capitalist dynamics can shift the dilemma of a connection between short and long periods to a prior level of generality. The result is the coexistence of opposing visions in none of which demand and supply, and its equilibrium, play any central role and whose respective, ongoing developments underlie the emergence of separate major schools of thought in Post-Keynesian growth and distribution theory (see Harcourt 2006).

Responses to her general critique of the inherently static nature of marginalist method and theory, and so too to the relationship between trend and cycle, continue, over 30 years later, and include Neo-Keynesian theorists. Paul Samuelson and Robert Solow both sustained a defiant correspondence with Robinson around issues connected, at first, with Keynesian growth theory and then capital theory. Fifty years on, they were still addressing her challenge concerning the static method of neoclassical economics, although with mathematical tools. They both use a method of analysing processes as distinct from identifying differences while remaining within a neoclassical vision of accumulation, growth, and distribution. Solow (2010) specifies separable trend and fluctuation. This changes the vision of capitalism from Robinson's Kaleckian-Keynesian endogenously generated movements of growth, where growth and cycle processes are accounted for within a common theoretical structure, to one of exogenous and unpredictable 'shocks' to the otherwise steady process, a trend, of growth. Solow separates the theory of demand from the main determinants of growth, as explained by an aggregate production function. What is needed, he suggests, is 'a theory of the evolution of invest-

ment, capital stock and level of technology' (ibid.: 60), a recurring sentiment in the history of economic thought. Hence, a 'medium-run' offers a way of linking the (short-period) theory of recession or inflation and the long(er)-period theory of trend. This is not a coherent solution, but it is consistent with Robinson's 1952 suggestion and also with Garegnani's fundamental 'vision', even though they held opposing theories. More than 30 years after her death, Joan Robinson has critics who still do not, or cannot, answer her arguments.

In 1980, Robinson published a joint paper (Bhaduri and Robinson 1980) in which she sought to place her historical method into a more fundamental framework, and to synthesise, or at least reconcile, those specific theories which she considered robust and relevant to her sense of the inequities of capitalism. The aim was a unifying structure with a classical vision of the circularity of reproduction set out in its schema, whose links between different economic processes could be seen to derive from the more general properties of capitalism.

The authors integrated into one structure a two-sector production analysis which they called 'Sraffaesque'⁵ and a Kaleckian-Keynesian theory of effective demand and capitalist distribution. The analysis of the distribution of the product takes place at the level of class so that the political relations involved need to be consistent with the assumptions about the class nature of expenditure and saving. This is illustrated in Kalecki's two analyses of profits: the share of gross profit in the product of industry is determined by the level of the gross margin, which involves a discussion of market structure; and the aggregate flow of profits per annum depends upon the aggregate flow of capitalists' expenditure on investment and consumption. With this classical framework, Robinson can introduce 'historical' features into the logically prior analysis of production. This means accommodating implications of 'time', for both the conditions and relationships, and the authors describe the assumptions that need to be made if the investment decisions are to be appropriate to the technical requirements of the composition of spending—or the distribution of income. Past expectations of profitability are manifest in the existing stocks of machines and of goods, and these will only be 'correct' if expected profitability is realised. As with 'equilibrium', the outcomes of different distributions of income, or rates of exploitation, for a uniform rate of profits, can only be analysed in a more abstract model with necessary and restrictive assumptions not applied to any particular case.

⁵ Sraffa's work has generated many lines of research. One important direction in Cambridge growth theory looks at the process of structural transformation in response to productivity growth and changing consumer tastes. Luigi Pasinetti (1993) developed a pure production model of structural change based around Sraffa's (1960) scheme. His pioneering work has generated its own questions and lines of development, sometimes referred to as the Anglo-Italian School.

This analysis of the actual 'share of wages in net output (and therefore the potential ratio of profits on capital) depends upon commercial, social and political influences and the fortunes of the class war' (Bhaduri and Robinson 1980: 111): '[T]he workers' share is governed by the ratio of exploitation, while accumulation...turns the potential share of the capitalists, set by the ratio of exploitation, into the corresponding rate of profit by creating enough effective demand to realise it' (ibid.: 109). The authors conceptualised the capitalist process in a language (with which Marx may not have been comfortable) of contemporary capitalist forms and abstract methods qualified accordingly. Their approach inspired real and analytical questions and underlies much of the subsequent Post-Keynesian developments.

8 Socialism and Planning: Development Issues

Robinson had confronted some of the problems of undeveloped countries during the two years spent with Austin in India in the 1920s. Her interest resurfaced in the post-war period, alongside her emerging theoretical work. She was a keen traveller and enthusiastic to learn about the places she visited. Robinson visited economies struggling with emergent capitalism and others following socialist objectives and plans. Their common and overriding problem was poverty, and this was usually compounded by technical backwardness and entrenched, vastly unequal distributions of income and wealth at low levels and rates of growth of output. Together, these made the domestic surplus needed to improve investment seem impossible: access to foreign sources was another extreme difficulty. The precarious state of an impoverished economy under capitalism was apparent in the many destabilising versions of capitalists' rules in different places and times. She always advocated some form of socialism for the greater potential it had through planning, to improve output by providing greater certainty and efficiency and to make possible a more equitable distribution of the surplus (see above, Section 6). Robinson also considered that socialism allowed a more moral life, that individuals would respond to moral incentives to act out of their own interest to favour a common 'good' (Robinson 1962b).

After the war, Robinson (among many other Western socialists) looked to existing socialist countries with a rather uncritical and visionary trust in their futures. Robinson paid particular attention to China; over 30 years, and visiting eight times between 1953 and 1978, her thoughts on the path of Chinese socialism changed over this time as its own vision changed. At all times, she emphasised

the necessity for a high rate of capital accumulation tempered by politically and humanely realistic levels and rates of growth of real wages. Population growth had to be curtailed and urgent changes made to the organisation of the agricultural sector. This was not straightforward because of its fundamental (and non-marketed) role in the well-being of the population and the scale of its reorganisation. Her interpretation of the political economy, the official policies and manner of their implementation, was controversial within China and among mainstream China scholars in the West. (For a more detailed account of her involvement with China, see Harcourt and Kerr (2009: Chapter 9) and Tahir (1990).)

After 30 years of observing and discussing the experiences of many countries, Robinson brought her views together in *Aspects of Development and Underdevelopment* (Robinson 1979b). Her involvement in development economics had included a critique of the mainstream, largely neoclassical approach which based its advice on an aggregate production function and a laissez-faire approach to international trade and finance. Her own analytical approach drew largely on her logical analyses and models of accumulation and distribution (Robinson 1956, 1962a) and of international trade while also taking into account her many observations and experiences of the countries she visited. This book brought together many of her long-standing political and economic grievances, particularly those connected with global capitalism, as experienced by its most helpless victims.

9 Political Economy

After retiring (sic) in 1971, she wrote, with John Eatwell, an alternative, political economy, undergraduate textbook of rather a different understanding of Keynes and capitalism than Samuelson's standard (Robinson and Eatwell 1973). It was not a success, perhaps because the experience of the authors was with students in the Cambridge Tripos and its Cambridge Keynesian lecturers. Also, the audience whom Eatwell and Robinson had in mind was first year undergraduate students, particularly those in the USA, for whom a combination of their own lack of background knowledge of the content and of experience in argument and her own (typical Robinson) style, succinct and even obscure, discouraged it from widespread use. Robinson had optimistic expectations that this introductory text would make significant inroads into American universities and for (Cambridge-UK Keynesian) political economy to become part of standard economics courses; it would, at least, make students

aware that they were being offered only one option, in the guise of being the only option, through which to analyse economic problems. There was during the 1970s much campus interest in her political views as a speaker, and in her theoretical challenges to the economics being taught, but these were complex critiques in terms of unfamiliar literary methods on which those sentiments were based. Nevertheless, for the better prepared, the book remains an excellent guide to the basic principles and developments of a Cambridge Keynesian political economy approach up to that time, and to its critique of the mainstream.

Her reputation as a significant theorist among American economists had been established with the publication of *EIC* and particularly noticed because of its coincidence with Chamberlin's *The Theory of Monopolistic Competition* (1933). However, her *Essay on Marxian Economics* (Robinson 1942) was met with scandal, and her outspoken views on socialism and even communism distanced her from the mainstream of economics in the USA during and after the war. From her point of view, America represented all that she disliked about capitalism. In the immediate post-war period, economists in both UK and USA were developing their own versions of growth theory. There were 'Keynesians' in both locations, but it took time to bring to notice that this label meant different things for each. The Cambridge-UK model argued that investment created the necessary savings while in the Solow–Swan model, for example, it was the reverse. The gap between the two was compounded by their separate methods of theorising: US economists used mathematical models, while Robinson used a more literary form of varying precision. This meant that debate between Robinson and leading US economists did not lead to many 'conversions'. Solow and Samuelson, both Neo-Keynesians, nevertheless corresponded with Robinson, tolerating her impatient responses to the many disputed points.

She maintained a controversial profile in the USA during the ensuing capital debates of the 1960s and into the 1970s with complex theoretical contributions, in mainly UK and European journals. Her post-war books aroused interest and were reviewed in significant American journals, albeit often with hostility. Her excellent knowledge of Marshallian-based neoclassical theory made her an intimidating opponent in argument. Her confrontational style and unrelenting tenacity possibly compromised her acceptance even further than did her politics by many US conservatives. On the other hand, she was also attracting interest from scholars sympathetic to her critiques of both the irrelevance and the theoretical failings of the mainstream. *Monthly Review*, a well-regarded,

left-wing US magazine which discussed issues of politics and economics, did publish her articles. She first visited the USA in 1961, invited by Robert Clower initially but also giving seminars and lectures at Harvard, MIT, and Chicago, among other universities, making new friends and more hostile critics.

In 1971, John Kenneth Galbraith, as the elected president of the American Economic Association, invited Robinson to deliver its annual and prestigious Richard T. Ely Lecture. She met Galbraith in 1937 when he spent a year at Cambridge, and they had remained lifelong friends. Her lecture was 'The Second Crisis of Economic Theory' (Robinson 1972), and she used the opportunity to once again criticise the inadequacy of contemporary theory to address the crises associated with its free market objectives, a theme she revisits in both public lectures and well-placed essays. As her interests moved on from capital theory to developing a theory of growth and income distribution as an historical process, her arguments, always sound, took on an even less precise form with the introduction of 'historical' concepts. However, this made it easier for the bulk of the mathematicians-cum-economists to ignore her contributions. Through the late 1970s and into the 1980s, she gave more time to student requests, from both undergraduates and graduates, for her to visit and give seminars on the economic theory which could replace their standardised models and provide a meaningful basis for policy. Her hopes of making a difference to economic theory in the USA were now placed on younger economists who wanted to learn and away from the stalwarts with their vested interests.

Robinson often drew the attention of economists to their responsibility to recognise the ethical aspects of their arguments. In 1938, she became very ill. Her sensitivity and intensely felt horror of the persecution of war meant her views towards Britain's role in opposing fascism on the Continent conflicted with the stance of her military family and Austin. In letters between Kahn, Austin, and Keynes at this time, her isolation and despair were considered to underlie her illness (although Keynes thought that the stress of intellectual work and babies together also contributed). The hopelessness of war as a solution seemed to her obvious. These were views which reappeared in her work at numerous times and places. She gave a lecture to students in 1941, beginning with a passionate plea to their humanity and moving on to consider the wastage of destruction and of resources used in producing armaments. In her writing on developing economies, she alerted them to beware that promised 'aid' should not in fact be tied to military contracts. In 1981, she delivered the Tanner

Lecture on Human Values at the University of Utah, her subject being the arms race and the supposed economic necessity for arms expenditure. She clarified the fallacies and errors in the frequently voiced economic justifications, which often deferred to empirical or theoretical support, even 'proof', for the continued pursuit of the broad-ranging dynamic of the military–industrial complex. Despite her clarity and ordered argument, her trust in human nature seemed in doubt.

10 Conclusion

It is impossible in an essay of this length to appreciate all of the contributions that Joan Robinson made to economics. She had many interests which were not central to her focus but which nevertheless inspired further research by others; she had a mode of theorising which was intrepid and tenacious, always aware of (often unstated) assumptions and their limitations; she was lucid and incisive and would write or speak with her particular audience in mind (although her judgement of this may not have always been appropriate). The many strands to her ideas have inspired and provided the analytical bases for the many branches of Post-Keynesian theory which have subsequently developed, with their own debates, and with dedicated journals.

Robinson had once said that her publications were not her final word on an issue; her ideas were always open for discussion. It is interesting to read her essay 'Thinking about Thinking' (Robinson 1979a: 110–119), which displays her frustrations with neoclassical economists persisting with their use of methods and arguments which she had so forcefully dismissed. She concludes the essay with an optimistic challenge to 'radicals' to commit not only to their beliefs but also to the more demanding exercise of generating the necessary underlying theories; she retained her faith in the ultimate power of reason. During the autumn/winter term of 1982, Robinson visited Williams College in the USA, a trip taken against her doctor's advice but which also allowed her to visit her daughter and her family in Canada. Her time at Williams was mostly spent with the students, formally and informally, and the visit was a rewarding one. On returning to Cambridge, she expressed her hope to her Williams 'minder', Juliet Schor, that the friendships she had made there would continue through correspondence (see Turner 1989: 204–207). However, her health deteriorated and, in August 1983, she died. She had been formidable and humane and remains an inspiration and model.

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31

Richard F. Kahn (1905–1989)

Maria Cristina Marcuzzo and Annalisa Rosselli

1 Introduction

In his short autobiographical note (Kahn 1984), with characteristic modesty and sparing use of words, Richard Kahn set out the basic facts of his apparently unglamorous life, which was however rich in emotions, replete with academic honour, and burdened with many teaching and administrative responsibilities.

He was born on 10 August 1905, in London, son of a government inspector of schools; from 1918 to 1924 he attended St Paul's School, London, and went on to study mathematics and physics at King's College, Cambridge, achieving modest results in the Natural Sciences Tripos. Thus he fell short of qualification to enter the academic world, but the early disappointment turned into success. Since he still had a further year's scholarship available, under the guidance of Gerald Shove and John Maynard Keynes he set about studying for the Economics Tripos, which he passed with brilliant results in June 1928. It is worth noting that the dissertation he wrote between October 1928 and

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December 1929 to obtain a Fellowship at King's was also the happy outcome of initial failure—in this case to gain access to the Midland Bank's monetary statistics, which Keynes thought could offer good material for his dissertation. Since the data could not be used, Kahn was allowed to choose his own subject. Under the influence of Marshall's *Principles*, and following a suggestion by Shove and Piero Sraffa, he chose *The Economics of the Short Period* (Kahn 1989). The dissertation secured him election as a Fellow of King's on 15 March 1930; with very few interruptions, notably during the Second World War and a sabbatical year in Geneva, Kahn spent all his life in his College, where he supervised students in economics from 1936 to 1951, having moved, after Keynes's death, into his rooms. He died in Cambridge on 6 June 1989.

He was very active and influential in the Faculty of Economics and Politics at Cambridge, where he started as Lecturer in 1933, finally becoming Professor in 1951. He chaired many committees and masterminded academic activities and appointments. He had several administrative duties at King's: in particular, he was appointed Second Bursar in 1935, and subsequently succeeded Keynes as First Bursar in 1946, also becoming executor of his will and trustee of his estate. In this capacity, he was responsible for the stock exchange portfolio of the Keynes Trust until Lydia Keynes's death in 1981.

At the outbreak of the war, Kahn worked at the Board of Trade, where he was involved in the point-rationing scheme to curb consumption and free resources for the war effort; he then acted as Deputy Director of the Middle East Supply Centre in Cairo, where he was given many administrative duties. Subsequently, he moved on to the Ministry of Supply, where he started working on issues related to post-war organization of international economic institutions, and became Head of the General Division of the Board of Trade after the end of hostilities. From the late 1940s he worked for a number of international organizations (the Organisation for European Economic Co-operation (OEEC), the United Nations Conference on Trade and Development (UNCTAD), and the Food and Agricultural Organization (FAO)) and for various British Labour governments, for which he drew up incomes policy schemes. In the 1960s he served for three years as part-time member of the National Coal Board. He received a life peerage in 1964.

Starting in the early 1930s he invested in shares, bonds, and commodities with alternating fortunes. He was in charge of the finances of friends and relatives and devoted a considerable part of his time to the management of their savings. Unlike Keynes and Sraffa, with whom he frequently discussed financial matters, he made modest gains and did not die rich.

He was an enthusiastic mountaineer and even late in life would still spend most summers in the Alps. In all matters he was a careful planner, holidays included. He never married, but his lasting relationship with Joan Robinson

was ever at the centre of his sentimental and intellectual life. He always remained very close to his family, particularly his three sisters, supporting them financially and emotionally. An important aspect of his life was his strong Jewish identity, even when he gave up religious practice, as testified by the fact that he wanted to be buried in the Jewish part of the Cambridge cemetery (Pasinetti 2007).

Kahn did not publish much, but his contributions are landmarks in the economics of the twentieth century. A complete bibliography can be found in Marcuzzo (1989). In the following pages we aim to offer a critical exposition of Kahn's main contributions to our understanding of the working of the economic system by focusing on the aspects which best show his originality and innovative thought. He investigated a wide range of topics: employment, money, market structure, international trade, and systems of payments. He liked to approach a problem in general abstract terms but, when necessary, he was also interested in carefully reconstructing the details of the actual workings of the economy. He brought to his research in economics his strong logical powers, a perfectionism which came close to becoming an obstacle to publishing, a strong commitment to the public cause, and a love for the Cambridge way of life.

In the following sections we present Kahn's scientific works under three headings: Employment, Liquidity, and the Fight Against the Quantity Theory of Money (Section 2); Markets and Prices: Superseding the Theory of Perfect Competition (Section 3); and Reforming International Economic Institutions (Section 4). Rather than following a strict chronological order, we have grouped Kahn's contributions according to subject matter and followed the evolution of his thought within. The order and length of the sections reflect what we believe to be the level of their theoretical significance. Furthermore, in Section 5 we show Kahn as protagonist and champion of Cambridge economics, in close association with Keynes and Robinson. In the concluding section we draw on his parliamentary speeches to reveal the force of his commitment to the cause of full employment.

2 Employment, Liquidity, and the Fight Against the Quantity Theory of Money

Kahn is by no means the most widely known of the Cambridge economists, and if he is known it is mostly in association with the concept of the multiplier, which he presented in an article 'The Relation of Home Investment to Unemployment', published in the *Economic Journal* (Kahn 1931). Even less

well known is how he contributed to the development of an approach which turned out to be pivotal in Keynes's *General Theory*, namely the concept of the short period, which he presented in his Fellowship dissertation, and in an unpublished and unfinished book under the same name (Kahn 1932a).¹

Kahn accepts Marshall's distinction between the short period—when the productive capacity of the firm is constant both in terms of physical capital and organization of production—and the long period, defining the distinction as 'one of Marshall's most fruitful contributions' (Kahn 1989: xiii). The distinction is not seen as grounded on logical foundations but as a matter of fact, that is, even if *natura non facit saltus*, there is a factual dichotomy between the life of fixed capital and the period of production, the former being considerably longer than the latter (Kahn 1932a: Chapter 2, page 2). The productive life of physical capital is not distributed along a continuum, but, as Kahn noted (1989: xiii): 'As a general rule, the life of physical capital is illustrated either by the mayfly or by the elephant'.

Although the short-/long-period distinction has a factual basis, the actual length of the short period cannot be defined on the basis of technological considerations alone. These determine only the upper and lower boundaries of the interval in which the short period varies because fixed capital cannot remain constant for a period longer than its physical life or be substituted before the period of production comes to an end. Within this interval, the length of the 'short period' is determined by the expectations of entrepreneurs regarding demand conditions. Accordingly, depending upon whether entrepreneurs believe changes in demand to be transitory or permanent as compared with the level considered as 'normal', the decision on whether to modify plant or organization—typically long-period decisions—will or will not be taken.

The two aspects—the nature of the production process, which is characterized by changes that occur rapidly (such as output and employment) and others that occur only slowly (such as alterations in fixed plant) and expectations of changes in demand relative to the level perceived as normal—are combined to explain why in the short period productive capacity is not

¹One draft is extant, with annotations and related material, amounting to roughly 300 pages (RFK/2/7. References beginning with RFK are to the documents preserved in the Richard Kahn Papers, King's College Archive Centre, Cambridge University). Of the planned 11 chapters, according to the index, Chapters 1, 3, and 4 remained unwritten, while Chapters 7, 9, and 10 are seemingly unfinished. The draft was almost certainly written in the last quarter of 1932. We will refer to it as Kahn (1932a). Part of Chapter 7 merged into 'Imperfect Competition and the Marginal Principle', which was an article Kahn submitted to Frank Taussig in 1933 for publication in the *Quarterly Journal of Economics* and which, having been rejected, still remains unpublished in English (Marcuzzo and Rosselli 2008). We will refer to it as Kahn (1932b).

altered. This is so because a change in the conditions of demand is not perceived as permanent; in fact, the ‘ideal’ short period is defined as a situation where ‘any change that occurs is not expected to be permanent’ (Kahn 1932a: Chapter 2, page 10). The conditions prevailing in the economy—depression or boom—mould expectations of a return to normal conditions of demand and introduce an asymmetry in the length of the short period. In a boom, short-period equilibrium implies that expectations are such that increasing production, at higher costs, is preferred to building up capacity until the increase in demand is confidently perceived as ‘permanent’; the short period, however, can be very short and innovations are introduced rapidly. In a depression, short-period equilibrium implies expectations that demand will return to its normal level; in this case, the short period can last decades if the firm survives (at a loss) while its physical capital is decaying. Since what matters are expectations regarding the normal value of the level of demand, it follows that the short period need not be a ‘short’ time interval, nor is it a transitory state before the long-period forces work out their effects. It is, rather, a position which is maintained as long as the set of decisions, depending upon the expected level of demand, do not change. This attributes to short-period equilibrium a meaning showing a fairly evident analogy with the unemployment equilibrium of Keynes’s *General Theory*.

Turning to another major contribution by Kahn to Keynesian economics—the multiplier—its importance lies in providing the terms for analysis of the conditions that see an increase either in the level of prices or in the quantities (or a combination of the two) in aggregate, following an exogenous increase in demand (in the case presented in the 1931 article, public investments in road building). In a depression—as prevailed at the time—Kahn showed that the effect on quantity outweighs that on prices, so that an autonomous increase in demand could boost income and employment. The multiplier has a story of alternate acceptance and rejection in the 80 plus years of its existence. After a long period of neglect and suspicion by the majority of the economics profession, it has come back into favour.²

Kahn was very close to Keynes in the years preceding and following the publication of *The General Theory*. He played the leading role in orienting Keynes’s thought in certain significant aspects through discussion and debate during the period when *The General Theory* was taking shape. In particular,

²As recently noted: ‘In the 1950s and 1960s, when Keynesianism was in contrast at its height, the multiplier was generally assumed to be about 2. Then in the 1990s and 2000s, these estimates gradually dropped, leaving the consensus range about 0.5–0.7 by 2009’ (Davies 2012: 3), in contrast with the current figures of 0.9–1.7 presented in the 2012 *World Economic Outlook* by the International Monetary Fund.

the influence of Gerald Shove and the collaboration with Joan Robinson during the writing of the *Economics of Imperfect Competition* (see below) persuaded Kahn of the need to recover, rather than discard, Marshall's apparatus, against Sraffa's criticism in his 1926 article.³ The task—as Kahn saw it—was to transform certain concepts into precise analytical tools and apply them to Keynes's new ideas to obtain logically coherent results. The framework Kahn provided—the aggregate demand and supply curves—arguably had the eventual effect of taking the presentation of *The General Theory* in directions that departed from Keynes's approach in *A Treatise on Money*, and at any rate of restricting its scope, but it helped Keynes to finally shake off the 'stranglehold' of the quantity theory of money. Kahn always prided himself on 'finally disposing of the idea that the price level is determined by the quantity of money' (Patinkin and Leith 1977: 147).

After Keynes's death, Kahn carried on his battle against the quantity theory to the end of his life. Opposed as he was to an approach to monetary policy which paid exclusive attention to the money stock, Kahn developed his own theory of money following lines which moved him slightly away from Keynes. In particular, Kahn was critical of Keynes's representation of the demand for money as a smooth curve, which could be placed at the same level of importance as the consumption function or the marginal productivity of capital as a key component of the structure of the economic system (as it was in fact placed within the IS–LM framework). This was not the only source of dissatisfaction with *The General Theory*, as Kahn explained in 'Unemployment as Seen by the Keynesians', where he disapproved of the way Keynes defined involuntary unemployment, going so far as to say that the second chapter of *The General Theory* always left him 'very cold' (Kahn 1976a: 23). However, where Kahn distanced himself most from Keynes was in relation to the liquidity preference theory.

In his essay 'Some Notes on Liquidity Preference' (Kahn 1954),⁴ Kahn defends Keynes's analysis of the long-term rate of interest as depending on the expectations for future rates held with uncertainty, and puts forward his own views on the workings of the financial markets and the behaviour of investors. According to Kahn, in the financial markets there are two kinds of investors: those who hold definite expectations for the future of interest rates and those 'who do

³ Sraffa (1926) produced a devastating critique of Marshall's supply and demand apparatus for the analysis of competitive markets, showing that the assumption of increasing costs underlying the upward-sloping supply curve was inconsistent with the assumption of partial equilibrium or was confined to the case of an industry which employs the totality of a productive factor. However, at the time, Sraffa was not yet ready to offer his alternative theory based on the surplus approach.

⁴ An excellent analysis of Kahn's theory of liquidity preference can be found in Dardi (1994).

not have a clue' (ibid.: 241) whether the rate of interest is going to increase or decrease. The same division exists in the mind of the investor himself, who can reveal contradictory preferences and decide to hold money and securities at the same time, while rational behaviour (and expected utility theory) would imply a definite choice between the two. On the basis of this observed behaviour, Kahn argues that a finite elasticity of the demand for money relative to the rate of interest is associated not only with the heterogeneity of opinions held by the public divided between bulls and bears, but also with the lack of conviction individuals show in their own conjectures. It is as if bullish and bearish sentiments 'operated inside each person's mind, one being responsible for his holding securities and one for his holding money' (ibid.: 247). This analysis implies the 'unsuitability of thinking of a schedule of liquidity preference as though it could be represented by a well-defined curve or by a functional relationship expressed in mathematical terms or subject to econometric processes' (ibid.: 250).

In terms of monetary policy, it follows that since the monetary authority would be facing a shifting schedule of liquidity preference, a given quantity of money cannot be taken as a policy target. As Kahn confirmed in his evidence to the Radcliffe Committee, the quantity of money necessary to bring about a fall in the rate of interest varies with the circumstances (Kahn 1960: 742) and the state and responsiveness of the market: 'If the market likes to help by altering its expectations... the authorities will not have to do so much. If the market proves very obstinate and does not believe that they are going to succeed in doing what they are trying to do, then they will have to do much more' (ibid.: 743).

In the 1970s—when the tide was turning against the hegemony of Keynesian thought and the Western economies were hit by levels of inflation such as had never been reached in the post-war period—Kahn deployed all the defences he could muster against monetarism, which, to all intents and purposes, amounted to the restoration of a pre-Keynesian approach. In fact, when Margaret Thatcher became Prime Minister in 1979, for the first time since the war the British government ceased to proclaim the goal of full employment, for, according to the monetarist philosophy behind official policy, it was not an objective which the government could pursue directly with policies to support demand. What the Conservative administration aimed at was cuts in public spending, the tax burden and the economic demands of the public sector, and the restoration of market mechanisms. It was these mechanisms that were supposed to boost employment indirectly. In addition, the goal of reducing inflation was to be achieved through a decrease in the money supply growth rate, this being in sharp contrast with the incomes policy advocated by Kahn (1976b, c).

Even today, the all-time record of money growth expansion overseen by the Federal Reserve, the European Central Bank, and the Bank of Japan to fight the present downturn has proved Kahn's arguments: markets have been flooded with liquidity, but rather than inflation we have witnessed a general deflation, in the absence—with the possible exception of the USA—of that expansionary fiscal policy which the multiplier argument suggested as the means to increase income and employment.

3 Markets and Prices: Superseding the Theory of Perfect Competition⁵

In his dissertation, reviewing the cost structure of the typical firm in the British cotton industry, Kahn argued that in the short run the average cost curve of the firm is not U-shaped, but is an inverted L, meaning that the average cost remains constant up to a point corresponding to the maximum obtainable level of production. Given this form of the average (and marginal) cost curve and the assumption of a horizontal demand curve in perfect competition, the only way to determine the level of production at which the firm maximizes profits is by postulating imperfect competition, or in other words that the firm is facing a downward-sloping demand curve. In this way Kahn explains why the degree at which productive capacity is exploited during a period of economic downturn falls below full capacity for all the existing firms. His account tallied with the empirical evidence, which revealed that all the firms in question worked full time for some days of the week and suspended production for the rest, but clashed with the theory of perfect competition where it stated that only efficient firms surviving the competitive struggle would be able to continue production, and at a level corresponding to full capacity, while the others would stop production altogether (Marcuzzo 1994).

The dissertation formulates two fundamental questions that arise once the hypothesis of perfect competition is abandoned: (1) If the firms are no longer subject to perfect competition but decide the price themselves, how will they take into account the behaviour (the reactions) of the other firms in the same industry?, and (2) Is the pursuit of maximum profit the best way to describe a firm's behaviour? Three papers by Kahn, 'Imperfect Competition and the Marginal Principle' (Kahn 1932b), 'The Problem of Duopoly' (Kahn 1937), and 'Review of *Oxford Studies in the Price Mechanism* edited by T. Wilson and

⁵This section draws on Marcuzzo (2011).

P.W.S. Andrews' (Kahn 1952a), represent successive stages in the evolution of his thought and the contemporary literature that developed on the subject. However, what they have in common is the same approach in answering these two questions.

As to the first question, in the *Principles* Marshall had addressed the problem of the determination of monopoly equilibrium, but he had not ventured very far into the effects of the behaviour of rival firms on a firm's demand elasticity, or, indeed, into the effects of one firm's price policy on the behaviour of rival firms. Despite the works by Cournot and Edgeworth on duopoly, at the time that Kahn's dissertation was being written, a decisive solution to the problem had yet to be arrived at and integrated into general analysis of imperfect competition. There are two aspects of particular interest marking out the dissertation from the previous literature on oligopolistic markets. The first is Kahn's invention of a way to measure the degree of market imperfection assuming linearity in the demand and average cost curves, through the so-called annihilation coefficient (Kahn 1989: 121), which is conceptually identical with that measurement that was subsequently to become known as the 'degree of monopoly'.

The second interesting aspect is the approach taken to the effects on firms' demand curves when one of the rival firms changes its price. A whole section of the dissertation is dedicated to this issue, and here Kahn also criticizes the conclusion Sraffa had arrived at in his 1926 article, to the effect that in an imperfect market the final equilibrium price is the price that would be arrived at if the market were entirely controlled by one single monopolist. Kahn argues his point on the basis of the idea that the slope of the individual demand curves reflects the conjectures that each firm makes about the behaviour of the others. Kahn demonstrates that whatever the hypothesis on the conjectures, the relation between the individual and market demand curves is such as never to produce equilibrium at the monopoly price, but at a lower price, and consequently with a greater quantity than would have been chosen by a monopolist in the same technical conditions (*ibid.*: 117).

Thus, we must reckon Kahn as among the inventors of the kinked demand curve, even if he defines his treatment of the problem as 'primitive and incomplete' (*ibid.*: xx). However, it had the merit of introducing 'conjectures' into demand curves, in the form of elasticity values, for analysis of price and quantity produced in a duopolistic or, more broadly speaking, oligopolistic markets. In 'The Theory of Duopoly', which became a classic on the subject, without any mathematical tools, Kahn goes through the whole complicated discussion on the effects of the actions of one duopolist on the behaviour of

the other, and thus the role of the strategies implicit in every move of each firm, anticipating the later development in the theories of oligopoly.

As to the second question—the validity of the hypothesis of profit maximization—and in contrast to the traditional marginalist analysis, Kahn unfailingly considered it as the one certain rule upon which to build economic analysis, but interpreted the pursuit of maximum profit by firms as tantamount to a ‘trial and error’ method rather than as a manifestation of an optimizing rationality on the part of entrepreneurs. But, again marking himself out from approaches alternative to those based on marginal analysis, Kahn never accepted explanations of the price-formation mechanism of the descriptive type or based on a hypothesis of non-‘rational’ behaviour.

From the theoretical point of view, the leading advocate for the abandonment of the theory of perfect competition and of the hypothesis of profit maximization in favour of price-formation theory based on (fairly steady) mark-ups on (fairly steady) prime costs was Michał Kalecki, who formulated his theory subsequent to a study of the prices, proceeds, and costs of a number of British industries (Marcuzzo 2011: 189). Unlike Joan Robinson, who subsequently adopted it, Kahn remained averse to mark-up pricing, maintaining to the very end,

I think that the concept of the horizontal short period supply is exaggerated. I do not hold altogether with the ideas of Wynne Godley about the fixed write up margin. What determines the size of this margin? And of course it varies between firms in accordance with their differences in prime costs (Kahn quoted in Marris 1991: 184).

Similarly, and unlike Robinson, Kahn was never convinced by Sraffa’s long-period production prices, prompting Robinson to state bluntly: ‘Cannot we agree on Piero’s prices for the long run and on Keynes’s prices for the short run and leave it at that?’ (Robinson to Kahn, 19 May 1961, RFK 13/90/6/199–200).

4 Reforming International Economic Institutions

Kahn was always concerned about the capacities of the existing international institutions to perform their task of generating virtuous behaviours and opinions in the markets. The role of these institutions should be to create the right conditions for decisions to be arrived at which are ‘virtuous’ from the point of view of the community, given that the individual pursuit of maximum profit or utility is not always able to generate the optimal outcome for society,

nor indeed, over the long period, for the individual. To this end, Kahn was involved in designing and establishing new institutions which could foster international cooperation and trade.

In the 1950s, Kahn started to write a monograph for the FAO on the instruments required to curb commodity price volatility. In this report, Kahn analysed in great detail the causes of the price fluctuations of raw materials, which were damaging both for producers, who could never rely on a constant source of income or adequate price signals for their investments, and for consumers, who were subject to sudden price variations. Since both demand and supply of commodities are generally inelastic or adjust only slowly to the conditions of the market, small variations in quantities imply large variations in prices. The causes are therefore structural and policies must be devised to mitigate their impact without resorting to restrictive measures. Kahn advocated the establishment of an international buffer stock agency for each commodity. By buying or selling the commodity on the market, the agency could stop or at least alleviate otherwise excessive increases or decreases in prices. The agency was supposed to be managed by a supranational board, made up of experts chosen not as national representatives but for their knowledge and skills. Under their guidance, the price determination of raw materials, unlike under a quota system, would be ‘not a matter of bargaining strength, but of judgement based on scientific enquiry and expert experience’ (RFK/2/12.3).

Although Kahn explicitly built on Keynes’s proposal for a buffer stock scheme (Keynes 1938),⁶ his approach to the use of buffer stocks was entirely original, differing also from other schemes which were being put forward in those years, according to which the managers of the buffer stock were obliged to intervene whenever the price of the commodity hit given limits. Indeed, Kahn’s analysis and experience of the workings of commodity markets persuaded him that the managers of buffer stocks could only be successful if they were endowed with much greater freedom of action than Keynes had envisaged, without following any predefined rule. A buffer stock with limited resources could exploit speculation to its own advantage only if speculators were left permanently in doubt about the next decisions of the buffer stock managers. Being subject to no automatic rule for intervention, these managers should aim at increasing the uncertainty under which speculators acted and stabilize prices by influencing market expectations and exploiting bearish or

⁶In 1938, Keynes put forward the first buffer stock scheme to curb the volatility of commodity prices, which he refined over and over in the early 1940s (see Fantacci et al. 2012) as part of his more general endeavour to help stabilize the international monetary system.

bullish sentiments (the similarity with the arguments developed in Kahn's liquidity preference article is undeniable). In other words, the managers of buffer stocks should become speculators among other speculators, not for their own profit, but in the general interest. Kahn's monograph, never completed nor published, for reasons that have yet to be clarified, is a good example of his approach to economics, where mastery of Marshallian partial equilibrium analysis, Keynesian interest in rational decision making under uncertainty, and direct observation of markets are combined into an original analysis with relevant policy implications (Rosselli 2012).

Kahn addressed the institutional aspects of the international payments system extensively years later, working from 1965 to 1969 as a member of the UNCTAD team of experts. He took a hand in the drafting of four reports on the reform of the monetary system to cope with the economic growth needs of both the developing and the developed countries. He contributed to these issues with important papers, reconstructing the design and implementation of the new international monetary system that came into being in the aftermath of the Second World War. His essays 'International Regulation of Trade and Exchanges' (Kahn 1952b), 'The International Monetary System' (1973), 'Remarks' (Kahn 1974), and 'Historical Origins of the International Monetary Fund' (Kahn 1976d) offer examples of his rare ability to combine detailed knowledge of institutional arrangements and accuracy of historical reconstruction with concern for the policy implications of institutional choices.

5 Kahn as a Cambridge Economist

In an earlier work we characterized the Cambridge economists in the interwar years as a group whose

identity stemmed from motivations, values, life-styles and work-styles, leaving room for reciprocal respect, overriding many contrasts, and keeping the sense of belonging alive; ...the points of theoretical division, precisely because they generated discussion, did not break the group up but served to form a connective tissue; ...precisely because there was no common corpus of accepted ideas to defend, the characteristic feature seems rather to have been elitism—a system of cooption based on characteristics that were neither ideology nor exactly academic performance or success, but rather the features of a moral and intellectual aristocracy (Marcuzzo et al. 2008: 583).

Kahn fits this description perfectly and, although not a prolific writer,⁷ he had an extraordinary influence. This can be seen in the two most important books produced by Cambridge economists in the 1930s, Keynes's *General Theory* and Robinson's *Economics of Imperfect Competition*.

Kahn contributed more significantly than anyone else in the circle around Keynes to the Keynesian revolution. The relations between Keynes and Kahn were strong, continuous, and fertile, with an apparently paradoxical inversion of roles: it was the pupil who intervened to correct, tidy up, and sound out the master's rationale. There are aspects independently worked on by Kahn that Keynes subsequently incorporated, adapting them to his aims and *forma mentis*, which eventually became part of *The General Theory*, readjusting the framework upon which his *Treatise on Money* had rested. The major points where Kahn's influence can be better appreciated are short-period analysis and the application of aggregate demand and supply to determine price levels and equilibrium quantities. Within this framework, the multiplier principle and the supply schedule for output as a whole were tools that proved particularly productive in building the new theory.

The other intellectual and indeed strong personal relationship which was pivotal in Kahn's life was with Robinson. There is hardly a theoretical work by Kahn in which mention is not made, with approval, of the ideas of Robinson, showing how the work of one would be carefully read and commented upon by the other, even when their main fields of research and approaches diverged, as they soon did after their joint work on imperfect competition and on Pigouvian themes in the early 1930s. Their collaboration started during the writing of Robinson's *Economics of Imperfect Competition*. The collaboration probably proved congenial from the outset because they both shared a method of analysis based on meticulous classification of all the possible combinations of two or more elements, the reasoning often going: if X can be A or B and Y can be C or D, let us examine all four combinations AC, AD, BC, and BD. Behind this method of reasoning there were, however, probably two different needs. Robinson had no other analytic tools at her disposal. She knew absolutely nothing of calculus, and headway could only be made in a rigorous manner by breaking the problem down into simple cases which might be examined one by one. Kahn had been educated as a physicist and this method satisfied his need for order, his love for taxonomies, as well as paving the way to pinpointing real cases corresponding to the theoretical cases examined. However, since Kahn had mathematical demonstration at his command, we cannot tell whether his choice in suggesting to Robinson that she use geometry instead of calculus for the proofs in the book was dictated by a personal preference for the graphically representable or the decision to use only such tools (high school geometry,

⁷ Kahn published only one volume of collected essays, Kahn (1972).

trigonometry) as were accessible to Robinson and the average economist (see Rosselli 2005). After the Second World War, their interests somewhat diverged since Kahn did not take part in the capital controversy of the 1960s and 1970s and was more involved in active economic policy, but many intellectual ties continued to unite them: the fight for Keynesian ideas and against laissez-faire policies, both in the public arena and in Cambridge academic life.

Kahn's relations with both Keynes and Robinson, although obviously different in kind, epitomize the kind of intellectual collaboration that was typical of Cambridge: a sharing of time and space, which 'also entailed a sharing of knowledge and the habit of exchanging rather than possessing ideas' (Marcuzzo and Rosselli 2005: 10).

Finally, mention should be made of the influence on Kahn of Gerald Shove, since it is often neglected. Shove, who became Lecturer in Economics in 1923 and only in 1926 was made Fellow of King's College, published very little; he worked for years on a book which was never finished and all his papers were destroyed after his death in accordance with his will. However, we know that he was an excellent teacher and all his students preserved fond memories of and admiration for him. Kahn was no exception. He learnt his Marshallian economics from Shove, attending his lectures, and in his Fellowship dissertation he acknowledges his debt to Shove. Respect and affection lasted over the years; it was Kahn who wrote Shove's obituary for the King's College *Annual Report* in 1947 and, in his article on Shove for the Palgrave Dictionary, he called him a 'dauntless thinker'. The apparatus of Marshallian partial equilibrium, on which Kahn's reasoning rested all his life, was part of Shove's enduring legacy to him.

6 Conclusion

In the House of Lords, where Kahn made his maiden speech on 28 July 1966, he intervened on various economic matters more than 30 times over 10 years, initially under Wilson's first Labour government, then under Heath's Tory government, and finally under Wilson–Callaghan's Labour governments, making his last speech on 26 January 1977. This was the period in Britain and elsewhere of two-digit inflation following the oil shock and the wage disputes which hit most industrialized countries, accompanied by the rise of monetarism and the attack on Keynesian theories and the expansionary Keynesian policies of the 1950s and 1960s.

If we ask ourselves what Kahn's legacy is, a good answer may perhaps be found in his parliamentary speeches of the mid-1970s, in which he argued

out his strong beliefs on the specific issues of unemployment and inflation and the general approach to economic policies. Contrary to Friedman's dictum, Kahn objected to the doctrine that inflation is always and everywhere a monetary phenomenon; it is rather the outcome of the growth of money wages and increases in the price of imported commodities. It follows that the proposed monetarist 'cure' of reducing the growth of the money supply misses the point, since it has neither a direct nor an indirect impact on prices through an increase in unemployment following a monetary restriction (unless unemployment reaches politically unacceptable levels). Kahn was highly critical of the expectations-augmented Phillips curve, arguing that there was no evidence of an inverse relationship between the rate of growth of money wages and increases in the rate of unemployment (House of Lords speech, 30 July 1974). Against the money illusion behind the demand for higher money wages, he denounced the pernicious leapfrog effect which would follow, keeping real wages lagging behind money wages. Thus, the role of government is to carry out an incomes policy, reassuring the trade unions that employers would not take advantage of wage moderation to increase prices in order to reap higher profit margins, while at the same time guaranteeing employers that the increase in productivity brought about by their investments will not be wiped out by demands for wage rises and labour disputes.

The propelling fuel for the economy, according to Kahn, is not consumption deriving from high wages, but rather investment which is necessary to boost productivity and thus competitiveness. For him, restrictive monetary policies discouraged both. Profits, or rather the expectation of profits, in favourable economic conditions are the recipe for growth. It follows that expansionary public expenditure is not always the solution, as simple-minded Keynesianism would have it. True to the spirit of his mentor—Kahn prided himself on being remembered as 'a disciple of Keynes' in the last interview he gave (Kahn 1988)—he favoured a middle way between action by the government and market incentives: the former to check unemployment, the latter to generate investment.

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32

James Meade (1907–1995)

Susan Howson

1 Introduction

In January 1933, Maynard Keynes judged James Meade to be ‘one of the most promising of the very young but interesting school of Oxford economists’. As he explained, Meade was ‘a Fellow of Hertford [College] of some two years’ standing, and spent a year at Cambridge, where I saw a good deal of him, after taking a first in Modern Greats. I considered him to be of excellent promise then’ (Keynes to Daniel Macmillan, 10 January 1933, Keynes Papers CO/5/80). Meade had been an undergraduate at Oriel College and after graduating with a First in Philosophy, Politics and Economics (PPE) in 1930 had been elected to a Tutorial Fellowship in economics at Hertford on the understanding that he go away for his first year to learn more economics before teaching it. He went to Trinity College, Cambridge, invited by Dennis Robertson, whom he already knew through his family in Bath. He had invited Robertson to Oxford to speak to an undergraduate society in June 1929. This year was ‘the intellectually most exciting year of my life’ (Meade 1988a: 1). He became a member of the Cambridge Circus, the group of young economists including Richard Kahn, Austin and Joan Robinson, and Piero Sraffa, who met to analyse Keynes’s *Treatise on Money* once it had been published on 31 October 1930. He was thus one of the very first Keynesians.

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Meade returned to Cambridge 26 years later as the Professor of Political Economy in succession to Robertson. In between he was lecturing in Oxford until December 1937 when his College gave him leave of absence to edit four issues of the annual *World Economic Survey* of the Economic Intelligence Service of the League of Nations. He stayed in Geneva for only two and a half years (and edited only two Surveys) because of the Second World War, managing with his wife and young family to drive across falling France in May 1940 in order to take up a position with the British wartime government. In what became the Economic Section of the War Cabinet Offices he was to play a major role in implementing Keynesian policies, both for financing the war and for constructing a better post-war world.

Meade remained in government service until 1947, Deputy Director of the Economic Section from late 1941 until the end of 1945 and Director until September 1947. Before he left government service he returned to Geneva as a British delegate to the Preparatory Commission for an international conference on trade and employment. He returned to academic life at the London School of Economics (LSE) as Professor of Commerce with special reference to International Trade and there accomplished the work in international economics (both macro and trade) for which he was awarded a Nobel Memorial Prize in Economic Sciences in 1977.

In his long career Meade wrote on many subjects: price theory and welfare economics, national income accounting, economic growth, economic development and population problems, nationalisation and planning, income distribution and equality, public finance and some aspects of industrial organisation, as well as macroeconomics and international economics. This chapter concentrates on the work in macroeconomics which was influenced by his Cambridge colleagues or influenced them. It falls into three parts, covering his postgraduate year in Cambridge and his work in Oxford following that year, the years in Geneva and London, and his Cambridge years from 1957.

2 The Cambridge Circus

Meade arrived in Cambridge in October 1930 to spend a year studying monetary economics under Dennis Robertson's supervision. Robertson told him that amongst the younger economists he should get to know was Richard

Kahn of King's College, which Meade promptly did. Kahn had already written his first paper on the employment multiplier, 'The Relation between Primary and Secondary Employment', a note circulated to the Committee of Economists of the Labour government's Economic Advisory Council (Keynes was chair of the committee, Kahn a secretary along with the civil servant Francis Hemming).¹ Kahn considered the total increase of employment that could result from expenditure on public works and showed that the amount of additional, 'secondary' employment generated could be calculated as a definite multiple of the initial increase, 'primary employment', the size of the multiplier depending on the amount of additional income that each newly employed person spends on consumption of home-produced goods. As Kahn put it in his article in the *Economic Journal* of June 1931 (Kahn 1931) if k is the number of men employed as a result of the increased expenditure generated by one man placed in primary employment, the number who receive secondary employment is $k + k^2 + k^3 + \dots = k/(1 - k)$, which is the ratio of secondary employment to primary employment, that is, the multiplier.

In addition to his work with Robertson, Meade in Cambridge was trying to learn mathematics. As he recalled: 'I decided as an exercise in the Higher Mathematics to repeat [Kahn's] exercise summing not what income recipients decided to spend but what they decided to save'; he found that the increments of saving added up to the initial increase in investment (in this case the expenditure on public works), thus answering Kahn's original question 'Where does the finance for public works come from?' (Meade 1993a: 664–665). Kahn christened the result 'Mr Meade's Relation' when he used it in his article, where he wrote it as

$$\begin{aligned} \text{cost of investment} &= \text{saving on dole} + \text{increase in excess of imports over exports} + \\ &\text{increase in unspent profits} - \text{diminution in rate of saving due to rise in prices} \end{aligned}$$

or, in modern terminology, the increase in investment equals the reduction in government expenditure on unemployment benefit plus the increase in the current account deficit plus the increase in saving, since, as Kahn acknowledged in 1932, the last two items are both part of saving (Kahn 1932: 492–495; see also Shackle 1951). Kahn's multiplier and Meade's relation are two of the essential ingredients of the theoretical system of *The General Theory of Employment, Interest and Money*.

¹ See Howson and Winch (1977: 49). There is a copy of Kahn's paper in the Meade Papers.

As soon as *A Treatise on Money* had been published, Kahn and the other young Cambridge economists plus Meade had begun to discuss it critically, at first informally amongst themselves in Kahn's rooms in King's, their discussions continuing in the Lent Term 1931, then as a 'seminar' held in Trinity in the Easter Term which a few carefully selected undergraduates were allowed to attend. Austin Robinson remembered Meade 'as more active than any of us, not only in the Keynesian arguments but also in those around the theory of value' (Austin Robinson 1977: 33–34). Joan Robinson remembered the 'main speakers' in the seminar as 'Kahn, James Meade...Sraffa...Austin Robinson and myself' (Joan Robinson 1978: xii). Kahn and the others quickly realised that the equations of the *Treatise* really applied only when output was constant, Austin Robinson noticing (as he and Joan both remembered) that the analogy of the 'widow's cruse' (Keynes (1930 [1971]): 125) implicitly assumed a fixed output of consumption goods. Kahn reported to Keynes on the deliberations of his friends; Meade reported some of their findings to Robertson, who used them in his criticisms of the *Treatise* in the *Economic Journal* in September 1931 (Robertson 1931: 408–409; see also Laidler 1999: 133).

Kahn's multiplier analysis was a first step in incorporating changes in output in response to changes in demand; his article also introduced a supply curve of consumption goods. David Vines relates what Meade once told him:

I said the following to the other members of the Circus. 'Haven't any of you read Marshall's *Principles of Economics*? In that book, in the short run, the economy lies on a short-run, upward-sloping, supply curve. But that curve adds an extra equation to the model. This means that—in comparison with the model in the *Treatise*—we can make *both* prices *and* output endogenous at the same time (Meade quoted in Vines 2008: 486; italics in original).

Meade attributed the supply curve of consumption goods to Kahn. He wrote to George Shackle in 1950:

I can remember making only one other contribution in the discussion ... Richard Kahn introduced the idea of the supply curve of consumption goods in general. I then tried to introduce the idea of a supply curve of capital goods in general. This appeared in the *General Theory*, where the rise in the price of capital goods as their output increases is one of the things which brings down the marginal efficiency of capital and thus determines the effect of, e.g., a change in the rate of interest on the value of investment. I feel that this is the only contribution which I made and which may not be apparent from the record.

Kahn commented: 'I do not at all quarrel with what he says except that he is of course unduly modest about the part that he played' (Meade to Shackle,

10 June 1950 and Kahn to Shackle, 15 June 1950, Meade Papers 4/1, LSE Archives).

Meade's first book, *The Rate of Interest in a Progressive State*, written in the year after he returned to Oxford, clearly shows the influence of his year in Cambridge. As Joan Robinson wrote in her review (Joan Robinson 1934: 282), 'Mr Meade is a follower of Mr Keynes, but he has an original contribution to make ... Breaking away from the vain circle of controversy and misunderstanding, Mr. Meade has struck out on a path of his own'. Meade acknowledged the influence of both Keynes and Robertson and the help of Kahn and Maurice Allen of Balliol College, Oxford, 'with both of whom I have spent many hours arguing about these problems' (Meade 1933: vii–viii). He also mentioned Friedrich Hayek's *Prices and Production*: Meade was in Cambridge when Hayek gave an address to the Marshall Society on 22 January 1931 a few days before his lectures on *Prices and Production* at LSE.² The effect of Hayek was on the questions Meade was asking, not on his answers; he told Keynes on 11 January 1933 that 'I fear the book is very incomplete and does not get very far, but I have tried to incorporate some of the method of thought of Hayek, without introducing what I consider to be his obscurities' (Keynes Papers CO/5/82, King's College, Cambridge).

Meade opened his book with the questions 'What can be meant by an equilibrium rate of interest? What changes in this equilibrium rate are to be expected in a progressive society? and In what conditions will the market rate of interest move so as to conform with the equilibrium rate?' By a progressive society he meant one in which output per head is increasing; for simplicity he assumed a closed economy with a centralised banking system, perfect competition, and a constant population. The last assumption was to make it possible to define a 'neutral money system': 'one which simply interprets the decisions of individuals, of companies or of the government without, by its own action or inaction, making the effects of such decisions different from what they would have been in a non-monetary economy' (Meade 1933: 11); with a constant population such a system would keep final incomes constant—an idea he attributed to Robertson (*ibid.*: 24, fn. 2). Joan Robinson, with justice, saw this concept of neutral money as 'monetary mysticism' but she noted that most of the argument of the book did not depend on it. She particularly commended the chapters on the relations between short-term and long-term interest rates and on the effects of interest rate changes on investment and Meade's notion of a supply curve of capital goods.

Other friends of Meade's also disliked his use of a concept of neutral money, including the Hayekian Evan Durbin, with whom Meade was collaborating

² Hayek's Cambridge lecture has recently been published (Hayek 2012).

closely in work on monetary policy for the New Fabian Research Bureau (NFRB) which had been created in March 1931 (Durbin had read PPE at New College, Oxford, 1927–1929, and was now a Lecturer at LSE). He swiftly gave up on it, admitting as much to Joan Robinson in June 1934 (Durbin to Meade, 15 November 1933, Meade Papers 2/3; Meade to Robinson, 3 June 1934, Meade Papers 10/48). Keynes meanwhile commended the book for publication to Macmillan without having read it. But Keynes had read the memorandum on ‘The Market Rate of Interest’ that Meade wrote for the NFRB in the winter of 1931–1932 for Kahn had shown it to him in the spring.³ This memorandum contained the ideas that were developed into the central chapters that Joan Robinson commended; the book was completed by the Michaelmas Term 1932 when Meade lectured on it to undergraduates at Oxford. Both the book and the lectures include sections criticising Hayek’s *Prices and Production* and Keynes’s *Treatise on Money*.⁴

Meade kept in touch with his fellow members of the Cambridge Circus, especially Richard Kahn and Joan Robinson. Kahn came to stay in Oxford in June 1932 and June 1934; Meade and his wife Margaret spent a weekend with the Robinsons in 1934; Richard and Joan visited the Meades in Geneva in 1938. Meade attended the inaugural meeting of the London and Cambridge research students’ seminar at Newport in August 1933, and contributed two of the series of ‘Notes on the Elasticity of Substitution’ (by Paul Sweezy, Abba Lerner, Kahn and John Hicks, and others) in the first two issues of the *Review of Economic Studies*.⁵ He may have given a paper to a meeting of Keynes’s Political Economy Club on 22 January 1934. Meade invited Joan to talk to the Economic Theory group of the Political and Economic Society at Oxford in Michaelmas 1933 and he invited Keynes, who came in February 1935.⁶ Joan read and commented on drafts of Meade’s next book *An Introduction*

³Durbin to Meade, 2 February 1932, Meade Papers 2/3; Kahn to Meade, 18 April 1932, Meade Papers 2/4. While Durbin’s and Kahn’s lengthy comments on Meade’s memorandum have survived, the memorandum itself has not.

⁴‘F. Taylor Ostrander’s Notes from Lectures by James E. Meade, Hertford College, Oxford University, 1932–33, Concluded’. In M. Johnson and W.J. Samuels (ed.) (2009) *Research in the History of Economic Thought and Methodology*, volume 27-C: 3–34. Warren Samuels mistakenly entitled the lectures ‘Second Term: Linking Monetary Theory with the Pure Theory of Value’. The lecture lists published in the *Oxford University Gazette* give the correct title and the terms (Michaelmas 1933 and 1934 as well as Michaelmas 1932) in which the lectures were given.

⁵Kahn to Meade, 14 June 1932, Meade Papers 2/5 and Meade to Joan Robinson, 3 June 1934, Meade Papers 10/48; Keynes (1973a: 338, fn. 2), Kahn (1984: 183), and Meade (1988b: 1–8).

⁶Kahn to Meade, 15 December 1933, Meade Papers 2/4; Meade to Robinson, 25 June 1933, Meade Papers 10/48; Meade to Keynes, 1 January 1935, Keynes Papers PS/6. Meade explained to Joan Robinson that ‘The Political & Economic Society corresponds to your Marshall Society and the Economic Theory group is perhaps the nearest thing we have to your Keynes Club’. If Meade gave a paper to the Keynes Club, it is unlikely to have been his ‘The Amount of Money and the

to *Economic Analysis and Policy*, which was published soon after *The General Theory* (Meade 1936: vi–vii). At the same time, Meade remained close friends with Robertson, meeting and corresponding regularly, especially over trade cycle theory (see, for instance, the exchange in Meade Papers 4/47). The reading list for his lecture course ‘Monetary Theory of Trade Fluctuations’ included Keynes’s *Treatise* volume 1, Robertson’s *Banking Policy and the Price Level*, Hayek’s *Prices and Production*, Marshall’s *Principles* Book V, Kahn’s article on the multiplier, and one by Roy Harrod entitled ‘Notes on Supply’ (Harrod 1930).

Meade’s published work in the 1930s was thoroughly Keynesian. In his *The Rate of Interest* he argued that to counter the inevitable discontinuities in investment and expenditure it would be necessary for the state to undertake public investment schemes (Meade 1933: 103–104); he expanded on this in a paper published as an NFRB pamphlet in January 1933, using his ‘relation’ to counter the objection that extra borrowing by the state for public works will take funds that would otherwise be used by private industry (Meade 1988a: 10–11). He also advocated Keynesian employment policies in his work for the Labour Party (see, for instance, his ‘Outline of Economic Policy for a Socialist Government’ in Meade 1988a: 33–78). Since expansionary policy undertaken by one country could lead to balance-of-payments problems for a country with a fixed exchange rate, and exchange rate depreciation could cause difficulties (of domestic inflation or foreign debt repayment) for a country with a floating exchange rate, he consistently argued for international cooperation as well as exchange rate flexibility.

Meade, who lectured on ‘Introduction to Economic Theory’ in the Trinity Terms of 1934, 1935, 1936, and 1937 and ‘Introduction to Theory of Money’ in Michaelmas 1935 and 1936, wrote the first Keynesian textbook in economics, *An Introduction to Economic Analysis and Policy*, completed in April 1936. He wanted to write a book which would ‘expound the whole corpus of Economic Theory without taking any knowledge of technical terms for granted and with full use of the more recent developments’ and include every piece of theory with an application to current economic problems (Meade 1936: v). In addition to the Keynesian developments in macroeconomics, which he expounded in Part I, he particularly wanted to cover the work of Joan Robinson and others on imperfect competition (Part II); he was also very interested in the distribution of income (Part III) and in population (Part IV); Part V dealt with international problems, including the balance of pay-

Banking System’ which Keynes had read and accepted for the *Economic Journal* in September 1933 (Keynes to Meade, 21 September 1933, Meade Papers 2/4; Meade 1988a: 26–32).

ments, exchange rates, trade, migration and capital movements, international economic cooperation, and the economic causes of war. Joan Robinson read the full manuscript except for Part V.

The book was resolutely non-technical, with no equations or even diagrams. Meade provided a more technical account of Keynesian theory in his 'A Simplified Model of Mr Keynes' System' for the sixth European meeting of the Econometric Society in Oxford on 25–29 September 1936. The first session of the conference was a symposium on 'Mr Keynes' System' to which Roy Harrod and John Hicks also presented papers. Meade's model was a static short-period equilibrium model of eight equations, which determined the outputs and prices of capital goods and consumption goods and the numbers employed in each industry, total money income and total money profit, given the stock of money, the money wage rate, and the proportion of income saved. As in *The Rate of Interest* he assumed perfect competition but this time he assumed the elasticity of supply of capital goods was the same as the elasticity of supply of consumption goods (see Phelps Brown 1937: 361–363). Hicks in his paper introduced the IS–LM diagram which became the favourite device for teaching purposes and which Joan Robinson excoriated as 'bastard Keynesianism'. The conference was very well attended but, as Luigi Pasinetti has commented, 'Very unwisely, neither Richard Kahn nor Joan Robinson participated...— a too-obvious mistake on their part' (Pasinetti 2007: 32, fn. 7).

Harrod's and Hicks's papers were published in *Econometrica*, Meade's in the *Review of Economic Studies* (Meade 1988a: 79–90). At the conference Ragnar Frisch had challenged Meade's claim for the stability of the solution of his model; as editor of *Econometrica* he said he would like to publish it if 'the definition and the fundamentals of the stability situation could be discussed in more exact terms' (Frisch to Meade, 8 October 1936, Meade Papers 2/3). Ursula Hicks asked for less drastic changes for the *Review of Economic Studies* (Hicks to Meade, 18 November 1936, Meade Papers 2/4). Earlier, Meade had sent his paper to Keynes who responded on a postcard on 14 September: 'It's excellent. I have no criticisms to suggest' (Meade Papers 2/4). Meade told Warren Young that Keynes had also told him that he could not publish the paper in the *Economic Journal* because he had already accepted another simplified account of *The General Theory* for the journal, which was anyway 'absolutely full of commentary' on the book (Keynes quoted in Young 1987: 37). Robertson also commented briefly on the paper when Meade presented it to a meeting of the Economic Club in Geneva in April 1937; when Meade 'said that The General Theory presented a "short-run stable equilibrium model" ... DHR commented, "That is what is wrong with it"' (Meade to Elizabeth Durbin, 11 August 1982, Meade Papers 10/21).

Meade wrote a third, short book in the summer of 1937: *Consumers' Credits and Unemployment* (Meade 1938a), in which, concerned that in a depression expansionary monetary policy and public works might not suffice to prevent unemployment, he put forward the idea of giving grants or allowances to consumers when aggregate demand threatened to fall off in order to maintain consumption expenditure. Keynes, reviewing it for the *Economic Journal* in March 1938, was critical: although 'there is a good idea behind this', he thought consumers' subsidies would be less effective in creating employment than an equivalent amount spent on increasing investment (see Keynes 1983: 439–444).

3 Geneva and London

In April 1937, Robertson introduced Meade to Alexander Loveday, Director of the Economic Intelligence Service of the League of Nations, just as Loveday was being frustrated in his search for an experienced and literate economist to succeed J.B. Condliffe as editor of the *World Economic Survey*. Three months later, Loveday wrote to Meade asking if he would be willing to be considered for the post, having decided (so Loveday later commented) that he would prefer 'a first-class mind and less experience [to] the obverse' (Loveday to Meade, 17 August and Loveday to N.F. Hall, 16 October 1937, Loveday Papers, Box P144, League of Nations). Meade began work in Geneva in January 1938; his first *Survey* appeared in September, his second a year later (1938b, 1939). Loveday told Keynes that he thought Meade 'has really made a very good job of it' (Loveday to Keynes, 31 August 1938, in Keynes 1973b: 291).

Richard Kahn reviewed Meade's first volume very favourably in the *Economic Journal* (Kahn 1939: 97):

The *Survey* is far more than a factual account of trends and policies. Mr. Meade believes that economic theory is capable of accounting for observed events and that policies may be judged by the standards of the theoretical economist. But he makes no parade of the theoretical apparatus which underlies his writing (and which accounts very often for the brilliancy of his treatment) ... Theory is none the worse for appearing under the guise (though that is not the correct word) of common sense.

Keynes described it as an 'authoritative study having international scope' when he used it in his article on 'Relative Movements of Real Wages and Output' in the same issue (Keynes 1939: 42).

The Second World War gave Meade the opportunity for close collaboration with Keynes, who was working in the Treasury from August 1940 until his death in April 1946. It was Austin Robinson who summoned Meade home. Austin had joined the government's Central Economic Information Service (CEIS) in December 1939. On 20 October he had heard Keynes put forward his proposals on the financing of war to the Marshall Society in Cambridge and in early 1940 he managed to persuade senior civil servants of the need for reliable estimates of national income and expenditure. He asked Meade to come and prepare them and later asked Richard Stone (a recent Cambridge graduate working in the Ministry of Economic Warfare) to assist him. Meade started work in June and by the time Stone arrived at the end of August had already drawn up the framework for the estimates in the form of a complicated system of balancing tables (Meade 1988a: 106–117). Early in the new year, Keynes submitted their first draft to the Treasury's Budget Committee and persuaded it to use the estimates in formulating the budget for 1941–1942. The first official national income and expenditure estimates were published as a White Paper along with the first Keynesian budget in April 1941 (see also Meade and Stone 1944).

Meade joined the Economic Section of the War Cabinet Offices when the CEIS was split into the Economic Section and the Central Statistical Office (CSO) in January 1941. Stone went to the CSO; at the end of the war he returned to Cambridge to head the new Department of Applied Economics (DAE). (Austin Robinson, meanwhile, moved to the Ministry of Aircraft Production in February 1942.) The Section soon began to consider problems of post-war reconstruction: Meade wrote the first of a long series of memoranda on the subject in February 1941. His proposals for post-war employment policy, which were widely circulated within Whitehall in July 1941 (ibid.: 171–183), included measures for influencing consumption expenditure counter-cyclically along the lines of his *Consumers' Credits and Unemployment*. The Beveridge Committee on Social Insurance and Allied Services in 1942 provided Meade with an opportunity to put forward a detailed scheme for variations in the rate of social security contributions as a means of stabilising the demand for labour (ibid.: 184–192). He tried it first on Keynes who was now 'converted' despite his usual preference for controlling capital expenditure, before he submitted it to the Committee. Although Meade's scheme did not appear in the Beveridge Report of December 1942, it was included in an appendix to the White Paper on Employment Policy in May 1944.

Meade wrote the first draft of what led eventually to the White Paper in March 1943 for a ministerial Reconstruction Priorities Committee established in January 1943. Within the Economic Section it went through at least four drafts, with Lionel Robbins, the Director of the Section, redrafting it for

the ministerial committee since ‘after all that he has had to put up with [by way of criticism] from us, it would have been a last indignity to ask J.E.M. to do this’ (ibid.: 199). (The last of Meade’s three drafts is in ibid.: 199–232.) It met strong adverse reaction in the Treasury from the permanent secretary, Sir Richard Hopkins, down. Keynes, less pessimistic than Meade about the possibility of post-war depression, was not entirely sympathetic either, but he endeavoured to educate his Treasury colleagues of the necessity of a macroeconomic employment policy. When a small steering committee including Hopkins and Robbins managed to produce an agreed report at the end of 1943, Keynes commented it was ‘indeed an outstanding State Paper which...represents a revolution in official opinion’ (Keynes 1980: 364).

Meade did not take part (neither did Keynes) in the subsequent drafting of the White Paper, which involved almost all every other economist working in Whitehall in 1944 (including Robertson who was in the Treasury). After the White Paper appeared Meade persuaded Hopkins to conduct an enquiry into post-war monetary and debt management policy. The National Debt Enquiry took place in the early months of 1945. Meade found himself defending interest rate flexibility to permit the counter-cyclical use of monetary policy after the war, against Keynes, who favoured low and stable long-term interest rates to encourage investment and keep down the cost of the national debt (Meade 1990a: 48–49, 55–66). Keynes won the day.

Meade was particularly concerned to create an improved international economic order after the war. Early in the war, before he left Geneva, Meade had written a short book, *The Economic Basis of a Durable Peace* (Meade 1940), in which, believing that a satisfactory peace settlement would require the existence of an international organisation, he considered the economic role that such an organisation would need to play in the post-war world. It should be able to include countries with competitive free markets and with planned economies. An international currency system like the gold standard would be ‘appropriate for a group of liberal economies which practise an internal policy of *laissez-faire*’ (ibid.: 56) but was not appropriate for a mixture of liberal and planned economies: exchange rate arrangements had to be more flexible. He favoured an international bank with the power to issue an international currency against which individual countries would peg their exchange rates, but these pegs had to be adjustable when a country pursuing a high employment policy was persistently losing reserves. Countries adhering to these international arrangements would have to commit themselves to multilateral free trade and abandon bilateral exchange control and payments arrangements. Hence, when Keynes produced his ‘clearing union’ plan for the post-war international currency system, which he sent to Meade before the rest of the Economic Section in October 1941, Meade welcomed it enthusiastically; he also persuaded Keynes to agree that countries

in the union should apply controls only to international transactions on capital account not to those on current account. A few months later he put forward a proposal for a complementary ‘international commercial union’ to restore multilateral trade and remove trade restrictions after the war (Meade 1988c: 27–35). The Keynes and the Meade plans formed the basis of the British contributions to the wartime Anglo-American discussions on the post-war international economic order. Meade joined Keynes and Robbins on the mission to conduct talks with the Americans in Washington in September–October 1943.

Further discussions led to the Anglo-American Proposals for consideration by an International Conference on Trade and Employment published in December 1945. Meade served as a British representative on the Preparatory Commission for the conference, which met in London in 1946 and in Geneva in 1947 to produce a draft charter for an international trade organisation (ITO). The ITO Charter adopted at the Havana Conference in March 1948 was not ratified but its main principles were incorporated in the General Agreement on Tariffs and Trade (GATT) negotiated in Geneva in 1947.

In November 1944, Meade agreed to succeed Robbins as Director of the Economic Section at the end of the war; he officially took over in January 1946. He had high hopes of what government economists could achieve on both the domestic and international economic policy fronts; he also thought he might be able to pursue his academic ambitions by writing a new edition of his *Economic Analysis and Policy* (Meade 1990a: 1). But the Chancellor of the Exchequer of the first majority Labour government, Hugh Dalton, was not inclined to heed Meade’s advice. Meade’s macroeconomic conception of ‘economic planning’—essentially the use of financial policy to influence aggregate demand—was adopted by Dalton’s successor Stafford Cripps in November 1947, by which time Meade had resigned in frustration and ill health. As two of his colleagues noted, Meade ‘was advising the wrong minister at the wrong time ... Meade, more than any man—more than Keynes—was the prophet of demand management when the world was not yet ready for demand management’ (Cairncross and Watts 1989: 130; see also Meade 1948).

By the time Meade took up a chair at LSE in September 1947, he had made his research plans: the production of a multivolume ‘Treatise on Economic Policy’ which would cover much the same ground as his pre-war textbook but reflect his experience in government. He envisaged six volumes, one an introductory volume and five to correspond to the five parts of his *Economic Analysis and Policy*; he thought he would probably start by writing Part I or Part V (‘Scheme for a “Treatise on Economic Policy”’, June 1947, Meade Papers 17/9). But ‘as I was appointed at the LSE to teach international eco-

nomics, I started on *The Theory of International Economic Policy*. It grew into my two books *The Balance of Payments* [1951] and *Trade and Welfare* [1955a] ... These books took up practically the whole of my ten years at the LSE' (Meade 1988a: 3).

Meade did not follow the conventional order of writing on trade before finance: as Jagdish Bhagwati commented, 'Good microeconomics presupposes good macroeconomics. It was not for nothing that Nobel Laureate James Meade's classic work on the theory of international economic policy had two volumes, one on balance-of-payments management and a second (in the proper sequence) on the theory of commercial policy' (Bhagwati 1988: 129). For the first volume Meade constructed a Keynesian general-equilibrium comparative static model for an economy open to trade and capital flows, synthesising Keynes's theory and Hicksian general equilibrium and extending it in several directions so as to be able to use it to analyse the effects of policy instruments and other variables on internal and external balance. Since his 'method of work...[was] to make a simple mathematical model of most of the problems before writing about them', the book and its successor came with separate mathematical supplements containing the models, which also appeared in journal articles (Meade 1951b, 1952, 1955a, 1988c: 95–147). The first systematic exploration of the relationship between domestic and international equilibrium, Meade's model has become 'part of the baggage of every economist' (Corden and Atkinson 1979: 529), the most important single influence behind the development of open economy macroeconomics in the next four decades.

The second volume was equally path-breaking. It made at least three major and lasting contributions to economics: a fundamental reformulation of the theory of economic welfare to make it both operational and more widely applicable; the use of this new theory to analyse controls on factor movements as well as trade controls; and the extension of the analysis from two-country models to a many-country world, including its application to the theory of customs unions (see Meade 1953, 1955b). Meade originally drafted much of the book on the basis of the 'new welfare economics' of the late 1930s, but he rewrote it to develop the method of his former wartime colleague Marcus Fleming's 'On Making the Best of Balance of Payments Restrictions on Imports' (Fleming 1951):

It was a brilliant feat of imagination...to realize...[Fleming's method] was capable of large-scale generalization into a powerful tool for welfare analysis of practical policy problems, and an act of great intellectual honesty and courage for him to scrap his existing draft and rework the whole problem on the new approach (Johnson 1978: 73).

Harry Johnson had notoriously criticised the first volume for its ‘taxonomic’ approach. Johnson also criticised, quite unfairly, Meade for his ‘British’ approach to economics, ‘the single scholar working away in his study’ (ibid.: 65) who then presents his major findings in books rather than articles or conference papers. But from his year with the Cambridge Circus to the end of his life Meade was the happiest and most productive with colleagues, especially contemporaries and younger economists, from whom he could learn and on whom he could try out new ideas.

4 Return to Cambridge

In 1949 Meade was offered a new, second Chair in economics in Cambridge. He would have become a Fellow of King’s had he accepted. But, although he went up to Cambridge to talk to both Kahn and Robertson about the offer, he declined, as he was ‘very happy’ at LSE (Meade to Robertson, 6 November 1949, Robertson Papers C18/86, Trinity College, Cambridge). (The Chair went to Austin Robinson.) Eight years later he was offered Robertson’s Chair, which was harder to turn down. Robertson (and Meade) hoped he could be offered a Fellowship at Trinity, but Robertson found out this was not possible. Meade accepted the Chair and a Professorial Fellowship at Christ’s College, of which the then vice-chancellor of Cambridge University, Brian Downs, was master (Robertson to Meade, 22 and 25 May; Downs to Meade, 18 and 25 May; Alan Prest to Meade, 28 May 1957, Meade Papers 19/7).⁷

Meade’s relations with his old friends of the Cambridge Circus were still cordial in 1949 and in 1957 but in the intervening years the Cambridge Faculty of Economics had become increasingly polarised, split between the ‘Keynesians’ and the rest of the Faculty. As Luigi Pasinetti has bluntly put it:

The behaviour (or misbehaviour) of each single member of the Keynesian group [which now included Nicholas Kaldor] gave the impression, in the 1950s and 1960s, that they believed themselves to be the masters of the place and of the theory ... [T]heir behaviour appeared rather arrogant. To those who disagreed with them, it even appeared doctrinaire (Pasinetti 2007: 38–39).⁸

⁷Trinity made Meade an Honorary Fellow 30 years later.

⁸Kaldor, who had moved to Cambridge with LSE in 1939 and moved back to London with LSE in 1945, but keeping his family home in Cambridge, had been offered a University Lectureship and a Fellowship at King’s in 1949, which he accepted.

As another of the younger Cambridge economists put it, Joan Robinson ‘had a bad habit of bullying anyone who did not accept her opinions’ (Bliss 2010: 633–634). Open hostility between Joan Robinson and Dennis Robertson went back a long way, to the late 1930s; Austin Robinson, then and later, especially as chairman of the Faculty Board from 1950 to 1962, usually managed to keep the peace.

Some of those who wrote to congratulate Meade on his appointment were worried. His former boss in Geneva ‘fear[ed] you will have a very difficult time in Cambridge which will require all your well known tact & patience ... It is delightful to think that Cambridge has been saved from the tyranny of one school of thought’. But Meade’s Cambridge friends had written to welcome him, Richard Kahn telling him that ‘It is going to be so pleasant to have you here as one of us ... I look forward, as does everybody else, to helping you to feel that once again Cambridge has something to offer you as you have to Cambridge’. Joan Robinson wrote more cryptically: ‘A lot of water has flowed since the days of the Circus and the weekend at Newport, but I hope the bridge is still standing’ (Meade Papers 19/7).

In his first year at Cambridge Meade lectured on international trade as he had done at LSE. The following year he began giving a lecture course on economic analysis for the preliminary (second year) examination, as Robertson had done from 1945–1946 until 1956–1957. But while Meade lectured on Tuesdays and Thursdays, Joan Robinson lectured on Mondays and Wednesdays to the same group of students on ‘Principles of Economics’. While Robertson had been lecturing on ‘Principles of Economics’, she lectured on ‘Money’, until 1954–1955 when her course was renamed ‘Theory of Employment’. The year before Meade’s arrival she started giving ‘Principles of Economics’ and continued to do so after he arrived, though the division was made clearer when the courses were renamed ‘Economic Analysis (Value and Distribution)’ and ‘Economic Analysis (Employment)’ in 1960–1961 and ‘Principles of Political Economy’ and ‘Employment, Prices and Growth’ thereafter. By then Meade had resumed work on his projected multivolume treatise on the principles of economics, beginning with *The Stationary Economy* which appeared in 1965, to be followed shortly by *The Growing Economy* in 1968.

When Meade arrived in Cambridge the theory of economic growth was becoming a fashionable topic, especially in Cambridge. ‘It was impossible’, Meade wrote, ‘to study economics in present-day Cambridge without being

affected by the ferment of ideas on this subject' (Meade 1961: vi).⁹ After preparing his Inaugural Lecture (on the control of inflation) Meade wrote a note on Kaldor's 1957 article which he sent to Kaldor and to Kahn on 14 January 1958, at the same time beginning to construct his own model. He was soon conducting a prolonged, initially amicable, argument with Joan Robinson over it. Joan, characteristically, became increasingly exasperated when Meade refused to give in to her objections, her core objection being to Meade's constructing a 'neoclassical' model of equilibrium growth with labour and capital continuously fully employed. At the same time, Robinson was commenting on Kaldor's model and on David Champernowne's article in the June 1958 *Economic Journal*, as was Meade, and they were commenting on each others' comments; from July 1958 Meade was also reading and commenting at length on the typescript of Robinson's *Exercises in Economic Analysis* (Joan Robinson 1960).¹⁰

Meade wrote a note on Champernowne's article which he submitted to the *Economic Journal* on 22 January 1959. Harrod accepted it on 26 January, 'subject to your not wishing to withdraw it' as a result of showing it to Champernowne. On 1 October, Meade wrote to Harrod again: 'I am afraid my economic-growth thing has become much too long for an article. If it is published at all, it will have to be a short book' (Royal Economic Society RES6/1/153, LSE Archives).

Meade completed his book, *A Neo-Classical Theory of Economic Growth*, in December 1959. It presents a model of equilibrium growth in an economy with two goods and three factors of production assuming perfect competition and constant returns to scale and a banking system which always sets the rate of interest so as to preserve a constant money price of the consumption good and hence full employment of all resources. In the early chapters he also assumed perfect substitutability between capital goods and consumption goods, thus making it a one-good model but the second, mathematical part is worked out in terms of two goods, thus making a considerable advance on the one-sector 'neoclassical' models of, for instance, Robert Solow and Trevor Swan.

Joan Robinson wrote a long and critical review in the *American Economic Review*. She did not go so far as to accuse Meade of 'bastard Keynesianism'

⁹ Cambridge contributions in the 1950s included David Champernowne (1958), Kahn (1959), Kaldor (1957), and Joan Robinson (1956).

¹⁰ The extensive correspondence, with memoranda, survives in the Meade Papers (10/4 and 10/6). With respect to Robinson's comments on Meade's model the earliest dated letter is one of 19 February 1958 from Meade to Robinson, the latest, from Robinson, 17 October 1959, when she was reading the typescript of the book. It is worth noting that in January 1958, Meade wrote the nomination for her election as a Fellow of the British Academy.

but in a survey of ‘Pre-Keynesian theory after Keynes’ in 1965 she took his book, along with the work of Paul Samuelson and Robert Solow in the other Cambridge, as exemplars of the genre (Joan Robinson 1965: 15–29, 56–69). Two years earlier, Solow, in Cambridge to give the Marshall Lectures, had, he told Amartya Sen (26 October 1964, Solow Papers, Duke University), ‘got a little annoyed...by the indiscriminate use of “Keynesian” as an adjective meaning “mine” and “neo-classical” to mean “yours.” To the extent that “neo-classical” describes the belief that a capitalistic economy tends automatically to full employment, I am no neo-classical and neither is James Meade’.

Meade had responded forthrightly to criticisms in the Preface to the second edition of his growth theory book:

This book is certainly not classical in the sense of being pre-Keynesian. It is...based on the assumption of an ideally successful Keynesian policy which at every point of time manages to keep the value of investment at the desired level...

This book is classical in the sense that it is built on the assumption of perfect competition and of payments to factors equal to their marginal products ... [This is not realistic but is the simplest assumption to use]...

There remains one further way in which this book is distinctly ‘classical’ in spirit. The analysis is based on the existence of the three classical factors of production—land, labour, and capital. In fact in the real world factors of production do fall into the three broad categories of natural resources, human effort, and man-made instruments. The interrelationships between these three categories is of great importance in economic growth; and in this second edition I have paid even more attention to them than was done in the original version of this book (Meade 1962: ix–xi).¹¹

In an interview for a Cambridge student magazine in 1972 he remarked ruefully:

I wish I had never used this term ‘neo-classical’ ... I ought to have called it ‘The Keynesian Classical Theory of Economic Growth’, because I am basically a very good Keynesian—though perhaps not a very good ‘neo-Keynesian’. You see the great Keynesian notion...was that you won’t automatically get full employment. But supposing that you do successfully undertake policies...giving you full employment, then all the old classical problems [allocation and distribution] come into their own again [and deserve consideration] (Meade 1972: 4–5).

¹¹ In the second edition, Meade added a whole new chapter using his model to consider growth in an overpopulated economy.

In the 1960s Meade continued to write on economic growth, with his colleagues Champernowne and Frank Hahn (Meade 1988b: 389–428), thus establishing himself in the other camp from the Cambridge Keynesians' biased point of view. As Pasinetti noted (2007: 39), Meade 'was pushed more and more into neoclassical analysis' while his friends Richard Stone and Austin Robinson were also 'left isolated', in the DAE (though Stone had lost the Directorship in 1955) and at the *Economic Journal*, respectively. Meade also became more active in areas such as distribution and population, which will not be discussed here. His own favourite book was his *Efficiency, Equality and the Ownership of Property* (Meade 1993b, 1964) which brought together his views on (real world) economic growth, the microeconomic role of the price mechanism, the size and composition of the population, and the distributional implications of property ownership. In 1968, he resigned from the Chair of Political Economy, fed up with the Cambridge Keynesians' method of conducting Faculty business (in which he was ex officio involved) as well as economic argument. His College gave him a Senior Research Fellowship which he held until his formal retirement in 1974 during which time he completed two more volumes of his treatise.

At the time he completed his second volume he thought the third might be entitled *The Fluctuating Economy*, but, having learned from younger colleagues about the theory of optimal control, it became a volume on *The Controlled Economy* (Meade 1971; see also Meade 1970). He thanked the members of a small seminar in Cambridge including David Livesey, Tony Atkinson, Geoff Heal, and David Newbery. By the time he wrote the fourth volume, he had come to doubt that he would write further volumes. But having envisaged since at least the second volume that the final volume must be on *The Mixed Economy*, he had lectured on that topic (Meade Papers 12/18) and he had written a short book, *The Intelligent Radical's Guide to Economic Policy* (Meade 1975), which 'treated in the tone of a political pamphlet much of the ground which would be covered more tediously in *The Mixed Economy*' (Meade 1976: 10).

Meade gave the last of his 'Principles of Political Economy' lectures in 1966–1967, though until 1974 he was still giving some lectures to undergraduates in Cambridge—including 'Economic Analysis and Policy' for Prelims for one year after Joan Robinson retired and a course on welfare economics the following year.¹² His 'Principles' lectures followed closely the material he

¹²In that year, the arrangement of complementary or competing lectures for Prelims resumed, this time given by Hahn and Pasinetti.

was using for his *Principles of Political Economy*. In the first volume, he had ‘claim[ed] only to present a series of “models”—i.e. of economic systems, each built on greatly simplified assumptions about human motives, technology, and social institutions—and to undertake in each case a series of “exercises”—i.e. to examine the links of causal relationship in each case’. For ‘while no final decision about policy should ever be taken without adequate empirical enquiry, experience has convinced me that a training in economic principles of [this] kind...inculcates a way of looking at things which helps greatly in reaching a sensible final decision’ (Meade 1965: 7–8).

The economist father of one student recalled that his daughter sometimes consulted him about difficulties she had encountered in lectures and showed him her notes:

Those from the Meade lectures were remarkable in that (a) it was always clear what you said and (b) one could see precisely where she had failed to take a point and run into trouble. This is the supreme test of clarity in the exposition of theory, and I have always regarded you as a great master (Worswick to Meade, 18 October 1977, Meade Papers 19/8).

When Meade was free of the demands of lecturing, he could turn to economic issues and policy problems out of intellectual curiosity and his continuing desire to make a better world. In 1974 he took on the chairmanship of an important inquiry on the structure and reform of direct taxation; his young economist colleagues included Atkinson and Mervyn King from Cambridge and John Flemming and John Kay from Oxford. Their ‘Meade Report’ came down in favour of the expenditure tax originally promoted by Kaldor (Meade 1978).¹³ In 1978 Meade returned to his Keynesian macroeconomic roots to lead a major research project on ‘stagflation’ with a group of young economists in the DAE. In his Nobel Lecture in 1977, he had revisited the concept of internal balance he had used in *The Balance of Payments* (Meade 1988a: 349–362). He said that if he were rewriting the book he would have three policy targets (the balance of payments, full employment, and price stability) instead of two (internal and external balance). He proposed a ‘New Keynesian’ assignment of policy instruments to these targets: monetary and fiscal policy to maintaining total money expenditure (nominal GDP), reform of wage-fixing arrangements to full employment and

¹³It was not the first ‘Meade Report’ which was Meade (1961).

exchange rate policies to the balance of payments. In an inflationary world, the old Keynesian assignment of demand management to the employment objective served to increase inflationary pressure which could not be suppressed by any feasible ‘incomes policy’. The monetarist alternative of controlling the rate of inflation by fixing the rate of growth of the money supply was unacceptable as it would generate intolerably high unemployment. His young collaborators, who included David Vines and Martin Weale, worked on the design of the appropriate demand management policies while he concentrated on the reform of wage-fixing arrangements (Meade 1982; see also Meade 1986a, b, 1989; Meade et al. 1983, 1989).

Although New Keynesianism might be thought to be more ‘monetarist’ than the Keynesian Non-Monetarist position of many of Meade’s Cambridge colleagues (Meade 1988a: 382), it ‘has much of the merit of the original Keynesian prescription to use financial policy to maintain *real* GDP—the general level of output and employment’ (Meade 1995: 20–21; italics in original). In his last book, *Full Employment Regained?*, published shortly before he died in 1995, Meade tackled the question whether ‘Full Employment’ could be regained without serious inflation and increasing inequality, arguing ‘as an old Keynesian’ that appropriate macroeconomic policy is always necessary to achieve full employment.

5 Conclusion

James Meade was an unusual Cambridge economist, even though he was the successor to Marshall, Pigou, and Robertson in the Chair of Political Economy. But although his most important and most lasting work was accomplished in the period between the year he spent in Cambridge in 1930–1931 and his return to Cambridge in 1957, this does mean that his work was not influenced by his time in Cambridge, nor that he failed to influence Cambridge economics. This is as true of his later Cambridge years as it is of the 1930s. In 1930–1931 he influenced the work of other members of the Cambridge Circus and he and they influenced Keynes, persuading him to move on from the *Treatise* to *The General Theory*. In the 1960s, even if his old Cambridge friends wished to sideline him, he nonetheless had a major impact on Cambridge economics. He educated a generation of Cambridge economists, influencing their views on the utility of economics and helping them to implement their ideas. As one of them wrote after his death:

Like many young economists in Cambridge in the 1960s, I owed him an enormous amount. He taught us to combine the highest intellectual standards with courtesy in debate. He encouraged us in our early research, and steered us towards exploring new fields, in my case looking at how economic analysis can be applied to urgent social problems...

If, in years since then, I have on occasion despaired of there being reasoned and humane discussion of public issues, then reading James' writings has restored my confidence. In this I was not alone (Atkinson to Margaret Meade, 29 December 1995, Meade Papers 19/9).

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33

Nicholas Kaldor (1908–1986)

John E. King

1 Introduction

Nicholas Kaldor was part of an astonishingly talented generation of Hungarian economists born in the early years of the twentieth century that included Thomas Balogh, William Fellner, George Katona, John von Neumann, and Tibor Scitovsky. The son of a prosperous Jewish lawyer, Kaldor was born in Budapest on 12 May 1908. He was educated at the University of Berlin and the London School of Economics (LSE), where he spent 20 years (1927–1947) as undergraduate, research student, and Lecturer. After two years at the United Nations Economic Commission for Europe (UNECE) in Geneva he returned to academic life in October 1949 as Fellow of King's College, Cambridge. Kaldor was appointed to a personal Chair in 1966. He retired in 1975 but remained very active in research and policy advocacy right up to his death in Cambridge on 30 September 1986. The core of Kaldor's voluminous writings can be found in the nine volumes of his selected economic essays (Kaldor 1960–1989), supplemented by the posthumously published 1984 Raffaele Mattioli Lectures (Kaldor 1996); an excellent sample of his work is provided by Targetti and Thirlwall (1989). There are three intellectual biographies (Thirlwall 1987; Targetti 1992; King 2009), and details of the Cambridge context of his work are provided by Harcourt (2006), King (2002), and Pasinetti (2007).

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In terms of institutional affiliation, Kaldor was a Cambridge economist only for the last 37 years of his life. Intellectually, though, his affiliation with Cambridge can be traced back to the late 1930s, when he came under the influence of *The General Theory* and began to discuss the new macroeconomics with Keynes's younger disciples. Geographically, Kaldor became a Cambridge economist in September 1939, on the outbreak of the Second World War, when the Aldwych premises of the LSE were taken over by the Ministry of Works and its entire staff and student body moved to Cambridge. But the focus of this chapter will be on the period after his arrival at King's in 1949.

The chapter begins with a brief discussion of Kaldor's work before 1949 (in Section 2). This is followed by the three principal sections, dealing with his theories of growth and distribution (Section 3); his writings on economic policy, first for developing countries and then for Britain, including his attack on monetarism (Section 4); and his broad-based post-1970 critique of mainstream economic theory (Section 5). In the brief conclusion (Section 6), I assess Kaldor's contribution as a Cambridge Post Keynesian economist.

2 Before Cambridge

Kaldor began the 1930s as an undergraduate student and ended it as one of the world's leading young economic theorists. He was always a prolific writer. In Berlin, at the tender age of 19, he had worked as a stringer for the Budapest press, a practice he continued for a while after his move to London. While still a research student, he was also co-translator of Friedrich von Hayek's extended critique of underconsumption theory, which was published in the LSE house journal, *Economica* (Hayek 1931).

His own publications began in 1932 with a 12-page article in the *Harvard Business Review* on the Austrian economic crisis. Over the next eight years, he published 22 journal articles, eight substantial book reviews, and a translated book (again by Hayek). Kaldor's interests covered the entire gamut of economic theory, and he also engaged with economic philosophy and policy. His 1934 paper on the nature of equilibrium theorising raised important questions of methodology, the full significance of which he only came to appreciate several decades later. In 1937, in the only article that he ever published in *Econometrica*, he defended the Austrian theory of capital. Then two major articles in 1939 established him as an original theorist of real stature. The first, drawing on Lionel Robbins's critique of interpersonal comparisons of utility, set out the compensation principle as the foundation of a non-Benthamite approach to welfare economics, while the second

dealt with the theory of money and finance and the macroeconomic consequences of speculative behaviour.

By now Kaldor had abandoned his earlier sympathy for Hayek's Austrian approach to economic theory and had become a committed Keynesian. In 1940, as editor of the *Economic Journal*, Keynes published Kaldor's paper on the trade cycle, which attempted to model the insights contained in chapter 22 of *The General Theory*. Kaldor did so in a characteristically informal manner, relying entirely on diagrams and without any algebra. He emphasised fluctuations in saving and investment, not monetary instability. Kaldor had already set out his attitude towards the quantity theory in a 1939 review of Arthur Marget's *Theory of Prices*. Money influences the economy only indirectly, through changes in the rate of interest; changes in the money stock affect output and employment, not prices; the price level depends on the money wage rate, not on the stock of money. These principles would underpin Kaldor's later objections to monetarism in theory and practice.

As already noted, Kaldor spent the war years in Cambridge. While he had become a British subject in 1934, and on the outbreak of war made enquiries about joining the civil service as an economic advisor, he was told that his Hungarian origins would disqualify him from anything other than menial duties in Whitehall. He therefore decided to stay in academia. Now based at Peterhouse, Kaldor was able to deepen old friendships and develop new ones. A war Circus of economists began to operate, named by analogy with the Cambridge Circus of young theorists who had interrogated Keynes in 1930–1931 after the publication of the *Treatise on Money* and helped to focus his mind on the revolutionary breakthrough of *The General Theory*. In addition to Kaldor, the war Circus included Joan Robinson, Piero Sraffa, and (when he could escape from official duties in London) Richard Kahn. Kaldor was particularly fond of Sraffa and also became a close friend and walking companion of Robinson.

Kaldor wrote extensively on the problems of the war economy and the prospects for post-war reconstruction, endorsing Sir William Beveridge's proposals for the introduction of a comprehensive system of social security after the war. Kaldor also set out the fundamentals of Keynesian fiscal policy, anticipating what his old LSE friend Abba Lerner was to call the principle of 'functional finance': government spending and taxation and revenue should be set to produce full employment without demand inflation, and there should be no concern as to whether the budget was in surplus or deficit. This contrasted sharply with the traditional principle of 'sound finance', which required that the budget always be balanced, either annually or over the course of the trade cycle. Kaldor's final contribution to the debate on post-war reconstruction

was to apply the principle of functional finance to the problem of maintaining full employment after the war. This was the subject of the second Beveridge Report, *Full Employment in a Free Society*, which was published in 1944 and to which Kaldor contributed a lengthy technical appendix setting out a number of alternative fiscal scenarios.

Kaldor's break with Hayek now had a political as well as a theoretical dimension. Significantly, Kaldor did not move back to London when the LSE returned there at the end of the war. Instead, he remained in Cambridge and commuted to carry out his teaching duties. In 1945 he was seconded to the United States Strategic Bombing Survey, and in the following year, he worked briefly at the Ministry of Defence as advisor to the British Bombing Survey. He also advised the Hungarian and French governments. In 1947 Gunnar Myrdal invited him to join the UNECE in Geneva. Kaldor was refused leave of absence from LSE, and so he resigned. While in Geneva, he largely wrote two of the Commission's annual 'Economic Surveys of Europe', served as adviser to the United Nations Technical Committee on Berlin Currency and Trade, and wrote much of the 'Report on National and International Measures for Full Employment' for another United Nations expert committee.

3 Growth and Distribution

When he returned to academic life in late 1949, at the age of 41, Kaldor was in mid-career. He was well known and well respected as an original theorist in both microeconomics and macroeconomics and had also made significant contributions in applied economics and to policy debates. His first major paper as a Cambridge economist was devoted to his original and controversial macroeconomic model of income distribution. It appeared in 1956, the same year as Joan Robinson published her magisterial (but seldom read) *Accumulation of Capital*, and has to be seen as part of the Cambridge project to 'generalise *The General Theory*' by extending Keynesian macroeconomics to the long period and replacing the remaining elements of Marshallian theory with something that was not subject to the logical defects that were beginning to emerge in the neoclassical theory of capital (see the Chapters 27, 30 and 31 in this volume).

In his distribution paper Kaldor explicitly repudiated both the marginal productivity of relative shares and Michał Kalecki's degree of monopoly theory (which he regarded as tautological). His own model was explicitly Keynesian in its focus on the relationship between investment, saving, and the share of profits in national income that was required for macroeconomic equilibrium.

It had been foreshadowed by Keynes himself in the well-known ‘widow’s cruse’ parable in the *Treatise on Money* and also by Kalecki in a wartime article (Kalecki 1942), and something similar was hinted at in Robinson’s book. But the details of the model were original to Kaldor.

In a simple two-class Keynesian model with no foreign sector and no government, in which investment (I) drives saving (S), and the savings propensity of the capitalists (s_c) is greater than that of the workers (s_w), the profit share is determined by the ratio of investment to income and the share of wages is a residual. In the simplest case, where workers save nothing (so that $s_w = 0$), macroeconomic equilibrium requires that

$$I = S = s_c \cdot P \quad (33.1)$$

where P is total profits. Dividing both sides of Equation (33.1) by Y and rearranging terms, we have

$$P/Y = (1/s_c) \cdot (I/Y) \quad (33.2)$$

In Kaldor’s own numerical example, $I/Y = 20\%$, $s_c = 0.5$, and the profit share $P/Y = 40\%$ (so that the wage share, W/Y , is 60%). Since nothing is saved by wage earners, capitalists must get 40% of total income so that, when they save half of it, savings and investment are both equal to 20% of Y . If I/Y were to rise to 21% , the profit share must increase to 42% , and the wage share must fall to 58% (Kaldor 1955–1956: 96, fn. 2). If the propensity to save out of wages is positive (or negative, as in the lead-up to the sub-prime mortgage crisis in the USA in 2006–2007), Kaldor’s algebra is slightly more complicated, but the underlying principle remains the same.

It is, however, subject to a rather obvious difficulty. If workers save, they also acquire capital, from which they derive an income, but no account is taken of this additional income in Kaldor’s algebra. In 1962, Luigi Pasinetti showed that this problem could be overcome by a simple reformulation of the model, leaving the underlying Kaldorian conclusion unchanged: relative income shares still depend on the ratio of investment to income and on the capitalists’ propensity to save. The workers’ propensity to save is irrelevant, whether they have income from ownership of capital or not (Pasinetti 1962). An extensive literature emerged on the Kaldor–Pasinetti macroeconomic distribution theory, much of it by Italian scholars, extending the model to an open economy with a government sector and with rentiers as well as capitalists.

The microeconomic implications also began to be teased out, with the appearance of oligopoly pricing models in which the size of the markup over variable costs of production was determined by the company's need for retained earnings to finance its investment plans (see Chapter 9 in this volume).

This points, however, to an empirical problem with the Kaldor–Pasinetti model. It is best suited to a world of managerial capitalism, characterised by the separation of ownership and control, in which the behaviour of the modern corporation can be summarised as: 'retain and reinvest'. This is the world described by Berle and Means in their classic *The Modern Corporation and Private Property* (1932), published at a time when the power, prestige, and moral authority of financial markets were at an all-time low, and the autonomy of professional managers was correspondingly large. In the twenty-first century, financialisation—more accurately, the continuing process of *refinancialisation* that began in the 1970s—has undermined this autonomy, restoring 'shareholder value' as the driving force underpinning corporate behaviour and enforcing a new guiding principle: 'distribute and downsize'. Macroeconomic equilibrium still requires that Equations (33.1) and (33.2) be satisfied, but the causal relationships between the variables are no longer obvious.

The profit share has in fact increased in almost every advanced capitalist economy since about 1980, but this has not been the result of a sustained increase in I/Y ; instead, s_c has decreased—a possibility not seriously considered by Kaldor, though the reverse phenomenon (increased working class militancy generating a rising wage share and a corresponding increase in s_c) had been suggested by Maurice Dobb (1929). Institutionalists and Marxians would conclude from all this that Kaldor had ignored the broader social and political determinants of class shares, which in practice were more important than the abstract macroeconomic relations that he had emphasised.

None of this was obvious in 1957 when Kaldor published his first formal model of economic growth. This was his initial contribution to the 'generalisation of *The General Theory*' to the long period by tracing out the implications of positive net investment and the resulting increase in the capital stock. The Oxford economist Roy Harrod (1939) had led the way with his demand-driven (more precisely, investment-driven) growth model, in which the 'warranted' (or equilibrium) rate of growth (g_w) depended on the ratio of investment (and hence saving) to income (s) and the technically determined ratio of capital to output (v): $g_w = s/v$. However, the maximum or 'natural' rate of growth (g_n) depended on two supply-side factors, the rates of growth of population and of labour productivity, with the latter being determined exogenously by technical progress. Harrod noted that there was no guarantee

that g_w would be equal to g_n , or that either would equal the actual growth rate (g). He concluded that any initial inequality between g_w , g_n , and g would give rise to substantial and continuing macroeconomic instability in the form of steadily rising unemployment or accelerating demand inflation. This was the notorious ‘Harrod knife-edge’.

In practice, capitalist economies were not wildly unstable, and Kaldor believed that his distribution model helped to explain why. The aggregate savings propensity (s) was not constant, as Harrod supposed, but variable. As we have seen, for Kaldor it was closely related to the distribution of income, and in his 1957 growth model changes in the shares of wages and profits served as an indispensable source of macroeconomic stability. In other respects, Kaldor’s model was similar to Harrod’s. They were both one-sector models, with no distinction being made between agriculture, manufacturing, and services, and they both assumed a closed economy. Unlike the canonical neoclassical Solow–Swan model, first published in the previous year (Solow 1956; Swan 1956), in Kaldor’s 1957 model growth was demand-determined, with investment driving savings rather than savings driving investment. There was no aggregate production function, no question of capital–labour substitution in response to relative prices, and hence no possibility of v being variable, which was the mechanism that provided stability in the neoclassical growth model.

In one crucial respect, however, Kaldor broke with Harrod. He argued that technical progress was endogenous: it depended on investment, through what came to be known as the ‘vintage effect’ (referring to the superiority of old wine and new technology). The higher the ratio of investment to income, Kaldor maintained, the lower the average age of the capital stock and the greater the proportion of machines that embodied more recent, and therefore more productive, technology. Kaldor introduced a ‘technical progress function’ that made productivity growth a positive function of I/Y and gave an additional reason to expect macroeconomic stability, since an increase in g_w could now be expected to induce a corresponding increase in g_n . He went even further, claiming that sustained growth was consistent only with continuous full employment, a strangely anti-Keynesian notion that led Paul Samuelson to describe him sardonically as ‘Jean-Baptiste Kaldor’.

Kaldor was no mathematician, and his 1957 growth model was itself a low-tech affair, which must have contributed to the neglect that it suffered outside Cambridge. Five years later, he published a much more sophisticated version, the mathematics being provided by his young co-author, the future Nobel Laureate James Mirrlees. Kaldor’s 1962 model made an even sharper break with neoclassical growth theory, since there was no longer any mention of the capital stock, only its rate of increase: everything now depended on the

rate of investment. Possibly this reflected the influence of Sraffa's *Production of Commodities by Means of Commodities*, which had appeared two years previously and cast serious doubt on the coherence of neoclassical capital theory. By this time, Kaldor had fallen out with Joan Robinson, whom he now accused of being excessively neoclassical in her own approach to the theory of growth.

In one respect, though, he was becoming dissatisfied with his own analysis. The 1962 model, like that of 1957, was a one-sector affair, and this seemed to Kaldor to shed little light on the poor growth performance of the post-war British economy by comparison with the rapid growth that had been achieved in many parts of Continental Europe. In his 1966 Inaugural Lecture, *Causes of the Slow Rate of Economic Growth of the United Kingdom*, he developed what was effectively a third model of economic growth, now emphasising the crucial difference between the primary and secondary sectors. The British economy had matured early, he argued, in the sense that primary production now accounted for a very small proportion of total employment, and the scope for productivity gains by transferring labour from agriculture to industry was correspondingly low. This meant that manufacturing output was growing more slowly in Britain than in countries like France, West Germany, and Italy, where there remained a large reservoir of low-productivity agricultural labour that could move to industrial employment. This was a very important point, since there were dynamic increasing returns to scale in manufacturing, as revealed by Verdoorn's Law (discovered by Kaldor's former colleague at UNECE, the Dutch economist P.J. Verdoorn): productivity growth is a positive function of the rate of growth of output. So the relatively immature Continental economies were enjoying much faster productivity growth than was possible in Britain, or for that matter in the USA.

Kaldor's 1966 model was not recognisably Keynesian, and indeed it was almost neoclassical in its emphasis on the supply of labour to manufacturing industry as the fundamental constraint on growth. It was also once again an informal, low-tech affair, almost devoid of algebra, but (he believed at the time) both more realistic and more useful than his earlier efforts had been. Once again, however, he soon became dissatisfied with it. This time he focused on its neglect of the external sector. By 1970 Kaldor had come to see exports as the only genuinely exogenous source of effective demand and the balance of payments as the binding constraint on growth. This reflected his frustrations as an adviser to the British government after 1964, where recurrent balance-of-payments difficulties gave rise to a 'stop-go' economy, and characteristically he never developed the arguments in any systematic way. However, the subsequent formalisation of Kaldor's insights by John McCombie and Tony Thirlwall has generated a substantial international literature on balance-of-payments-constrained growth.

Two final points should be noted in relation to Kaldor's growth models. First, they anticipated what later came to be known in the mainstream literature as 'endogenous growth' theory, and he is sometimes given credit for this by economists who are otherwise unsympathetic to his approach to economics more generally. Second, they have significant implications for the analysis of economic development on a global scale, as Kaldor acknowledged in the 'North–South models' that he produced in the final decade of his life.

4 Economic Policy

Kaldor's interest in development economics had begun much earlier, in the aftermath of the post-war decolonisation and the emergence of what Kalecki had termed 'intermediate regimes', like those in India and Indonesia, which were neither communist nor fully committed to free market capitalism. Kaldor belonged to a generation of economists who believed that the problems of what were then termed the 'under-developed countries' (in today's language, the 'global South') were profoundly different from those of the North, and that the same policies (and even, to some extent, the same theory) could not be applied uncritically to them. This marks a big difference from the intellectual world of the so-called Washington Consensus that emerged in the neoliberal world of the 1970s, and even from the post-Washington Consensus that subsequently replaced it.

In this sense—and only in this sense—Kaldor was sympathetic to Marxism, since he accepted the principle that economic theory should be historically and socially specific, not universal and timeless. In every other respect, however, he was highly critical of Marxian political economy, which (as he told a Beijing audience in a 1956 lecture) he believed to be relevant only to the unreformed capitalism of the mid-nineteenth century. He had no sympathy for the economically inefficient and politically repressive system of the Soviet Union and Eastern Europe in the post-1945 era (and indeed had relatives who suffered under the communist regime in his native Hungary).

He did, however, have a strong interest in economic policy and a taste for offering advice to governments, which could not be fully satisfied in Britain during the 'thirteen wasted years' of Tory rule between 1951 and 1964 (Morgan 1990: 236). Instead, he turned to the Third World, travelling widely and serving as a consultant to several governments. Between 1956 and 1962 he visited India, Ceylon (now Sri Lanka), Mexico, Ghana, British Guiana (now Guyana), and Turkey, and his writings on the problems of economic development can be found in five of the nine volumes of his *Collected Economic Essays* (all of volume 8 and parts of volumes 3, 4, 6, and 9). His influences included the Dutch

social democrat Jan Tinbergen, other former colleagues from UNECE, and Latin American structuralists like Raul Prebisch. Economic development was a major interest of many Cambridge economists in the 1950s and early 1960s, and Kaldor will certainly have discussed the central issues with Joan Robinson (though he never shared her interest in or enthusiasm for Mao's China).

Kaldor recognised that the unemployment problem in backward areas was enormous. It was also chronic and structural rather than cyclical and almost certainly not susceptible to rapid reduction through Keynesian policies of aggregate demand management. Similarly, one-sector growth models of both the Harrod–Domar and Solow–Swan varieties, which ignored the distinction between primary and secondary activities, were of very doubtful relevance to the problems of the poorest countries. Kaldor was stating the (almost) obvious when he asserted (in 1964) that the key to an accelerated growth of the underdeveloped areas of the world lay in bringing about fundamental changes in both the mental outlook and the technical knowledge and skill of their peasant populations. He pointed to the survival, especially in agriculture, of a traditionalist outlook that discouraged risk-taking and profit-making. He concluded that the problem of economic development could not be left to the economists, since it required an explanation of how different mental attitudes came to develop in society, why at certain stages of a society's development traditionalism had given way to rationalism, and hence would benefit from the integration of economics and sociology.

However, Kaldor also had some sympathy for the American Marxist Paul Baran, who, in his 1957 *The Political Economy of Growth*, had emphasised the role of social stratification in the countryside, rather than the backwardness of an undifferentiated rural population. Latin American countries, in particular, had a tremendous burden to carry in the form of maintaining the idle rich: the detrimental effects of what Kaldor graphically described as the unbridled greed of an oligarchical ruling class were very serious. The rulers enjoyed a larger share of total income than their more productive counterparts in the advanced capitalist countries, were much less subject to progressive taxation, and spent a much larger proportion of their income on wasteful luxury consumption, much of it on imported goods.

All this had serious implications for economic policy. Kaldor argued that there was a strong case for increasing taxes on land in developing countries. This would enlarge the supply of foodstuffs to urban areas by encouraging landlords to bring some of their idle land into cultivation, and thus expand the amount of employment that could be offered outside agriculture without creating inflation. It would also encourage the more efficient use of land, in part because it would lead to the transfer of land ownership from relatively

inefficient to efficient cultivators. While land reform was necessary, however, it was not sufficient. In a controversial report to the Government of India on taxation reform, Kaldor concluded that both equity and efficiency considerations required that progressive taxes be imposed also on income, capital gains, new wealth, personal expenditure, and gifts. Like his recommendations to the governments of Ghana, Guyana, Mexico, and Turkey, these proposals aroused strong opposition from the rich and were never likely to be implemented.

On other aspects of economic policy, Kaldor took an even less orthodox position, revealing his sympathies with structuralists like Prebisch. The virtues of stable and uniform exchange rates, currency convertibility, low tariffs, non-discrimination, and the prohibition of export subsidies were genuine enough, Kaldor maintained, when applied to the conduct of the industrialised countries in their trading relations with each other. But they were much less evident when applied to the underdeveloped countries. He was a forceful critic of many aspects of the Washington Consensus, although he was always suspicious of import-substitution industrialisation and wherever possible favoured export-led growth.

As already noted, Kaldor's interest in economic policy for Britain began in the early stages of the Second World War and continued through his co-authorship of the technical appendix to the 1944 Beveridge Report and his spell at UNECE in 1947–1949. But he was unable to exercise the influence that he would have liked on British government policy after 1945, being too young (and perhaps also too foreign) to serve as a senior adviser to the Attlee government and too left-wing for the Tories. Thus Kaldor came into his own as a policy adviser only after the Labour Party's election victory in 1964.

He was in close contact with younger Labour politicians like Anthony Crosland and Hugh Gaitskell, and he did produce some impressive policy work before Labour returned to power. His 1951 paper on incomes policy remained unpublished until 1964, when he visited Australia and applied it to the problem of combating inflation in this small open economy. Convinced of the importance of cost-push inflation, in which the rate of increase of money wages played a central role, Kaldor argued that a national wages policy was essential to maintain price stability under conditions of full employment. To maintain the existing income shares of labour and capital, this required that money wages increase, on average, at the same rate as labour productivity.

Kaldor maintained that this average rate of wage growth should be applied throughout the economy so that industries with above-average productivity growth should be forced to reduce prices, offsetting price increases in industries with below-average productivity improvement. He was opposed to the suggestion that wages might be allowed to increase more rapidly in high-productivity growth sectors, on the grounds that this involved the

workers in less progressive industries subsidising their employers' inefficiency. Thus he was not a supporter of what later came to be known as 'productivity bargaining' at the level of the industry or the individual company (in Australian parlance, 'enterprise bargaining'). Kaldor's ideas were taken up by Eric Russell and Wilfred Salter and formed the basis of the influential Russell–Salter rule for national wages policy in Australia.

In 1951, which proved to be the final year of the Attlee Labour government, Kaldor had been appointed a member of the Royal Commission on the Taxation of Profits and Income. While he had no influence over the taxation policy of the incoming Conservative government, the experience did encourage him to write a 250-page book on the issues considered by the Commission. In *An Expenditure Tax* (1955), Kaldor argued the case for a progressive tax on 'spending power' rather than income. Earlier proponents of expenditure taxation, who included Mill, Marshall, Pigou, and Fisher, had emphasised the advantages of exempting saving from tax. Kaldor made the same point from a different angle, focusing on the need to tax *dis*-saving. In the 'age of austerity' that characterised the British economy in the late 1940s and early 1950s, wealthy people had been able to maintain their standard of living by running down their capital, an option that was not available to poor people. This, Kaldor argued, was patently unfair. Income taxation was also inefficient since it penalised saving and productive enterprise. Thus an expenditure tax would be both more equitable and more efficient than the existing system.

The book was favourably reviewed by eminent tax theorists like Richard Musgrave and Carl Shoup, but there was never any prospect of Kaldor's proposals (which included a marginal tax rate of 300% on annual expenditure in excess of £5,000) being implemented by the Conservative government of the day. However, he continued to develop his ideas on policy issues. In 1959 Kaldor, like his Cambridge colleague Richard Kahn, made a detailed submission to the Radcliffe Committee on the Working of the Monetary System. In written and verbal evidence, he repeated his earlier objections to monetarism. There was no evidence that the velocity of circulation was constant, Kaldor maintained, and hence no basis for the belief that the volume of expenditure was determined by the quantity of money in circulation. In fact, the velocity of circulation was a variable that depended on the course of monetary policy: thus attempts to restrict the money supply would be frustrated by the resulting increase in velocity. Monetary policy would work, if at all, only indirectly, through changes in interest rates, and there was very little evidence to support the use of higher interest rates as an anti-inflationary measure.

Kaldor came into his own as a policy adviser during the eight years of Harold Wilson's Labour governments (1964–1970 and 1974–1976). He served as Special Adviser to two Chancellors of the Exchequer, James Callaghan and Dennis Healey, and was at last able to see some of his ideas put into practice. The most celebrated and controversial measure was the Selective Employment Tax (SET), introduced in the 1966 budget to encourage the growth of employment in manufacturing at the expense of the service sector. In the following year, the proceeds were used to fund a Regional Employment Premium (REP), a subsidy to jobs in manufacturing in regions of high unemployment that was (as Kaldor noted) in effect a devaluation confined to the depressed areas of the UK. The intention was to exploit Verdoorn's Law by promoting faster output growth in the secondary sector, thereby improving productivity growth, increasing the country's competitiveness in export markets and permitting a faster rate of growth of the entire economy.

In this way Kaldor's growth theory was applied directly to macroeconomic policy. It was always controversial, and its critics regarded it as at best an irrelevance and at worst an impediment to the performance of those sectors of the British economy (above all financial services) with the best prospects of future expansion. He would have responded that the new tax and subsidy measures were long-term policies that were not given the chance to work; SET was abolished by the Conservatives in 1972, and REP was sacrificed four years later by the Labour government in the first wave of monetarist-inspired public expenditure cuts that Kaldor strongly opposed.

Kaldor was at odds with the majority of opinion in the Labour Party on another important economic policy issue, British membership of the European Union (then known as the Common Market). In the early 1970s this opposition was not confined to the anti-immigration populist right, but extended across the political spectrum to include elements of the left and (like Kaldor) the centre of the Labour Party. Kaldor objected that membership of the Common Market would benefit capital at the expense of labour, would adversely affect all sections of British society through the higher food prices imposed by the Common Agricultural Policy, and—this was his crucial argument—disadvantage British manufacturing industry by subjecting it to unrestricted competition from the faster-growing Continental countries that were benefitting from Verdoorn's Law. Instead, he urged Britain to retain a degree of national sovereignty, above all on the currency and the exchange rate. Had the issue arisen in his lifetime, Kaldor would certainly have been strongly opposed to British membership of the European Currency Union and the proposed adoption of the euro in place of sterling.

In his Euroscepticism, Kaldor had something in common with Margaret Thatcher. On every other issue he was a stern critic of the new neoliberalism. He was resolute in his opposition to the Thatcher government's economic and social policies, using his position in the House of Lords (to which he had been appointed in 1974) to attack the class war that the Conservatives were waging against organised labour and to denounce the huge and quite unnecessary costs of their efforts to overcome inflation. In 1983, 20 of his speeches were published in a slim volume entitled (with an obvious bow to Keynes's 1926 attack on Winston Churchill) *The Economic Consequences of Mrs Thatcher*. He did not worry unduly about causing offence. Thatcher's economic policies could be compared, Kaldor believed, with the deflationary measures introduced by Philip Snowden and Ramsay MacDonald in 1931; her administration was as bad as the disastrous Brüning government that had paved the way for Hitler's rise to power in the Germany of the early 1930s. Moreover, his attack on 'the scourge of monetarism' (Kaldor 1982) was now increasingly linked to criticism of mainstream economic theory as a whole.

5 Economic Theory

As early as 1966, in his comments on the defence of the marginal productivity theory of distribution by Paul Samuelson and Franco Modigliani, Kaldor had criticised neoclassical economic theory for its intellectual sterility. Quite apart from their highly questionable assumption of a well-behaved, linear homogeneous aggregate production function, neoclassical economists were unable to deal with increasing returns to scale, learning by doing, oligopolistic competition or uncertainty, all important features of the real world, which Kaldor described as being 'non-Euclidean' in nature. He developed these themes in subsequent critical work, including his 1972 Presidential Address to the Royal Economic Society, the Prefaces that he wrote in the late 1970s and early 1980s to the various volumes of his collected essays and in the Raffaele Mattioli Lectures that he gave in Milan in 1984, two years before his death. Kaldor emphasised several themes: the methodological problems that were raised by the use of equilibrium theorising, in particular for the theory of economic growth; the macroeconomic implications of a non-perfectly competitive economy; international issues, including the theory of trade and the prospects for global economic development; and the analytical and policy issues posed by the theory of endogenous money.

Kaldor regarded the habits of thought engendered by equilibrium theorising as a major obstacle to the development of economics as a science.

They required assumptions that were either unverifiable or directly contradicted by the evidence and distorted the focus of economic analysis, moving it away from the dynamic, creative functions of markets to a concentration on their static allocative functions. These errors were closely related to the assumption of constant rather than increasing returns to scale, which had led economists to ignore the forces of cumulative causation emphasised by his former UNECE colleague Gunnar Myrdal. Thus they had led them also to neglect history.

Once increasing returns to scale were allowed for, Kaldor maintained, the forces making for continuous change must be viewed as endogenous, and equilibrium states must be seen as path-dependent so that the end-state itself was no longer independent of the route taken towards it. This cast doubt on the entire Walrasian project, which had also gone astray by exaggerating the role of substitution and playing down the essential complementarity between different factors of production and different types of economic activity. Complexity theory and agent-based modelling were still in their infancy when Kaldor died, but he would probably have been intrigued by both projects.

There were significant implications also for macroeconomic analysis, not least for the theory of growth. The phenomena of increasing returns, cumulative causation, and path dependency reinforced Kaldor's insistence on the endogenous nature not only of technical change but also of the supply of labour and capital. This endogeneity was inconsistent with Harrod's notion of a 'natural' rate of growth that was given by the supposedly exogenous growth rates of the labour force and of technical progress. Here, Kaldor was on firm ground. His interpretation of the short-period macroeconomic consequences of increasing returns is less easy to justify. Unduly influenced by the Harvard theorist Martin Weitzman, he came to believe that the Keynesian principle of effective demand required imperfect competition and so was inconsistent with Keynes's own assumption of perfectly competitive product markets. In effect, this was an endorsement of New Keynesian microfoundations for macroeconomic theory, though Kaldor himself never used the term. He would certainly have objected to the dynamic stochastic general equilibrium (DSGE) models with sticky prices that have come to dominate the New Keynesian variant of the New Neoclassical Synthesis in the three decades since his death.

Kaldor's hostility to equilibrium theorising also made him increasingly critical of the orthodox theory of international trade, with its presumption that protection was always welfare-reducing. This was true under constant returns to scale, he conceded, when free trade would benefit all participants. It was a different matter under increasing returns, when trade would

tend to enlarge differences in comparative costs instead of reducing them. Thus free trade might be disadvantageous to poor countries, and to poor regions within rich countries, leading to an increasing gap between prosperous and depressed areas. Applied to the world as a whole, this led him to reassert his earlier views on global economic development in the form of a North–South model in which primary producers, who enjoyed neither increasing returns nor oligopolistic product market power, tended to fall further and further behind the industrialised countries, which benefited from both.

Kaldor's initial attack on the new monetarism came in a public lecture in March 1970, which was published four months later in the widely circulated *Lloyds Bank Review*. He raised strong objections to Milton Friedman's econometrics and his interpretation of US economic history but now emphasised a fundamental theoretical question: the endogeneity of the stock of money. The rate of change of the money supply, Kaldor argued, was not under the direct control of the monetary authorities, as the monetarists maintained. On the contrary, changes in the money supply were determined by the rate of change of money incomes, and thus depended on the factors that influenced this magnitude: on changes in the pressure of demand (including domestic investment, exports, and fiscal policy) and on the rate of wage inflation. Even more clearly than in his evidence to the Radcliffe Committee, Kaldor now insisted that the direction of causation in the quantity equation ($MV = PT$) ran from right to left, not from left to right: changes in the price level (P) and the level of output (proxied by T , the volume of transactions) caused changes in the stock of money (M) and in the velocity of circulation (V).

Ten years later, with the Thatcher government in power and what Kaldor described as 'the scourge of monetarism' now being wielded against the British working class, he drew one of the most influential diagrams of the era. Monetarism assumed a *vertical* money supply curve, with the stock of money as a fixed multiple of the quantity of reserves. Kaldor's theory of endogenous money entailed a *horizontal* curve, with the supply of money being perfectly elastic at the prevailing interest rate set by the monetary authorities. This was a remarkably original contribution to the debate on monetarism. Not even severe critics of Friedman's approach, like the American Post Keynesians Paul Davidson and Basil Moore, had gone this far.

With the benefit of hindsight it can be seen that Kaldor won the battle but lost the war. British and American experience in the 1980s proved that the stock of money could not be controlled by the authorities, for precisely the reasons that Kaldor had given: it was an endogenous, not an exogenous, variable. The Taylor rule established the rate of interest as the critical—indeed, the only—instrument of monetary policy, just as Kaldor had always insisted.

But the New Neoclassical Synthesis that emerged in the decade after his death would have been abhorrent to him for several reasons, including its abandonment of any explicit commitment to full employment, its rejection of fiscal policy, and the requirement that central banks be ‘independent’—independent of democratic control by parliament, he would have complained, but increasingly and dangerously dependent on financial markets.

Kaldor repeated many of these arguments in the Mattioli Lectures, which were the closest that he ever came to writing a theoretical treatise, along the lines of Keynes’s *General Theory* or Robinson’s *Accumulation of Capital*. As he himself admitted, his views were constantly changing and never sufficiently coherent or systematic to be put down in a single lengthy work. Still less did he ever contemplate writing an undergraduate textbook (in view of the commercial and intellectual failure of the 1973 Robinson–Eatwell *An Introduction to Modern Economics*, this was probably no bad thing). But his ideas on theoretical questions were always interesting and often original and provocative, as even a strong opponent like Frank Hahn conceded, after Kaldor’s death (Hahn 1989).

6 Conclusion

Along with Joan Robinson and Richard Kahn, Kaldor was one of the most eminent of the Cambridge Post Keynesians. He was appointed to the Academic Board of the *Journal of Post Keynesian Economics* when it was established by Paul Davidson and Sidney Weintraub in 1977—a purely honorary position, but also a prestigious one—and six years later the journal published a symposium on his work. But Kaldor was also his own man, and he never fitted into any of the more or less coherent and distinctive sub-schools of Post Keynesian thought. He was certainly not a Fundamentalist Keynesian like Davidson, but neither was he a Kaleckian with a Sraffian tinge like Robinson, and he showed little or no interest in Hyman Minsky’s financial instability hypothesis. This stubborn independence of thought is reflected in his academic publications, which numbered 140 in all; just four of them were co-authored.

By the time of Kaldor’s death in 1986, the decline of Cambridge Post Keynesianism was well advanced. The full story of the neoclassical renaissance in Cambridge has yet to be told, and it is therefore impossible to make any confident assessment of Kaldor’s share of the responsibility for it. In all probability, it was the inevitable result of the global transformation of academic economics that began in the USA after 1945 and in subsequent decades spread, more or less rapidly, to the rest of the non-communist world.

That said, it might perhaps have been attenuated or delayed somewhat if the leading Cambridge Post Keynesians had been less egoistic, more inclined to cooperate with each other, and more adept at the administrative manoeuvres that were needed to ensure that their generation of dissident economists was succeeded by younger scholars more in sympathy with their mode of thought than the likes of Hahn.

These criticisms should not be allowed to obscure Kaldor's substantial and enduring achievements. Some indication of the abiding influence of his work can be seen in the recent, authoritative two-volume *Oxford Companion to Post Keynesian Economics* (Harcourt and Kriesler 2013). One entire chapter, by Mark Setterfield, is devoted to 'Endogenous Growth: A Kaldorian Approach', and much of Robert Blecker's chapter on growth in open economies is concerned with Kaldor's treatment of export-led growth and the balance-of-payments constraint on growth. There are references to Kaldor in 19 of the remaining 44 chapters and (of course) in the editors' introduction, including several citations of his 1956 income distribution paper, his various articles on endogenous money and the scourge of monetarism, and his writings on equilibrium, business cycles, and wages policy. Apart from Keynes himself, only Michał Kalecki (with two chapters devoted to him, and references in 17 other chapters) and Joan Robinson (references in 20 chapters) come anywhere close. Thus Kaldor remains an essential source of ideas and inspiration for twenty-first-century Post Keynesian economists.

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David Gawen Champernowne (1912–2000)

Mauro Boianovsky

1 Introduction

Brian Reddaway (2000), David Champernowne's colleague at Cambridge for many decades, described him as 'one of the most colourful economists of the 20th century'. One of the most gifted Cambridge mathematical economists, Champernowne's (or 'Champ', as he was known to his friends) main contributions were in the field of economic statistics (measurement of income distribution and inequality; and probability, decision-making, and estimation methods in economics), and in the form of enlightened commentaries on works by Maynard Keynes, John von Neumann, Joan Robinson, Nicholas Kaldor, and others. Kaldor (1989: x)—chairman of the editorial committee of the *Review of Economic Studies*, where the English version of von Neumann's (1945–1946) mathematically demanding essay appeared—would recall that he asked Champernowne (1945–1946), 'the most mathematically-minded economist I knew, to write an explanatory paper *ad usum delphini*, for the use of the semi-numerates, to appear alongside' the translation. As noted by Reddaway, Champernowne loved behind-the-scenes refinement of ideas, from which A.C. Pigou, Dennis Robertson, Robinson, Kaldor, Piero Sraffa, and other Cambridge economists benefited.

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Champernowne held chairs at both Oxford and Cambridge Universities, but even during his Oxford period (1945–1959) he kept close ties with Cambridge.

Champernowne was born on 9 July 1912, in Oxford and died on 19 August 2000, in Devon. His father, Francis Champernowne, was a barrister and bursar of Keble College, Oxford. Champernowne was educated at Winchester College, a well-known traditional independent school for boys, where he first became attracted to mathematics. His mathematical talent secured him in 1931 a mathematical scholarship to King's College, Cambridge, where he was supervised together with Alan Turing, the pioneer of modern computing and codebreaker at Bletchley Park during the Second World War (for more on Champernowne's biographical background and general assessments of his contributions, see Cowell (1987, 2004), Reddaway (2000), Harcourt (2001), Pesaran (2004), and Presley (2013)). Champernowne and Turing would become lifelong friends, and, during their period at King's they designed one of the first chess computer machines, nicknamed 'Turochamp'. While still an undergraduate, Champernowne (1933) wrote a paper on 'normal numbers', which became one of his most cited articles. He provided the first example of such a number in base 10, known as the 'Champernowne Constant': 0.123456789101112... It is obtained by concatenating the natural numbers 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, ... after the decimal point. It is 'normal', that is, whether expressed in base 10 or any other base, it is constructed in such a way that each digit occurs with equal frequency (see also Pesaran 2004). Around that time, Champernowne had already been exposed to economics. When still at Winchester, he came across Marshall's *Principles of Economics* in the school library. His incipient interest in economics was confirmed by an early encounter with Robertson at Cambridge, followed by Keynes's advice to abandon his plans of becoming an actuary and turn to economics instead (Cowell 1987).

Champernowne switched to the Economics Tripos by taking the Mathematical Tripos in double-quick time (one year rather than the normal two), and by October 1933 started (together with Brian Reddaway) studying under Keynes's supervision and attending his lectures on the forthcoming *General Theory* (Keynes 1936 [1973]). Champernowne kept notes of Keynes's lectures, which have been used as one of the main sources of Rymes's (1987, 1989) compilation. Champernowne also took part and contributed papers in 1934–1935 to the weekly meetings of the Cambridge Political Economy Club run by Keynes (Pesaran 2004). Around that time he became, probably under Keynes's guidance, a member and secretary of the highly prestigious, selective, and somewhat controversial Cambridge Conversazione Society, better known as the 'Cambridge Apostles' (see Deacon 1985). The Apostles was essentially a conversational society, which required of its members that they be ready to question

any established views. Champernowne (1936) would follow that standard in his review of *The General Theory*, partly based on Keynes's 1933–1935 lectures, which questioned Keynes's analysis of the dynamics of the labour market and introduced the role of price expectations in determining the equilibrium level of real wages and employment. As noted in *The Telegraph's* obituary of Champernowne, '[T]his was a courageous stand to take, for it struck at the heart of Keynes's arguments, and Champernowne, still only 23, could hardly have improved his prospects in Cambridge economics by taking issue with his eminent supervisor'. Even though critical at some points, Champernowne's path-breaking mathematical and diagrammatical formulation of Keynes's system did not represent an attempted return to 'classical' economics, as discussed below.

Aware of Champernowne's mathematical and statistical skills, Keynes suggested that he should search for an explanation of the remarkable degree of conformity of income distribution data with the well-known Pareto's Law (Champernowne 1973: 1). That was the starting point of Champernowne's 1936 dissertation on *The Distribution of Income Between Persons* submitted successfully for a Prize Fellowship at King's College, but only published 37 years later. The core of the dissertation came out in article form in Champernowne (1953). It laid the foundations for the application of stochastic process models to interpreting income distributions. The economic-statistical analysis of income distribution would become a permanent focus of Champernowne's research agenda, culminating with his 1998 book co-authored with Frank Cowell, Professor at the London School of Economics (LSE) and a former PhD student of Champernowne's at Cambridge. After completing his Fellowship dissertation, Champernowne took a position as Assistant Lecturer at the LSE (1936–1938), where he investigated the empirical distribution of unemployment in the UK, under the influence of LSE economist William Beveridge (see Champernowne 1938, 1939). He returned to Cambridge in the late 1930s as a University Lecturer in statistics and Fellow of King's College (1938–1940).

Champernowne's academic career at Cambridge was interrupted by the Second World War, when he was drafted into the Statistical Section of the Prime Minister's Office (1940–1941) and later as Assistant Director of programmes in the Ministry of Aircraft Production. While in London, he collaborated with James Meade and Richard Stone in a study about the use of statistical adjustment techniques in assessing the quality of measurement of national accounts, called the 'Standard Adjustment Procedure' (Stone et al. 1942; see Comim 2001: 227–229). After the war, Champernowne moved back to his birthplace, where he became a Fellow of Nuffield College and Director of the Oxford Institute of Statistics (OIS), which had been created in 1935. Champernowne was the third Director of OIS (1945–1948), after Jacob Marschak and Sir Arthur Bowley. OIS was Oxford's first research institute in economics, with a role similar to Cambridge's

Department of Applied Economics. In 1948, Champernowne became Oxford Professor of Statistics and published an important article on time series analysis of autoregressive processes. Champernowne (1948) represented the first major attempt at the application of Bayesian techniques to time series analysis at a time when such techniques were out of fashion. It reflected Champernowne's continuing interest in Ramsey's probability theory (Cowell 2004). In a related later paper, Champernowne (1960) used computer simulation and essentially anticipated Granger and Newbold's (1974) discussion of the spurious correlation problem in econometrics (Pesaran 2004).

During his time at Oxford, Champernowne produced some of his most important papers, typically closely connected with the Cambridge research agenda in economics. This started with Champernowne's (1945–1946) literary interpretation of von Neumann's model of economic equilibrium, described by Harcourt (2001: 441) as 'lucidity in itself'. His better-known article from that period is probably his extended comment on Joan Robinson (1953–1954), in which Champernowne (1953–1954) clearly discussed, for the first time in the economics literature, reswitching and capital-reversing in capital theory. However, it was only later that Champernowne's early formulation of the essential elements of both sides of the Cambridge capital controversies of the 1960s was fully acknowledged (Harcourt 1972; Birner 2002). Kaldor's (1957) model of capital accumulation and growth also attracted Champernowne's (1958) critical attention, where he deployed a Kaldorian saving function but reached somewhat different results. Meanwhile, his contribution to the 1958 Corfu conference on the theory of capital focused on the consequences of labour-saving technical progress for economic growth (Champernowne 1961). Despite his high productivity throughout the 1950s, Champernowne's close links with Kaldor, Robinson, Robertson, Kahn, and Sraffa led him to prefer Cambridge's intellectual environment and thus to his resignation from his Oxford Chair in 1959 in order to take a Cambridge Readership and a Trinity Fellowship. He retired in 1978, but remained Professor Emeritus and a Fellow of Trinity. The most tangible result of the first decade of Champernowne's return to Cambridge was his trilogy on *Uncertainty and Estimation in Economics*, which represented the peak of his long-time interest in Bayesian probability and decision-making under uncertainty. Champernowne's (1969) awareness of the role of imperfect knowledge reflected also his personal experience with aircraft programming during the war, which had shown how inapplicable probability assumptions are (*The Times* 2000). Upon its publication, Champernowne was given a personal Chair as Cambridge Professor in Economics and Statistics and was elected Fellow of the British Academy in 1970.

2 Keynesian and Classical Approaches to Unemployment

Champernowne's (1936) critical reaction to *The General Theory* represented an attempt to fill the gap of the determinants of changes in money wages in Keynes's framework, which he identified as a weak spot in the argument of the book (Joan Robinson also criticized this aspect of Keynes's analysis; see Boianovsky 2005). Champernowne accepted Keynes's argument about the relation between the labour market and the market for goods, with its implication that a change in money wages can only affect the level of employment through its indirect effect on the determinants of effective demand, such as the rate of interest and the marginal propensity to consume. Champernowne suggested that Keynes's assumption that workers are more sensitive to changes in money wage rates than to changes in prices only applies in the short run. As time goes by, workers realize that prices are changing and adjust their money wage claims accordingly. When there are no unanticipated price changes, the economy will settle at the 'basic rate of unemployment' decided by the equilibrium between the supply and demand curves for labour written as functions of real wages only. Champernowne pointed out that real wages will move in the same direction as money wages only if aggregate demand changes in the process. Champernowne's 1936 comparison between the 'classical' and the 'Keynesian' analyses of unemployment was the first attempt to interpret *The General Theory* using equations and diagrams.

The explanation given by Champernowne (1936: 202) of money wage stickiness in the short run is based on the existence of contracts made on the expectation of a stationary cost of living and on workers' habit of thinking in terms of the price level of the past. An unexpected rise in the price level will, therefore, shift the 'real supply curve for labour' (ibid.: 215) rightwards at every real wage rate, causing a reduction of real wage rates (since money wages increase by less than the rise in prices) and a higher employment level. An unexpected fall in the price level will have symmetrical short-run effects on real wages and employment (ibid.). But these are temporary effects, since workers will eventually repair their 'oversights' (ibid.: 204) of recent changes in the cost of living and will demand a money wage that ensures them the desired real wage at present prices, which means that the real supply curve for labour will shift back to its original position. Champernowne implicitly assumes that the real demand curve for labour is not affected by price changes, since entrepreneurs' expectations of future prices are supposed to be correct (Keynes 1936 [1973]: Chapter 5; see also Champernowne 1964: 180–181).

The real wage rate that workers would demand if they forecast future prices correctly (i.e. under Champernowne's assumptions, if the price level had been stationary) is called the 'basic real wage' (Champernowne 1936: 203) and the corresponding unemployment level is the 'basic unemployment' rate (*ibid.*). Since Champernowne implicitly assumes perfect mobility of labour and the absence of search in the labour market, his basic unemployment is the difference between the given labour force and the effective labour supply at the basic level of real wages, that is, voluntary unemployment.

Actual unemployment will be below or above basic unemployment depending on whether the price level is, respectively, rising or falling. Champernowne (*ibid.*: 204) calls 'monetary unemployment' the excess of actual unemployment over basic unemployment, and symmetrically for 'monetary employment'. He states in a footnote that the concept of monetary unemployment is 'copied from Keynes's "involuntary unemployment," but differs from that concept' (*ibid.*: fn. 1). The difference, of course, is that while Keynes's (1936 [1973]: 15) concept is an attempt to describe movements off the labour supply curve and situations of excess supply in the labour market, Champernowne has in mind points of short-run equilibrium of the labour market corresponding to different positions of the short-run labour supply curve. Furthermore, Keynes's definition of 'full employment' as a fixed upper limit described by the absence of 'involuntary unemployment' is incompatible with Champernowne's concept of 'monetary employment', when the economy is *above* the level given by the basic rate of unemployment (this is explained by rightward shifts of the labour supply curve caused by unanticipated rising prices).

Champernowne (1936) pointed out in his article that the actual rate of unemployment will diverge from the basic rate only temporarily, for workers will tend to repair their oversights of recent changes in the price level. The dynamics of this process is an important feature of his framework:

A period of monetary unemployment is likely to cause falling money-wages and...a period of monetary employment is likely to cause rising money-wages. *In so far as we can assume that rising and falling money-wages will respectively cause rising and falling real wages*, we may conclude that a period of monetary employment contains the seeds of its own destruction in the form of a tendency for real wages to rise, whereas a period of monetary unemployment has in it the seeds of its own destruction, in the shape of a tendency for real wages to fall (*ibid.*: 204; italics added).

He endorsed Keynes's (1936 [1973]: 262) view that a change in money wages, for a given state of effective demand, leads to a corresponding change

in prices without any effect on employment and real wages. Champernowne eventually played a key role in convincing Pigou that this was an essential element of Keynes's framework (see Pigou 1938: 134). In the context of his 1936 article, 'the demand of labour for a certain real wage can only make itself effective in so far as it influences the attitude of the monetary authority and its manipulation of the rate of interest' (Champernowne 1936: 204). Starting from a position of monetary employment—caused, as we saw above, by a temporary inability of workers to realize that prices have gone up—Champernowne argues that workers will demand higher money wages as their oversight is gradually repaired, which will lead to a rise in the price level in the same proportion. Workers will react by demanding another rise in money wages, which will be accompanied by another rise in prices. If real wage aspirations by wage earners are not attained in the short period, his model allows us to predict what may happen in the next short period, and so on (see also Harcourt 2001: 440). According to Champernowne (*ibid.*: 205), this steady rate of inflation will accelerate, since the 'bargaining power' of the representative worker increases as he becomes more confident in his ability to raise his money wage and, more importantly,

[t]he speed with which he will revise his demands in the face of increases in the cost of living will become greater the more accustomed he becomes to the danger of his real wage being reduced by the rise in the cost of living. We see that a period of monetary employment will be accompanied not merely by rising money-wages and prices, but moreover by *money-wages and prices rising at a rapidly increasing rate* (*ibid.*; italics added).

This is close to the famous accelerationist result of the 'natural rate of unemployment hypothesis' formulated by Milton Friedman (1968) and Edmund Phelps (1968). The only way to keep the rate of unemployment below its equilibrium or 'basic' level is by increasing the rate of inflation. As in Friedman and Phelps, the mechanism behind acceleration is the (not fully developed) assumption that workers form their expectation of price increases based on past rises in prices, that is, adaptive expectations. Furthermore, Champernowne suggests that the length of wage contracts is reduced as workers become more aware of the price increase process, which contributes to the acceleration of the inflation rate.

The acceleration process will eventually and indirectly bring about an increase in real wages, since the monetary authority 'would be forced to put a stop to it' (Champernowne 1936: 205) by means of a higher rate of interest. However, since the action of the central bank is not anticipated, there would probably

be an overshoot, with real wages rising ‘sharply’ and the amount of employment falling below the level corresponding to basic unemployment (cf. Friedman 1968: 8–9 for essentially the same remarks about overshooting). The argument for the case of monetary unemployment is similar: the process of falling money wages and falling prices ‘is likely to become accelerated as labour becomes more disorganised by the depression, and as employers get more desperate and more confident in their power to cut money-wages’ until eventually ‘the lunacy of the situation will be realised’ (Champernowne *ibid.*: 205) and the monetary authority puts an end to the deflationary process, which leads to a reduction of the real wage rate.

The upshot is that periods of monetary employment or of monetary unemployment are not likely ‘to last for very long’, so that actual unemployment oscillates around the level of basic unemployment, which corresponds to the ‘trend value’ of unemployment (*ibid.*: 206). The length of depression periods tends, however, to be longer than for boom periods, since people are usually more sensitive to the threat of hyperinflation than to the prospect of a slump, which is reflected in the slower effects of deflations on the monetary policy of central banks (*ibid.*: 205–206). Champernowne (*ibid.*: 206) suggests that the ‘ordinary tools of classical analysis’ could be used when trend variables are the object of investigation and the assumption can be made that the amount of employment is not affected by the price level (*ibid.*: 206–209). The main tool of classical analysis is the ‘real supply curve of labour’ which may be a useful concept to estimate the trend of unemployment and of the interest rate, ‘provided that the monetary authority does not allow labour to be misled by too long periods of rising or falling cost of living’ (*ibid.*: 216). Classical analysis is not sufficient, however, if the rate of interest—because of scarce investment opportunities or increased liquidity preference caused by the fragile expectations of entrepreneurs—cannot be reduced by enough to avoid falling prices and monetary unemployment, which is Champernowne’s (*ibid.*) version of the so-called liquidity trap that would be later associated with Hicks’s (1937) investment saving–liquidity preference money supply (IS–LM) analysis (cf. Champernowne 1964: 191), where he takes into account the real balance effect but dismisses it as empirically small.

Champernowne used diagrams to illustrate his concepts, but he restricted diagrammatic analysis to the discussion of comparative statics, without any attempt to incorporate a diagrammatic representation of dynamic processes involving endogenous changes in expectations. Nevertheless, diagrams are especially useful to illustrate Champernowne’s suggestion (1936: 212) that the key difference between Keynes and the classics is that the former postulates that the supply of labour is also influenced by the money wage rate,

not just by the real wage (see also Darity and Young (1995: 15–19), for a discussion of Champernowne's equations and diagrams, and a comparison to the Hicksian IS–LM formulation). Another important difference between the classical and the Keynesian systems is, according to Champernowne (1936: 211–212), the impact of 'general nervousness, the state of the news' and price-level expectations on the demand for money and for investment in *The General Theory*, which he would stress even more in his 1964 reassessment. In the classical scheme, the starting point is the labour market with supply and demand curves in real terms, while the Keynesian scheme starts with the determination of the rate of interest, which decides the position of the 'demand for saving'. Given the employment level and the corresponding real wage, 'labour's demand' curve determines the money wage together with the vertical 'employers' offer' curve. The slope of labour's demand curve reflects the degree of money illusion of workers; it would be vertical if labour supply depended only on real wages (see *ibid.*: 215).

Champernowne's diagrams bring out the different 'logical structures' underlying the classical and Keynesian analysis (*ibid.*: 209). Hence, 'whereas the classical system of analysis was to deduce the levels' of the endogenous variables 'by considering in turn the demand and supply of labour, saving, and money, the Keynesian system is just the opposite, namely to consider the demand curves and supply curves for money, saving, and labour' (*ibid.*: 212). Like Pasinetti (1974: 43–46), Champernowne deployed a system of equations of a causal type as opposed to a completely interdependent system of simultaneous equations in the manner of IS–LM. In particular, Champernowne (1936: 211), in contrast to Hicks (1937), described the influence of income on the money demand function as an 'indirect effect' only, which allowed him to emphasize the 'direct effect' represented by the influence of the rate of interest and expectations.

In his attempt to clarify Keynes's theory of employment, Champernowne put forward the notion of a short-term trade-off between inflation and unemployment that would later be associated with the Phillips curve literature. As pointed out by Champernowne (1936: 201), under conditions of falling money wages in a depression, 'a situation must eventually arise in which the monetary authority takes action to check any resultant fall in prices, and so makes effective the attempt of the unemployed to accept a lower real wage'. However, as Champernowne (*ibid.*: 216) was aware, it might be impossible 'to lower the rate of interest sufficiently to cause sufficient investment to keep prices and money-wages from falling' if pessimistic expectations of returns and general uncertainty prevailed. Instead of Keynes's (1936 [1973]) taxonomy of voluntary, frictional, and involuntary types of unemployment, Champernowne introduced the notions of mon-

etary employment and monetary unemployment. In Champernowne's short term, the labour market clears only temporarily, since money wages will change in the next period as workers adjust their price expectations, until real wages correspond to their expected level in the long term and unemployment is accordingly at its basic or trend level. Champernowne's interpretation of labour market dynamics in the 1930s has clear implications for the formulation of economic policy. Although Champernowne (1936) did not explore this, one of the corollaries of his analysis is that a rate of unemployment below the basic rate is not a feasible goal of economic policy because it is associated with accelerating inflation.

3 Income Distribution and Stochastic Processes

Pareto's Law of income distribution had attracted critical attention at Cambridge at least since its rejection by Pigou (1912; see also McLure 2013). In 1935–1936, three articles about the Law came out in the *Economic Journal*, then under Keynes's editorship, reflecting a new wave of interest in it (see Persky 1992). This may explain Keynes's suggestion (apparently in disagreement with Pigou's opinion about Pareto's Law) to Champernowne concerning the subject matter of his 1936 Fellowship dissertation. Pareto (1897 [1964]) plotted the cumulative distribution of income for a few countries on double logarithmic paper and claimed that in each case the result was a straight line with about the same slope. He argued that the distributions could be described by the curve $\log N = A - \alpha (\log x)$, where N is the number of households with income greater than x , A is a parameter, and α is the absolute value of the slope and an indicator of inequality (the larger α the less unequal the distribution). Pareto stressed the asymmetric character of the curve and its divergence from a normal curve of distribution (see e.g. Persky *ibid.*). From the beginning, Pareto's Law became an inductive proposition in search of a rational basis. Champernowne's dissertation at King's, followed by his 1953 'A Model of Income Distribution', was the first to set up an explicit dynamic model of social mobility which could explain Pareto's Law (Chipman 1976: 138–140).

As explained by his former Cambridge colleague Hashem Pesaran (2004: 212), Champernowne used a Markov process to characterize the transition probability of an individual in one income class to the adjacent income classes, from one period to the next. Assuming that probability of shifts downwards and upwards along the income ladder is reasonably constant both across time and across income groups, Champernowne was able to derive the exact Pareto

distribution as the limiting property of his stochastic model, thus providing a neat theoretical explanation for Pareto's empirical finding that the frequency distribution of wealth, W , is proportional to a power of wealth, W^{-1} , at least for relatively large values of W .

In the Markov chain each year's income depends only on the previous year's income plus a random increment proportional to last year's income. Champernowne's probabilistic model of the income-generating process assumed that the probability of moving up one bracket is less than the probability of moving down one bracket. This is a necessary and sufficient condition for the existence of a limiting (equilibrium) distribution with a characteristic shape, and, moreover, it is consistent with Pareto's theory of the 'circulation of the élites' (Chipman *ibid.*).

Champernowne's 1953 distribution model has been discussed in detail by Josef Steindl (1965) as part of his account of stochastic processes and growth (see also Steindl 1987; Corsi 2012). As pointed out by Steindl (*ibid.*: 809–810), Champernowne's approach explains the characteristic pattern of income distribution amongst individuals 'as the steady state of a stochastic process which has been evolving in time, so that the pattern reflects something which has been going on in the past'. Such an approach establishes a relation between the stratification found in a cross section and the past history which has generated it, and which is mapped in the cross section. 'This is analogous to the stratification in geology or the rings in the trunk of a tree. Irregularities or shifts in the empirical distributions can according to [Champernowne's] view be explained by major disturbances of the process in certain points of time in the past' (*ibid.*). Biological analyses were, of course, part of Champernowne's Marshallian background (see also Corsi 2012: 157, fn. 15). He was primarily interested in the forces that change the incomes of different individuals and determine the long-term income distribution, whatever its original profile. If incomes are eternal—because personal and property income sources are transmitted from father to son—observed distributions may be regarded as approximations to this long-term equilibrium, so that the cumulative effect of random past changes dominates the original distribution of income sources (see Lydall 1974). Champernowne clarified his core ideas in a couple of passages added to the 1973 version of his King's dissertation:

The properties of the income-distribution are being moulded all the time by the forces of change. Under their action, the properties of the distribution move towards equilibrium, but never reach it, because they are shaken away by impulses of change, and also because the forces of change are themselves smoothly altering (Champernowne 1973: 9–10).

Champernowne (1978) compared his stochastic model to other approaches in his contribution to a collection about the state of research on income distribution and inequality in the 1970s.

Champernowne's stochastic framework is compatible with Keynes's notion of fundamental uncertainty (Corsi 2012: 162). Indeed, Champernowne (1973: 188) called attention to the similarity between his description of the income receivers as a party of gamblers at cards and Keynes's comparison of the operations of the stock exchange to those of a casino. Another echo of Keynes's *General Theory* is visible in Champernowne's (1973: 76) remark that in order to discover the forces determining the nature of the distribution of income—that is, the nature of the distribution of all 'qualifications for obtaining incomes', comprising inborn ability, education, and property—it is necessary to study 'carefully these links between past and present distribution that are provided by this tendency for the aggregate value of qualifications to preserve a continuity'. There are many links between 'the income of a father and of his son and between the income of a man and of his heir'. As observed by Champernowne (*ibid.*: 1–2), those passages reflected an indirect influence of Keynes's reference to capital as the link between the present and the future.

With the recent resurgence of interest in income inequality, Champernowne's original stochastic model—and its further elaboration within the context of economic growth by Stiglitz (1969; written during Stiglitz's academic year in Cambridge in the mid-1960s)—has attracted some attention. As pointed out by Thomas Piketty (2015: 74–75), Champernowne (1953) started a family of models in which shocks to the wealth trajectory of households can contribute to making the wealth and income distribution very unequal. They might be demographic shocks, shocks to the rate of return on capital, shocks to labour market outcomes, and so on. If shocks take a multiplicative form, Champernowne and Stiglitz showed that 'in the long run the inequality of wealth will converge toward a distribution that has a Pareto shape for top wealth holders', which is approximately the form observed in actual distributions and corresponds to large concentrations of wealth at the top (*ibid.*). According to Piketty, such models also established that the inverted Pareto coefficient is a steeply rising function of the gap between the rate of return r and the rate of economic growth g . Piketty claimed that the effect of $r-g$ on inequality comes from its dynamic cumulative effects in wealth accumulation models with random shocks à la Champernowne. These similarities between Piketty's and Champernowne's frameworks may also explain their shared emphasis on the role of hereditary in income and wealth distribution through time (see Champernowne 1973: Chapter 15).

After retirement from Cambridge, Champernowne's main project was to complete his 1998 textbook on *Economic Inequality and Income Distribution*. This is well described by his co-author Frank Cowell (2004: 1,016). The volume

brought together several [of Champernowne's] lifelong concerns: the questioning of fundamental assumptions underlying market-oriented theories of production and distribution, the application of mathematical modelling to the analysis of income distribution, the desire for theoretical rigour in explaining the fundamentals of income inequality (following his *Economic Journal* article of 1974), the application of ingenious numerical [computer] methods to solve analytically intractable problems...and the concern for distributive justice.

Champernowne (1974) had argued that there was no one 'best coefficient of inequality', but different coefficients suited to reflect different aspects of inequality. He showed formally the sensitivities of six distinct indexes to different types of inequality and warned that 'the choice of index could quite frequently decide the answer to such questions as whether inequality has increased or decreased in a country over a decade' (ibid.: 807; see also Presley 2013). This was the topic of Chapter 4 of the 1998 volume. Chapter 11 presented stochastic models of joint wealth and income distribution, a subject first tackled by Champernowne back in 1936. The book filled a gap in the literature at the end of the twentieth century, as reflected in Piketty's (2000: 462) assessment that it 'is a great textbook on income distribution' that 'covers basically all the topics one can think of' in the field.

4 Production, Capital, and Growth

According to Champernowne (1945–1946: 10), 'the supreme merit' of von Neumann's (1945–1946; originally written in German in 1937) article about balanced growth 'lies in the elegance of the mathematical solution of a highly generalised problem in theoretical economics'. Champernowne saw his role as translating von Neumann's argument—written from the perspective and in the style of a mathematician—for economists, who, in contrast with mathematicians, were not used (in the 1940s) to reduce explanations to a minimum and to state assumptions as concisely as possible. Hence, whereas to the mathematician the most interesting part of von Neumann's essay is the proof of existence of at least one equilibrium position, the economist is mainly interested in the properties of the system when it is in equilibrium (Champernowne ibid.: 11).

Champernowne's clarification of such mathematical properties led the way to interpreting von Neumann's general equilibrium growth model as rooted

in the tradition of classical political economy (Harcourt 1972: 206–207; Kurz and Salvadori 1995: 405–407, 421–423). In contrast to the ‘more traditional approach’, relative prices are determined in von Neumann’s model by supply conditions alone and not by the preferences of consumers (Champernowne *ibid.*: 12, 17). Champernowne advanced a view of the model as a growing (classical) system in which wages are kept at subsistence level, constant returns to scale prevail in all activities, labour supply is perfectly elastic, land is not scarce, and population is constant. The surplus is entirely in the form of profits, all of which is saved and reinvested. The production processes consist, from this perspective, of goods producing goods, where some are the wage goods consumed by workers. As pointed out by Champernowne, von Neumann’s assumptions lead to his conclusion that the rate of profit (or interest) is equal to the rate of economic growth, which depends on the rate of expansion of those goods that can be expanded least rapidly. Champernowne admired the model, but also provided criticisms concerning the unrealism of some of its assumptions, such as: (i) the requirement that every process requires as an input or produces as an output some amount, no matter how small, of every good; (ii) the absence of consumption, so that labour only gets amounts of goods as necessary inputs for production processes; and (iii) the requirement that overproduced goods should be free (see also Thompson 1987).

Champernowne’s ‘classical’ interpretation of von Neumann’s model has been challenged by a number of economists, who prefer to locate it in the tradition of Cassel’s (1918 [1932]) general equilibrium system of equations. Samuelson (1989: 100), for instance, despite praising Champernowne’s ‘brilliant interpretation for economists of the model’s mathematics’, criticized him for asserting that preferences play a minor role (if any) in the determination of equilibrium prices and for having fallen victim to ‘Kaldorian influences’ and ‘Sraffian dogmas’ (*ibid.*: 106). The matter has been examined by Kurz and Salvadori (*ibid.*), who provide textual evidence in support of Champernowne’s interpretation. Kurz and Salvadori have suggested the existence of what they call a ‘Champernowne connection’ (*ibid.*: 421) between Sraffa’s (1960) analysis and that of von Neumann. In his attempt to elucidate von Neumann’s model, Champernowne (1945–1946) advanced several concepts and raised issues which would be found again later in Sraffa. He used the Sraffian notion of a ‘system of production’ where all goods help to produce other goods, that is, are ‘basics’ in Sraffa’s terminology. Champernowne’s interpretation of the rate of interest as decided by technical processes of production, as well as its inverse relation with the real wage rate, also anticipated aspects of Sraffa’s framework. Indeed, Champernowne (*ibid.*: 10, fn. 1) acknowledged discussion with

Sraffa while preparing his introduction to von Neumann. As Kurz and Salvadori (2001) have documented in detail (on the basis of Sraffa's diaries), Champernowne and Sraffa met regularly during the very long gestation of *Production of Commodities by Means of Commodities*, in the 1940s and especially the 1950s. In a preliminary version of the Preface, drafted in 1959, Sraffa thanked Champernowne amongst his 'mathematical friends' (Kurz and Salvadori *ibid.*: 254) who had been consulted throughout the book's elaboration. But, for reasons that are still unknown, he dropped Champernowne's name from the published Preface. Anyway, Sraffa's diaries make clear that Champernowne read and commented in the 1950s on the analytical structure of Sraffa's famous book.

Champernowne's main contributions to production and capital theories are contained in his extended comment on Robinson's (1953–1954) well-known article about the production function and the theory of capital. As put by Harcourt (1972: vii), 'a thorough understanding of Champernowne's (1953–1954) comment...illuminates, as no other preparation could, the basic points of the subsequent controversies in the double-switching debate' that dominated what Harcourt dubbed the 'Cambridge controversies in the theory of capital'. However, despite the fact that Champernowne (1953–1954) was the first to discuss double-switching and capital-reversing phenomena, using the same model later deployed by participants in the Cambridge controversies in the mid-1960s, it remained with no or little influence, which prompted Harcourt's (1969: 372–373, 1972: 29–34) quoted remark and careful account of that article's contributions. Jack Birner (2002: 4, 38–41, 53–54, 168–169) has examined how Champernowne's results were independently 'rediscovered' in the Cambridge controversies, particularly his proof that a specific hidden assumption in neoclassical production theory was the cause that these phenomena (called 'anomalies') had not been predicted by the orthodox model. Champernowne clarified that the anomalies were only so if a comparative static model was used to represent processes in time, a notion that would only start to become familiar in the 1970s.

As is well known, Joan Robinson (1953–1954) complained about the ambiguity concerning the unit of measurement of capital in the neoclassical aggregate production function, the disregard of factor supplies and technical progress in the explanation of distribution and growth, and the ambiguities in the neoclassical concept of equilibrium (see Prue Kerr's chapter on Joan Robinson in this volume). The major puzzle was to find a unit in which capital could be measured, independent of distribution and relative prices. Her solution was to measure capital in terms of wage units. In the process, she noticed the phenomena of double-switching and capital-reversing, but regarded them a 'curious

possibility' (ibid.: 106), 'apparently paradoxical' (ibid.: 96) and 'perverse' (ibid.: 94). In the end, Robinson did not insert them in her critique of orthodox neo-classical capital theory. Double-switching is associated with the possibility that the same method of production may be the most profitable at more than one rate of profit; capital-reversing means the value of capital moving in the same direction as the rate of interest. They imply that the same (physical) capital goods will have more than one value (see Harcourt 1969, 1972).

Champernowne (1953–1954) objected to Robinson's solution to measure the quantity of capital in wage units, since it cannot guarantee a strict correspondence between output and capital, which prevents obtaining the rewards of factors of production by partial differentiation of the production function. More importantly, it brings about two anomalies: (i) the same physical capital may have a different value as between two equilibrium positions that differ 'only' (ibid.: 112) with respect to the wage and interest rate (the production function is not single-valued); and (ii) output and capital employed per worker may be negatively correlated. Champernowne's criticism is that Robinson's unit of measurement entails a negative bias due to the fall in interest rate, called 'interest effect' by her. In order to avoid this 'index number problem' (ibid.), Champernowne constructs an alternative chain index measure that links the amounts of capital in equilibrium positions. A 'natural method', argues Champernowne (ibid.: 115; italics in original)

by which to construct an index of quantity of capital in a historical sequence would be to form a chain index, increasing the index at each step by the proportion in which the cost of capital at current wage and interest rates at the end of the step exceeded the cost of capital at the beginning of the step, calculated at the same wage and interest rates. By shortening the steps, the distortion due to choosing wage and interest rates at the *end* of each step could be made as small as we please.

Hence, the amounts of capital in all equilibria are multiples of that measure (see Harcourt (1972: 29–34) for the mathematical argument).

However, Champernowne observes that a model using his chain index is not in fact free of the anomalies it was devised to circumvent as the production function may still be multiple-valued: 'Contrary to intuitive expectations, our assumptions do not ensure that a graph [of the production function] is a single-valued curve sloping upwards to the right' (Champernowne 1953–1954: 118). Champernowne's Figure 1 shows that as the interest rate falls, the same value of the capital–labour ratio is realized more than once, a phenomenon later named 'capital-reversal'. The 'further assumption' (ibid.) needed to eliminate this possibility is that of each pair of capital goods that is the most profitable at a particular rate of interest, the capital good that is the most

profitable at a lower interest rate has a higher labour productivity: ‘Under this assumption, a gradual fall in the rate of interest would entail increases in both productivity and the quantity of capital per head. But, although this may fit well with our preconceived notions, there is no logical justification for the assumption’ (ibid.).

Nevertheless, Champernowne (ibid.: 129) did not consider a multivalued production function paradoxical or problematic while dealing with stationary state equilibrium positions, when production factors receive their marginal products. Matters differ, however, if the production function is used to analyse a dynamic time sequence, which brings in the possibility of (what would be later called) ‘double-switching’ or ‘reswitching’:

The outcome of this discussion is that, although our method of measuring quantity of capital provides us with a production function satisfactory for describing the family of stationary states, formidable difficulties arise when we consider a sequence of states in time in a developing economy, unless we rule out cases in which a lowering of interest rates can cause the introduction of techniques with a *lower* productivity than those used up till then. A numerical example has shown that these cases cannot be ruled out merely on logical grounds (ibid.: 130; italics in original; see also 119).

Hence, without this assumption—that is, absence of reswitching—Champernowne’s chain index does not remove the main ‘inconvenience’ (ibid.: 130) of Robinson’s unit of measure. Champernowne concludes that, against his own expectation, it is not the measure of capital which is to blame. In the process, he discovers that all models yielding the prediction that no previously eligible production technique returns as the rate of interest changes are built on the (hidden) premise that ‘the ordering of techniques according to labour productivity is inversely related to the rate of interest that makes them the most profitable technique’ (Birner 2002: 53).

Apart from his comment, Champernowne added a mathematical note to Robinson’s article, written with Kahn (Champernowne and Kahn 1953–1954). This dealt with the effect of the rate of interest on the value of capital involved in the use of a given technique of production under equilibrium conditions, a problem first tackled by Wicksell. The Champernowne–Kahn formula was used by Robinson (1953–1954) and also in her 1956 book, where the Champernowne–Kahn article is reproduced as an appendix (see Harcourt 2001). In the Preface to *The Accumulation of Capital*, Robinson (1956: vii–viii) thanked Champernowne for helping with ‘the heavy artillery of his mathematical expertise’ and in the formulation of the theory of accumulation in a given state of technical knowledge.

Champernowne (1958, 1961) carried into his growth models the view that aggregate production functions—constructed according to his chain index—are warranted so long as their use is restricted to a comparison of a set of alternative possible stationary states. But as

soon as we depart from such a comparison of stationary states, the innocent-looking assumption [of a reduced production function] begins to land us in difficulties. The production function, like Alice's flamingo, will not keep still but wriggles about and regards us with an injured expression so soon as we attempt to use it (Champernowne 1961: 223–224).

Champernowne claimed that provided accumulation is 'slow' (ibid.: 224) and capital goods are not very durable, the errors caused by using the reduced form of the production function should be of the 'second order' (ibid.) only. In any event, Champernowne's cautious attitude is well illustrated by a passage in a new chapter added to the 1973 edition of his King's dissertation. Champernowne (1973: 188) recanted his 1936 discussion of income from particular qualifications, based on the 'crudest marginalist assumptions, which almost no reputable economist would take seriously today'. He referred to his original claim (ibid.: 70) that 'accumulation is a force tending to lower the rate of interest, by lowering the relative value of those qualifications which can be constructed'. Champernowne remarked that 'to claim this is Cambridge, England, today would be tantamount to suggesting that capital accumulation reduces the rate of profit, a lapse which would invite banishment to Cambridge, Massachusetts' (ibid.: 188).

Champernowne's (1961) Corfu conference paper introduced into the literature the first growth model in which technological change takes the form of machines ('robots') replacing tasks formerly performed by labour. Champernowne went further than models of bias of technological change towards capital versus labour: 'Capital accumulation takes the form not of the gradual modification of production techniques to embody more and more machinery, but the one-for-all mechanization in turn of each of a great many different processes' (ibid.: 225). Champernowne's approach has been adopted by some growth theorists, who refer to his 1961 paper in that connection (see e.g. Acemoglu 2010; Zeira 1998). By assuming that robots (producing consumption goods) are made by men alone and their efficiency is unimpaired forever, 'we side-track all the difficulties of measurement of capital. We can, without a blush, use J.R. [Joan Robinson] units of capital' (Champernowne ibid.: 227). At any one time, the degree of mechanization determines the rate of interest, which in turn gives the proportion of income saved. This determines the rate of investment, the rate of accumulation, and the rate of growth.

Interruption of economic growth ('stagnation') (ibid.) occurs if the margin of mechanization reaches its limiting position as the cost of robots increases and the rate of interest falls. However, if there is population growth, the demand for robots increases. Moreover, a lower rate of interest (or rate of profit) is accompanied by a fall in the propensity to save, with an ensuing surge of workers from the robot-making sector to the consumption sector. If the population growth rate is sufficient, a dynamic equilibrium may be reached with profit just low enough to keep savings down to the level required to balance the need for new robots. The model assumes that eventual stagnation is not foreseen, otherwise the demand for new robots would be reduced at an earlier date (ibid.: 228). As pointed out by Champernowne in the recorded discussion that followed his presentation at Corfu, the model is 'dominated by savings, not investment ... To illustrate the real world, one would have to bring in...the demand for investment goods' (Hague 1961: 377).

The conditions for the maintenance of full employment along the growth path were discussed by Champernowne (1958) in a model that adopted a Kaldorian saving function, but otherwise differed from Kaldor (1957) in many aspects. Kaldor (ibid.: 591, fn. 1) had acknowledged his debt to Champernowne, particularly in the development of the mathematical argument. Instead of Kaldor's full-employment assumption, Champernowne (1958) investigated the mechanism by which the supply of savings may be utilized for increasing productivity in such a way as to allow employment to remain full and avoid a stagnation crisis. He made clear that his results differed from many of Kaldor's in that 'we do not find any reason to expect an easy passage to an age of uniform progress with a constant capital-output ratio and a constant rate of growth of productivity' (ibid.: 233). Champernowne (1971) would come back to the mathematical properties of Kaldor's model (which he had helped to set up), this time with a focus on the stability of its steady growth path. He then established the model's stability if reformulated in terms of non-linear differential equations in continuous time. The stability conditions are shown to be more stringent than the limits prescribed by Kaldor (see also Gandolfo 1980: 506–516).

Another area of growth economics that attracted Champernowne's attention was optimal growth. He was one of the co-discoverers of the 'golden rule' that consumption along a balanced growth path is maximized if the rate of interest is equal to the rate of growth, which he demonstrated in terms of the condition that the savings rate must equal the rate of profit (or interest) (Champernowne 1962). Champernowne's interest in this issue may be traced back to his study of von Neumann's model in the mid-1940s. He also investigated the matter from the perspective of Ramsey's famous essay on saving (see Pedro Duarte's

chapter on Ramsey in this volume). As reported by Robertson (1958: 79; see also Hicks 1965: 258), in an unpublished paper Champernowne deduced the following formula for the proportion of income saved:

$$s = \eta \left(1 - \frac{p}{r} \right),$$

where η is the elasticity of the agent's marginal utility function, p is the rate of discount, and r the rate of interest. Ramsey's results can be derived from Champernowne's formula: (i) if there is no time preference, then $s = \eta$, so that the proportion of income saved along the optimal path is independent of the rate of interest; and (ii) if p is positive, saving will be zero when $r = p$, while if $r > p$, the propensity to save will increase with r , up to a maximum at $s = \eta$.

5 Expectations, Uncertainty, and Decision-Making

When Robert Lekachman put together in 1964 a *Report of Three Decades of Keynes's General Theory*, he reproduced reviews and obituaries of Keynes originally published in the 1930s and 1940s, together with new contributions in which authors were asked to give their present evaluations of the Keynesian revolution. Champernowne (1964) chose the theme of expectations, which he saw as central to Keynes's research programme and hitherto (i.e. in the early 1960s) relatively unexplored. As observed by Don Patinkin (1990), interpretations of Keynes's central message as focused on uncertainty and expectations started to appear in the early 1960s only and in particular in books and essays by Shackle. Champernowne's (ibid.: 175) 'revisiting' of *The General Theory* meant a suggestion to read the text with an eye to the role of expectations as a link between the economic future and the present, with no effort to take into account 'more recent attempts' to interpret and improve upon Keynes's ideas in that regard (see Champernowne's 1963 review of Shackle, which is critical of the latter's theory of 'degree of beliefs' as a basis for the theory of decision-making).

Champernowne (1964) may be seen as one of the first comprehensive accounts of Keynes's discussion of the effects of expectations about the future on present economic behaviour. As pointed out by Champernowne, it is through the concepts of marginal user cost, marginal efficiency of capital, and liquidity preference for money that opinions about the future affect current economic decisions in Keynes's framework. The Marshallian concept

of marginal user cost allowed Keynes to bring in the influence of long-term expectations on firms' pricing policy. The marginal user cost includes all extra cost connected with the firms' long-term prospects, as it corresponds to the consequent loss of capital equipment at the end of the period plus extra expenditure during the period on the product of other businesses. The value of capital equipment is related not to its replacement cost but to its prospective yields. Keynes's definition applied not only to the effect of extra output on the dates in which the equipment would need replacement but also to any effects on the prospective markets for the firm's goods. Champernowne (*ibid.*: 178, fn. 4) recalls that such notions were part of Cambridge's oral tradition, as illustrated by Shove's lectures on the theory of value at King's College in 1934–1935. Expectations also play an important role in Keynes's treatment of the aggregate demand function as part of the formulation of the principle of effective demand. Champernowne (*ibid.*: 180), however, finds problematic Keynes's shift, in the middle of Book II of *The General Theory*, to actual 'ex post' values, as it suggests a greater stability to the model than can in fact be claimed. Under the definition of the propensity to consume as composed of the sum of a set of separate functions determined by the firms' expectations of expenditures on their products (as a function of the known level of employment), it is liable to oscillation due to changes in each firm's prospects.

According to Champernowne, Keynes's concept of marginal efficiency of capital contained at least two novelties as compared to the traditional approach to investment. The relevant expectations are not quite those of employers planning capital investment, but private investors dealing on the stock exchange. On the other hand, Keynes's assumption of diminishing returns in the capital goods industry reduced the amplitude of fluctuations in the rate of investment and ensured that small temporary changes in the economic climate do not set off a cumulative process of 'rising' expectations and expanding activity. Expectations involved in Keynes's notion of liquidity preference cannot be described in the form of a probability distribution, but as a range of possibilities associated with such psychological states as bullishness, bearishness, and uncertainty. Moreover, as observed by Champernowne, Keynes's demand for money function clearly distinguished between variety amongst individuals in their expectations and uncertainty within each individual's own personal expectations. Chapter 17 of *The General Theory*, entitled 'The Essential Properties of Interest and Money', attracted Champernowne's attention. He regarded it as the most perplexing and theoretically deep chapter in the book. Keynes attempted in that chapter to explain why the volume of output and employment is more intimately bound up with the money rate of interest than with own rates of interest of other assets. After a detailed discussion of Keynes's involved argument, Champernowne (*ibid.*: 198–199) concluded that Keynes's case rested on

the assumption that wages would necessarily be and were expected to be more sticky in terms of money than in terms of other assets, which he found wanting. He agreed with Keynes's point that instability would follow if money wages changed in response to movements in employment, but contended that a constant expectation of a gradual drift upwards of money wages and money prices may not cause instability. On the contrary, it may allow the commodity rates of interest to fall very low (due to price expectations) even when the money rate of interest remains above that level due to liquidity preference.

Champernowne (*ibid.*: 200–201) endorsed Keynes's (1936 [1973]: 293) suggested division of economics between the theory of stationary equilibrium and the theory of shifting equilibrium where changing expectations are capable of affecting the current situation. The importance of money as a 'link between the present and the future' is only apparent when one is dealing with the theory of output and employment as a whole in shifting equilibrium. Champernowne had not always been positive about Keynes's distinction. He was 'sorry to find that in 1936 I added at this point in my copy [of the *General Theory*] the marginal note "cranky"' (Champernowne 1964: 201, fn. 30). Indeed, when he wrote his 1936 article about Keynes and the classics, Champernowne focused on the labour market, not on the inter-temporal coordination problem caused by the absence of futures markets. This changed in his 1964 revisiting of *The General Theory*:

Keynes made it abundantly clear that there was no efficient link between the decisions of employers how much to invest and the decisions of individuals how to divide their incomes between consumption and savings ... But although he did not give such a clear demonstration, he was greatly concerned also with the lack of any proper link between employers' plans regarding capacity for production of consumption goods in the future and the future decisions of individuals to spend on consumption goods. He realized that the lack of such a link would lead to violent swings in expectations and activity (*ibid.*: 201).

Keynes had much to say about how expectations about the future affect present economic behaviour, but not so much about the converse problem, that is, the effect of the present on expectations about the future. Champernowne (*ibid.*: 192–194), like others before and after him (see e.g. Hart 1947), was of the opinion that Keynes lacked a theory of the formation of expectations. Instead of setting up a detailed theory of the causation of expectations, Keynes discussed the 'conventions and habits of thought' which are behind these expectations. This remains a controversial aspect of the interpretation of Keynes's framework. It has been argued by Bateman (1996: 130–131), for instance, that Keynes assumed that agents form their expectations (or beliefs) intersubjectively by social conventions, via an extension of Ramsey's subjective probabilities.

The nature and influence of uncertainty in many areas of the economic system, as well as the methods available for proceeding in the face of it, formed the subject matter of Champernowne's 1969 trilogy, probably the first of its kind in the economics literature. He aimed at discussing the techniques for interpreting imperfect or incomplete statistical information and obtaining statistical estimates, or choosing decisions in the light of evidence (Champernowne 1969: volume 1, page v). The book dealt in detail with the general problem of how imperfect information may be used for estimation, decision-making, and statistical inference in economics. It was part of the series 'Mathematical Economics Texts' published between 1969 and 1972, edited by Arrow, Solow, Hahn, and others. Amongst other well-known titles, the series included Sen's *Collective Choice and Social Welfare* and Arrow and Hahn's *General Competitive Analysis*. The trilogy was suggested to Champernowne by Hahn, and benefited from discussions with other Cambridge economists including Reddaway, Deane, and Mirrlees, and from comments by Arrow (who visited Cambridge when Champernowne was working on volume 1). It combined theoretical, applied, and philosophical discussions of uncertainty and estimation, in typical Cambridge fashion.

Champernowne's (1969: volume 1, page 5) starting point is that the more important economic problems involving probability arise 'not directly from the "stochastic" or "chance" element in economic change, but from the ignorance and uncertainty influencing the economic decisions of individuals'. This is why the 'objective' theories of probability, so convenient in natural sciences, are ill-suited to economics, where 'personal' (or 'subjective') theories of probability—which deal with the effects of ignorance and uncertainty on the behaviour of individuals—should be deployed instead. What sets Champernowne (1969) apart from most other books in the field, as pointed out by Stone (1972) in his extended review, is the comprehensive coverage of the foundations and varieties of probability theory. Champernowne (1969: volume 1, Chapter 9) was especially careful in his account of Bayesian techniques for estimation, supposed to be the most general procedures available for revising probability models in the light of statistical evidence. Bayesian procedures are 'extremely powerful' (ibid.: 7) provided sufficient initial information on probabilities is available, which is often not the case. It is mainly for this reason that 'cruder and less ambitious special purposes statistical techniques' (ibid.) are often used, as discussed in volume 2. The last volume is concerned with the microeconomic and macroeconomic effects of uncertainty and risk aversion, including portfolio selection, and decision-making policy to prevent disasters. Interestingly enough, Champernowne (1969: volume 3, pages 2–3) provides some illustrations, taken from the Old Testament, of the ability to protect from natural disasters which can be anticipated: Noah's Ark, Joseph's buffer stock scheme, and Catastrophes associated with Moses, who 'took advantage of a foreknowledge of very unusual tidal movements to lead his people

over a stretch of dry land which was at all other times submerged'. Surely, those are not passages usually to be found in econometrics textbooks. But, then again, Champernowne was one of the 'most colourful' economists of the last century.

6 Conclusion

Champernowne's research agenda overlapped significantly with the Cambridge approach to economics, in its various manifestations. His contributions reflected the several dimensions of Cambridge economics, from welfare economics to macroeconomics, distribution, growth theory, economic statistics, and applied economics. His intense participation in Cambridge economic academic life (even during his Oxford period) is well illustrated by his insightful obituary note about Pigou, regarded by him as one of the founders of Cambridge economics, together with Marshall and Maynard Keynes. According to Champernowne (1959: 264), had Pigou written the *Principles of Economics* or *The General Theory*, they might have been 'less attractive works, but there would have been far less ambiguity left for lesser economists to resolve'.

One should bear in mind, however, that Champ (as we may call him at the end of this overview) was his own man, very independent, who always called things as he saw it. His critical comments on Keynes's employment and Joan Robinson's capital theories, respectively, are cases in point. According to the Cambridge oral tradition, he was meant to be co-author with Kaldor of the latter's well-known 1957 *Economic Journal* article, but he saw its fundamental flaw; the article was single-authored and Champernowne (1958) wrote a separate piece. Some of his long-lasting research topics (like income distribution and probability) were decided relatively early in life. However, he never returned to the analysis of labour market dynamics introduced in his 1936 article. As Champernowne acknowledged, that piece was written after conversation with Robertson, who would develop the theme further in his own writings (see the chapter on Robertson by Boianovsky and Goodhart in this volume). Champernowne saw the economic world through mathematical eyes, searching for logical connections and testable hypotheses. In a methodological paper about mathematical economics, Champernowne (1954: 369) quoted positively Marshall's famous remark that 'there is no room in economics for long trains of deductive reasoning'. However, despite Marshall's comment, Champernowne argued that 'economic theory which is not rigorously set out can suggest false conclusions' (ibid.: 370). Economic models, provided they can be applied to particular situations and used for economic policy, are the only way that economic arguments can be effectively developed, as Champernowne tried to accomplish in his own work.

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35

William Brian Reddaway (1913–2002)

Ajit Singh

1 Introduction

This is an essay on the life and times of Professor William Brian Reddaway,¹ a most distinguished and influential Cambridge economist. The following is not merely a tribute to Reddaway, but a means of learning from his outstanding contributions to teaching and research in economics. His distinct and practical philosophy towards teaching applied economics has long been a part of the Cambridge oral tradition and was particularly marked during the period of the late 1960s to the early 1990s. Reddaway would frequently say that his standard for the references he wrote for his students and colleagues was to always tell the truth. That same stringent criterion is used in this assessment of Reddaway's own contributions.

Reddaway was a Cambridge economist of the highest pedigree. As an undergraduate at Cambridge, he was supervised by Gerald Shove, Richard

Ajit Singh, who authors this analysis of Reddaway's work, regarded him first as a mentor who later became a good friend. The author would like to convey his deep gratitude to Shachi Amdekar for many helpful discussions on the subject. He is also grateful to Professor Martin Weale, Director of the National Institute for Economic Research, for his arguments which helped to change his mind on some of the important subjects discussed. However, he alone is responsible for any errors which remain.

¹ Always referred to as Brian Reddaway within close circles.

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Kahn, and by John Maynard Keynes. The next section of the chapter describes Reddaway's early academic career from the publication of his book on the Russian economy at the age of 22, to his appointment as the Director of the Department of Applied Economics (DAE) in Cambridge in the mid-1950s. Keynes had helped to establish the DAE as a department within the Faculty of Economics that encouraged and facilitated staff to undertake applied research. In his role, Reddaway was preceded by Richard Stone, who had been the first Director of the DAE. The six sections that follow will be concerned primarily with Reddaway's enduring contributions and influence in research, teaching and examining, and economic analysis. An important analytical issue discussed in an early section of the chapter is the question of Reddaway's influence and why he did not get as much attention from the economics profession as did, for instance, Stone, who won the Nobel Prize in Economic Sciences in 1984. Later sections focus on Reddaway's influence on teaching in Cambridge, and his major contributions to economic thinking, including corporate finance, attitudes to econometrics and mathematical statistics, and his work on the Indian economy.

2 Reddaway's Early Life and Career

An important observation about Reddaway's early academic career is that he was always an outstanding student. Following his education at Oundle School, he won a scholarship to read Natural Sciences at King's College, Cambridge. However, he was persuaded by his Tutors to study for Part I of the Mathematical Tripos instead of chemistry, and he was the only student to achieve a First Class for that examination. Rather than continuing with Part II of the Mathematical Tripos, in his final two years in Cambridge, Reddaway studied economics. This was motivated, as he explains in his reflective essay 'Recollections of a Lucky Economist' (Reddaway 1995), by his concern with mass unemployment and the 'desire to understand why the world was suffering from "poverty in the midst of potential plenty"' (ibid.: 3) which prevailed in Britain during the early 1930s. Reddaway admitted that his multidisciplinary academic trajectory, from the sciences to mathematics to economics was beneficial to his studies on many occasions (see ibid.).²

²His contributed chapter entitled 'The Chemical Industry' for *The Structure of British Industry* (Burn 1958) serves as a prime example of how Reddaway brought to significance his diverse academic interests and background.

Reddaway's academic career began when he was still an undergraduate in Cambridge. He visited Moscow in 1934 as an expert to advise Gosbank, and with the approval of the Bank of England (for whom he had arranged to work upon graduation), he conducted independent research about the Soviet financial system. After submitting an account of his experiences in Russia, he won Cambridge University's coveted Adam Smith Prize. He sent a copy of his winning essay to Keynes who liked the work and recommended it to Macmillan for publication. The book, *The Russian Financial System* (Reddaway 1935), enjoyed a long shelf life. It could still be found on London School of Economics reading lists 30 years later.

Reddaway came from a top Cambridge academic family, and benefited from growing up within Cambridge and the consequent familiarity of the Cambridge milieu. His father, a historian and a Fellow of King's College, was the Censor (head) of Fitzwilliam House before that institution was granted college status in 1966. This connection with Cambridge gave young Reddaway enormous self-confidence and opened for him many doors, which otherwise might not have been so open. After graduation, Reddaway was recommended by Keynes to go and work with a close friend of his, Australian statistician and political economist Professor Lyndhurst Falkiner Giblin, who had just been promoted to be a member of the policy making committee of the Australian central bank. From 1936 to 1938, Reddaway spent two extremely successful years in Australia. During this period, he wrote a celebrated review of Keynes's *General Theory*, which is still regarded as one of the best on the subject. He also had an opportunity to testify as an expert independent assessor in a trade union case demanding the revision of the Australian wage-fixing system³ at the Commonwealth Arbitration Court in 1937. Reddaway managed to persuade the judge to give workers a pay rise instead of a wage cut which the employers demanded. This award came to be known as 'The Reddaway' and served the country for 15 years.

On his return to Cambridge from Australia in 1938, Reddaway was appointed to a Fellowship at Clare College and to a Lectureship at the Faculty of Economics. Around this time, he also married Barbara Bennet, who was a great organiser and a close companion of Brian.⁴ The Reddaways were not extravagant people and spent their money with care, but took many family

³The Australian system of wage fixing had consisted of a general minimum wage which would be adjusted quarterly based on the retail price index.

⁴There is an excellent short introduction to the relationship between Brian and Barbara compiled by Reddaway's four children, entitled *Brian Reddaway (8 January 1913–23 July 2002) and Barbara Reddaway (15 August 1912–15 September 1996) – Memories*, edited by their son Lawrence Reddaway (Lawrence Reddaway 2003).

holidays both in the UK and abroad. Upon the outbreak of the Second World War, Reddaway served with the Board of Trade and was responsible for introducing the clothes rationing system in the country. He also acted as UK representative in a British, American and Canadian investigation into 'The Impact of War on Civilian Consumption' (1945), and aided with the post-war development of the Census of Production and the Census of Distribution (see Reddaway 1995). He also helped as an advisor to many governments of British colonies (including Cyprus in 1949 and Sierra Leone in 1955) to establish consumer price indices.

After the war, Reddaway returned to Cambridge and took a full and active role in College and University teaching as well as in University administration. He was popular with the students and was regarded as a brilliant teacher in the Faculty of Economics. He changed the nature of teaching economics in Cambridge and made it much more of a subject in applied economics, following in the footsteps of the legendary Cambridge economist, Alfred Marshall, who also favoured applied economics to economic theory.

Alongside gaining influence within the department, Reddaway was also appointed to some significant positions within the University hierarchy such as being a syndic of Cambridge University Press. At the same time, his qualities and merit began to be recognised outside the University in the highest government circles. He was sought after by governments for appointments to Royal Commissions such as that on the newspapers and the press, the Prices and Incomes Board, and other similar bodies. He was also greatly in demand by many foreign governments as well as international agencies. At one time, he served as the Director of the Organisation for European Economic Cooperation's Economics Department in Paris and made many foreign friends who still remember his Directorship with pleasure. He was a multilingual economist fluent in French and German. He also learnt Spanish when he was to go to Argentina as an advisor to the government. However, despite all this public recognition, Reddaway's primary focus remained on teaching, research, and examining for the Faculty of Economics in Cambridge.

3 Reddaway and Stone

As noted earlier, Reddaway was appointed as the Director of the DAE in 1955 to replace Richard Stone who was given the newly created Chair in Accounting and Finance. Reddaway held this position until 1969. Just as his term as the Director of the DAE was coming to an end, he was elected

to become Professor of Political Economy in Cambridge (to succeed James Meade, who had just retired). The Professorship of Political Economy is the most senior Chair in the Faculty of Economics at Cambridge. Reddaway held this position until 1980, when he formally retired. Nevertheless, he continued his association with the Faculty for many years, sometimes giving lectures, or even occasional courses. It was only failing health towards the end of his life that prevented him from continuing.

Until his formal retirement, Reddaway had thus held the two most senior leadership positions in the Faculty: Director of the DAE and Professor of Political Economy. Given the prominence and success of the roles Reddaway occupied, it is interesting to note that although his legacy and influence within Cambridge remains enormous, his impact on academic economists outside of Cambridge was rather smaller, particularly outside the UK. We might, for instance, analyse why Reddaway did not get as much attention from the economics profession as did, for instance, Stone. In retrospect, the appointment of Reddaway to the Directorship of the DAE caused some controversy. Friends of Stone regarded it as a coup against Stone organised by the Cambridge Keynesians who were critical of Stone's leadership of the Department. Yet Stone, despite his retirement as the Director of the DAE, continued to lead a large research group of his own in the Department. He cooperated with Reddaway and the two economists worked together in harmony. Singh (2006, 2009) has argued that the DAE's outstanding success in the 1960s was due to the close competition as well as collaboration between Reddaway's and Stone's teams. This led to what might be called the Golden Age of applied economics in Cambridge from the 1960s to the 1980s. Reddaway himself denied the allegation of any plot against Dick Stone, although others are not so convinced.⁵

A former pupil of Reddaway's, the distinguished British economist Martin Weale, put the reason for Reddaway's apparent career paradox in the following terms: Reddaway was not a theoretical economist or one interested in the history of economic thought. He liked dealing with practical questions of the day. Once these problems were solved, there was nothing much to be done with these issues. He therefore ignored the deeper questions which might arise from a more stringent examination of the same. Weale demonstrates this idea with the example of the Selective Employment Tax (SET). Once it has been examined, there is not much left to do. One cannot keep on analysing the SET ad infinitum. This is a more plausible explanation for Reddaway's

⁵I am grateful to Professor Ron Smith at Birkbeck College, University of London, for permitting me to see his correspondence with Reddaway on this matter.

comparative lack of recognition in the economics profession at large than the argument outlined in Singh (2006)—that Reddaway was not, for example, a leading candidate for a Nobel Prize because of the conservative domination of the profession. The available evidence is more in accord with the hypothesis that Reddaway was not interested in new analysis or new ways of resolving economic issues. He was content with being able to answer day-to-day questions of economic analysis in simple terms rather than shape economic theory to answer more complex questions. In this regard, Stone was much more of an innovator in dealing with economic or empirical issues.

Reddaway and Stone were two different kinds of economists but nevertheless found common ground to work together to produce one of the world's leading departments of applied economics. Stone was known for his encouragement of rigorous econometric methods and in the early post-war period Cambridge had become home to some of the world's leading econometricians, including Houthakker (1961, 1965), Prais and Houthakker (1955), Tobin (1958), and Farrell and Brown. As it was widely believed, the best two places in the world for doing theoretical econometrics were Cambridge and the Cowles Commission at Yale University.

On his appointment as Director of the DAE, Reddaway sought to change this emphasis from econometric methods to applied economics. In view of the outstanding reputation of the DAE as a place for doing econometrics, this was a hard act to follow but Reddaway was determined and he carried out his plan with total conviction. Instead of econometrics, the Department began to produce outstanding research in other areas, including British economic history and applied analysis of various economic problems both inside and outside the UK.

During his 15-year tenure, Reddaway oversaw several major economic modelling projects and retained his interest both in policy and planning. He worked on economic growth, on British investment overseas, and the SET. Significantly, his work included productivity growth and technical progress. This important line of work was started initially by Reddaway's doctoral research student, W.E.G. Salter. After Salter's untimely death, Reddaway produced a second edition of Salter's book *Productivity and Technical Change* (Salter 1969) by updating his analysis. There was similarly during this time outstanding work on many other areas of applied economics, as well as on sociology (by John Goldthorpe and his colleagues). In the meantime, Stone, for independent reasons, also changed his approach to economic analysis. Together with his associate Alan Brown, he launched a big new research project in the DAE, not on econometric methodology but on economic planning in the UK.

4 Reddaway's New Methods of Teaching and Research

It will be recalled that Reddaway was trying to make his mark when the Cambridge Faculty of Economics included world-famous stars such as James Meade, Piero Sraffa, Joan Robinson, Richard Kahn, and Nicholas Kaldor. Nevertheless, Reddaway's lecture course (which, incidentally, was wrongly entitled 'Statistics'), gained in popularity and attracted a stable audience of about 30 students. Many graduate students started to attend Reddaway's lectures. Posner (2002) pointed out that these students had to get special permission to attend the Reddaway lectures. Reddaway's students were obliged to present the results of their analysis in a straightforward manner and without reference to heavy statistical methodology. Moreover, questions began with a practical problem at hand, which might ask how statistics can be effectively, convincingly, and correctly used.

It would perhaps be best to illustrate this point with an actual question set in a typical Reddaway examination:

You are employed by a business tycoon of uncertain politics, who got a II.1 in economics, but did not take the statistics paper. You find that he has gone away for the afternoon leaving the following note:

I spent yesterday evening between two old College friends T. Ory and L. About. Ory was trying to convince me that the economic record for 1959–63 reflected great credit on the government, because there had been good rises in the following real terms:

- (a) The total production of goods and services.
- (b) Output per head in manufacturing.
- (c) Foreign trade.
- (d) Capital formation.
- (e) Personal consumption, both in total and per head of population.

Moreover, he insisted that there were other favourable features, such as:

- (i) Unemployment has been negligible.
- (ii) The growth in the quantity of money had been no greater than was justified by the rise in production.
- (iii) The rise in prices had slowed down to an easily tolerable pace.
- (iv) The balance of payments had, on the whole, been favourable.
- (v) The international position of the pound had been strengthened.

On the other hand, Abour maintained that in a progressive economy it was natural to have rises in all the items in Ory's list, and the real feature of the period was their smallness. As for his other points, Abour's rejoinder was as follows:

1. Unemployment had been rising throughout the period.
2. The movements in the quantity of money were, as such, of no real importance.
3. The rise in prices had been far from negligible, and had been kept down largely by the stability of import prices, for which even a Tory government could hardly claim the credit.
4. By the relevant tests, the balance of payments had been unfavourable, and indeed the Government had used its bad taste as an argument for keeping down wages.
5. We ended the period with less reserves than the start, and greater liabilities.

Please get out the statistics which you consider relevant for judging the truth of the above matters, prepare tables and/or graphs in such a way that I can draw conclusions from them, and *write notes on what your own conclusions are*, indicating any places where these are of a subjective character (italics in original).

This question was set for second-year Economics Tripos Part II undergraduate students in 1964. The students were provided with the National Income *Blue Book* and another government source book, *Economic Trends*. This (though abbreviated slightly) was essentially the whole examination, to be completed within three hours. Although the question is in its nature a simple one, it takes a research stance which demands original thinking and accurate judgement, and would be considered testing even by many doctoral students in British universities. Reddaway thus set a new and intriguing method of teaching applied economics to undergraduates in Cambridge.

As Michael Posner, who as a colleague was well acquainted with Reddaway's teaching methods, noted: '[Reddaway's] lectures in the 1950s and 1960s... should have been called "economic statistics and how to use them"' (Posner 2002). He describes how the lectures were filled with both students and colleagues from the Faculty of Economics, who sought permission to audit lectures and were made teaching assistants or given demonstrating roles. Posner describes, 'students, young bright people with the esoteric thrills and subtleties of high theory, were asked to sit in classrooms, roll up their intellectual sleeves, and attempt to construct charts and tables which would throw light on issues of evident importance and practical interest'. The so-called Reddaway-questions

were challenging; they provided economics students at Cambridge with the most important statistical skill—to maximise the relevant information, and to minimise the noise. Posner suggests:

The familiar monthly and annual volumes of government statistics provided the raw material for this class work; but students were challenged to adapt, compare and manipulate the figures so that they yielded the maximum of relevant information and the minimum of noise and confusion. Reddaway and his younger colleagues would write sharp comments on the students' efforts.

In terms of broader impact, former students of Reddaway would go on to occupy various public offices (e.g. the civil service, the offices of City brokers, and the editorial desks of the better class of newspapers), resulting in, as Posner suggests, 'the quality of public economic argument...[being]...much improved' (ibid.).

Thus, contrary to Singh (2006), Reddaway had enormous influence on British economics from the mid-1960s until the early 1980s. He changed the way of doing economics. Indeed, students today are increasingly demanding something like Reddaway's approach to economic questions at British and other universities around the world.

5 Corporate Finance and Portfolio Selection Theory⁶

The rest of this chapter will discuss the innovative papers or books to which Reddaway contributed and which have stood the test of time. Reddaway's most famous contributions include his pioneering work on the Indian economy, his essay on the UK's declining population, his stringent examination of portfolio selection, and his analysis of corruption in Bangladesh (Sobhan 2002), to name but a few. Amongst Reddaway's most enduring contributions include his works on corporate finance, which include his examination of portfolio selection in theory, his review of Rayner and Little's book *Higgledy Piggledy Growth* (Reddaway 1967), his theory of corporate takeovers, and his defence of using mathematics only sparingly in economic analysis and how that related to his investment management of the finances of Clare College.

It must be observed that Reddaway was very much interested in corporate finance. For many years he wrote a column for *Investors Chronicle*, a British

⁶This section closely follows and is based on Singh (2006).

weekly journal, under the pseudonym 'Academic Investor'. In this column, he described the methods he used for maximising College wealth in his capacity as a senior member of the College Investment Committee (CIC). His performance as an academic investor was simply outstanding. He invariably beat the main stock market index by making only minimal changes (normally once a year) in his list of holdings. Reddaway sets out in the *Investors Chronicle* his extremely simple method of creating a list of stocks and shares, which the College should hold to maximise its objectives. The CIC had four members who, as noted above, changed the list of stocks at an annual meeting. Many people described this policy as simple-minded. Reddaway describes it as a 'satisficing' policy rather than a maximising one.

The Committee only met for two or three hours in the whole year and made the minimal changes which were required to deal with takeovers, mergers, and the like. In addition, a couple of other changes in the portfolio were made in the light of circumstances. Up to 1953, the College had no equities at all and was investing partly in real property and partly in government securities. Essentially, the College followed 'buy-and-hold' policies. Reddaway believed that this approach would give the College a satisfactory return for a couple of centuries! The main principles of the College investment policy were summarised by Reddaway (1981) as follows:

- (a) To be continuously invested in equities;
- (b) To give these a wide spread over different industries and to select them as if the College was buying these 'for keeps';
- (c) In choosing the holdings, not to attempt to elaborate investment analysis, either ourselves or through advisers;
- (d) To make the most of Colleges' tax-exempt status although, in practice, it never amounted to much.

The Finance Committee kept no complete records of the College's investment successes and failures but, after 1961, Reddaway published in the *Investors Chronicle* a commentary on the results at each annual review. The main conclusions of those reviews were that the College portfolio had returned a higher income than the Financial Times (FT) Index in all 25 of the completed years, but that this result might easily have disappeared as the College's shares increased in value, this given relatively few changes to stock selection (see Reddaway 1981: 388).

Reddaway reflected on the sum total of these results in the following terms: 'In brief, my main impression from 26 years of portfolio management is the need for humility' (ibid.: 394). Reddaway went on to add: '[O]ur principles

suggested a basic recognition of our colossal ignorance about what the future will bring forth' (ibid.). Reddaway compared his results with those of a well-known American expert of portfolio selection theory, Professor Burton Malkiel, and concluded as follows:

Over long periods of time mutual fund portfolios have not outperformed randomly selected groups of stocks. The only concession to conventional theory which he [Malkiel] makes is that on average, "riskier" funds do better than the rest—but he adds that "randomly selected portfolios or riskier stocks also tended to outdistance the market" (ibid.: 398).

It may be useful to consider further Malkiel's four rules for successful stock selection, which Reddaway outlines in his chapter on 'Portfolio Selection in Practice' (Reddaway 1981: 401–402). These are given below with Reddaway's comments on them:

Rule 1: "Confine purchases to companies that seem to be able to sustain above-average earnings growth for five years or more." Reddaway notes that 'this seems to be crying for the moon,' since Malkiel has shown that successful earnings-projections are beyond human power.

Rule 2: "Never pay more for a stock than can reasonably be justified by a firm foundation of value."—In a crude way we try to follow this by choosing 'as if it were for keeps', and not in the hope of a quick re-sale at a profit.

Rule 3: "It helps to buy stocks whose stories of anticipated growth are ones on which investors can build castles in the air." On principle, we do not seek for 'help' of this kind, which is relevant to attempting to predict future share-prices.

Rule 4: "Trade as little as possible: in general, hold on to the winners and sell the ones that don't work out."—We do not really observe this, though we believe in keeping down the trading (ibid.: 401).

Reddaway next focuses on a crucial question—namely, what light does the theory of portfolio selection throw on 'our' activities, or our activities throw on the theory of portfolio selection? He attempts to analyse the fact that 'even if we take the modified results which allow for the deficiencies in the FT Index the College's portfolio has beaten the adjusted index for 19 out of 25 years and established a cumulative superiority of more than 2 to 1' (ibid.: 402).

His overall substantive result from this exercise is as follows: 'I feel that there must be something in what we do that gives us an advantage over random selection. I would not find this at all surprising if it did not seem to imply that we do better than the average result achieved by professional managers' (ibid.: 403; underlining in original).

Reddaway concludes his chapter on portfolio selection theory as follows:

On one point I am completely clear. I have no faith whatever in the theory that 'if anyone found a method of beating the index it would only continue to work if he kept absolutely quiet about it'. Our results have been published regularly since 1961, the portfolio has been published annually since 1970, and I have yet to hear of anyone copying either our methods...or our portfolio, or even our century-long objectives (ibid.: 404).

6 Reddaway on Statistical and Economic Significance

In the tradition of the Cambridge Keynesians, Reddaway maintained a healthy scepticism about the use of econometrics. This attitude was based on the familiar premise that relevant variables and their possible real world interactions far exceed the capabilities of the architecture of econometrics. One of the most distinguishing features of Reddaway's applied economics was his insistence on the fundamental distinction between statistical significance and economic significance. At an intuitive and elementary level, the issue is quite simple. A difference between two variables can be permanent and tangible without being 'significant' in statistical terms. On a similar note, a difference in variables can be highly statistically significant and yet not be valuable in practical economic analysis and therefore not have any economic significance. It is unfortunately common in applied economics not to recognise this essential distinction between economic and statistical significance, and is invariably ignored by many researchers, even in the top-rated journals.

Reddaway was convinced rather by simple and straightforward economic arguments than convoluted ones. An example of Reddaway's criticism of the improper use of statistics is his disparaging review of Rayner and Little's *Higgledy Piggledy Growth*. While conceding that 'the power of computer-aided research batteries to pulverise untenable arguments is certainly impressive' (Reddaway 1967: 595), Reddaway is unconvinced by Rayner's strength of final statement in his analysis and describes it as 'an almost classic case of a confusion between failure to prove concisely that there...[exists]...a tendency' and 'coming to a conclusion that there is not' (ibid.: 595). Choosing to remain 'rough and relevant' only where need be, Reddaway valued easy readability, good scholarship, and detail in his research and openly criticised where he found these qualities lacking. The use of statistical descriptors was highly relevant to Reddaway,

who was always a critic of indirect indicators without precise definition—such as the description of ‘performance’ versus ‘growth’ in discussing corporate strength over time (Reddaway 1967).

Reddaway’s argument was founded upon the work of Jerzy Neyman and Egon Pearson, who were two highly distinguished mathematical statisticians working during the 1930s. They identified two types of errors: Type 1 and Type 2, writing:

Is it more serious to convict an innocent man or to acquit a guilty? That will depend upon the consequences of the error. Is the punishment death or a fine; what is the danger to the community of released criminals; what are the current ethical views on punishment? From the point of view of mathematical theory all that we can do is to show how the risk of errors may be controlled and minimised. The use of these statistical tools in any given case...*must be left to the investigator* (Neyman and Pearson 1933: 296; italics added).

Wald (1939) developed this argument further and suggested that

[t]he question as to how the form of the weight [loss] function...should be determined is not a mathematical or statistical one. The statistician who wants to test certain hypotheses must first determine the relative importance of all possible errors which will depend on the special purposes of his investigation (ibid.: 302).

Reddaway’s important insistence on differentiating between economic and statistical significance in the practice of research was not unfounded. McCloskey and Ziliak (1996) carried out a survey to discover how economists use the concepts of statistical and economic significance and how consistent they are in the use of these concepts. The results indicated a considerable amount of ambiguity. Indeed, in their influential article, ‘The Standard Error of Regressions’, they found that out of 182 full-length papers published in the *American Economic Review* in the 1980s, 70% did not distinguish between economic and statistical significance. Other principal findings include the following (ibid.: 106–107):

- At the first use of statistical significance, typically in the “Estimation” or “Results” section, 53% did not consider anything but the size of t - and F -statistics. About one third used only the size of t - and F -test statistics as a criterion for the inclusion of variables in future work.
- 72% did not ask “How large is large?” That is, after settling on an estimate of a coefficient, 72% did not consider what other authors had found; they

did not ask what standards other authors have used to determine ‘importance’; they did not provide an argument one way or another whether the estimate $\beta = 0.999$ is economically close to 1.0 and economically important even though ‘statistically different from one.’

- 59% used the word ‘significance’ in ambiguous ways, at one point meaning ‘statistically significantly different from the null,’ at another ‘practically important’ or ‘greatly changing our scientific opinions,’ with no distinction.
- Despite the advice proffered in theoretical statistics, only 4% considered the power of their tests. 1% examined the power function.
- 69% did not report descriptive statistics...that would allow the reader to make a judgment about the economic significance of the results.
- 32% admitted openly to using statistical significance to drop variables ... One would have to have more evidence than explicit admissions to know how prevalent the practice is in fact.
- Multiple-author papers...more often spoke of ‘significance’ in ambiguous ways, used sign econometrics, did not discuss the size of estimated coefficients, and found nothing more than the size of test statistics to be of importance at the first use of statistical significance (Table 2 in *ibid.*: 107).

Despite the hope that following such a paper in a leading journal the prevailing situation might improve, McCloskey and Ziliak (2004) found in a follow-up article that this was not the case. Using a test of statistical significance, they found that out of 137 papers published in the *American Economic Review*, a high 82% ‘mistook statistically significant coefficients for economically significant coefficients’ (*ibid.*: 333). The paper indicated that a large majority—81%—considered it acceptable to look at the sign of a coefficient rather than its magnitude, thus ignoring the distinction between statistical and economic significance. McCloskey and Ziliak suggest that this state of affairs may be due to the chaotic rat race in getting papers published by academic journals.

These findings indicate the difficulties of correctly specifying the nature and meaning of statistical significance, which Reddaway always emphasised in both his teaching and his research. Studies find that even research published by economists attached to top-tier universities (e.g. Chicago, Harvard, MIT, Princeton, Stanford, and Yale) do not fare much better in this respect. As such and somewhat unfortunately, the ordinary usage of econometrics in economics, as Reddaway had always maintained throughout his career, more often than not incorrectly takes statistical significance to be the same as economic significance. In later years, the distinguished statistician, the late Morris DeGroot (1975: 496–497) observed:

It is extremely important...to distinguish between an observed value of U that is statistically significant and an actual value of the parameter ... In a given problem, the tail area corresponding to the observed value of U might be very small; and yet the actual value...might be so close to [the null] that, for practical purposes, the experimenter would not regard [it] as being [substantively] different from the [the null].

[I]t is very likely that the t -test based on the sample of 20,000 will lead to a statistically significant value U ... [The experimenter] knows in advance that there is a high probability of rejecting [the null] even when the true value...differs [arithmetically] only slightly from [the null].

Had Reddaway taken the time to develop his argument and formalise it, he might have had the kind of influence that McCloskey and Ziliak have achieved.

7 Reddaway and the Indian Economy

Reddaway took a keen interest in the Indian economy and he wrote, among other things, an excellent small book on the subject called *The Development of the Indian Economy* (1962). In the first of these books, he examined the issues which arise in defining the nature of a developing country from that of a developed country, describing the logic behind comprehensive long-term planning. Reddaway starts with the question of defining unemployment for a developing country. He suggests that the proportion of people in India who are unemployed, according to a Western-type definition—for example, who have done no work in the last week—must be quite small. Reddaway suggested this has to be the case in the absence of any public provision for the unemployed. One methodological conclusion which follows from his analysis is that one should not think of India's problems in terms of unemployment but rather in terms of inadequate numbers of openings for really productive activities.

Reddaway makes another subtle point in this context, namely even though there may be few people unemployed on Western definitions, development should nevertheless be planned in the sense that there was an inexhaustible supply of unemployed workers. This indicates that workers can be attracted to worthwhile jobs on any scale that is likely to be of practical interest without their recruitment leading to a fall in the output of other goods or services. In Reddaway's words (1962: 4):

Does this effectively unlimited supply of labour mean that one can apply 'depression economics' or 'Keynesian remedies' to secure a large increase in the

national output, merely by increasing monetary demand? The answer to such a simple question is clearly 'No'—but the question is, none the less, not as simple-minded as it might seem.⁷

In his work, Reddaway discusses the question of capital formation by bare hands and presents his interpretation of the third Five-Year Plan period for the Planning Commission of India. He makes the important point that, in addition to capital and labour, organisation is an essential input for increasing the supply of capital, particularly with bare hands. Reddaway's approach to the problems of the Indian economy is thoroughly modern and emphasises the role of practical long-term planning. Reddaway was very close to the Indian planners; to a considerable degree, his 1962 book reflects this close association. Reddaway's work on development problems suggests his emphasis on structural change and growth. His analysis supports the role of modern industry and import substitution, emphasising concern with the balance of payments. However, in a review of *The Development of the Indian Economy*, distinguished American economist, Professor Bert Hoselitz, takes Reddaway to task for his relative neglect of agriculture, the unemployment problem, and other similar subjects (Hoselitz 1963).

In the long history of Reddaway's scholarship, there is an interesting debate between the Indian economist Professor Padma Desai and Reddaway. In this exchange, Reddaway outlined his own basic philosophy of research:

I have attempted to tackle practical problems, whether on full employment, growth, underdeveloped economies, inflation, the effects of direct investment overseas, the selective employment tax, or the investment of portfolios. To do so, I have sought to combine theory with realistic data and to look for the factors which are quantitatively important, rather than those which are intellectually stimulating. I have tried to be pragmatic in my choice of methods for tackling problems and to be clear about the alternative position with which comparisons are effectively being made (and to be sure that it is a meaningful and consistent one). Favourite slogan for pupils and research colleagues: 'It is better to be roughly right than to be precisely wrong (or irrelevant)' (Reddaway quoted in Blaug 1999: 932).

Reddaway's depiction of Indian development always emphasised careful judgement over blind faith in numbers, resting 'on a logical foundation of cause and effect' (Reddaway 1963a: 320). As I explain in Singh (2006: 292):

⁷ For a full analysis of this issue, see Reddaway (1962).

In other words, in the real world, even though data are scarce, it is better to have rough orders of magnitude than none at all, in order not to operate entirely in the realm of abstraction. If data (or theory) did not exist, Reddaway's method was to use surveys to ask people for the information. If existing theory was inadequate, he would attempt to extend it to fill the gaps. However, the latter was not his prime aim. Colleagues recall him referring to theory as 'talky talk'.

We come back now to the specifics of the Desai–Reddaway exchanges. The whole debate appeared in a single issue of the journal *Oxford Economic Papers*. Reddaway's opening salvo was as follows:

I saw, and still see, no advantage in expressing the reasoning in the form of mathematical equations. Such equations are a useful device where there is a great deal of mutual dependence of variables, because a verbal description cannot then easily show the interactions and the process of mutual determination; moreover, it is then very laborious to arrive at the solutions which fit the conditions, except by some mathematical process analogous to the solution of simultaneous equations; and one might fall into the trap of not realizing that the system was under-determined, and arriving by trial and error at a set of figures which fulfilled the conditions but had no superiority over many other sets which would also do so (Reddaway 1963a: 326).

As I explain in Singh (2006: 292), this is how Desai criticised Reddaway's book:

Desai argued that the book did not set out a fully specified planning model so that it was difficult to judge whether the plan was efficient or not. She also thought that, from the information given in the book, the model was under-determined, i.e. the number of variables to be determined was greater than the number of equations. She further expressed irritation over the fact that Reddaway had not bothered to specify his model in terms of equations, which she regarded as essential to understanding the underlying economic and statistical analysis.

Desai thus criticised Reddaway for not explicitly stating the model he was working with, which she in any case also considered underdetermined. Reddaway, in his response, pointed out that his purpose was not to assess the efficiency of the Indian planning models but just to see whether the specific model of the third Five-Year Plan was compatible with the availability of resources. Reddaway was highly critical of Desai's defence of mathematics in economic analysis.

Reddaway's attitude towards government intervention was greatly influenced by his wartime experience. At that time, among the British government economists, Reddaway was regarded as being very much on the interventionist

side. The statisticians at the Board of Trade were half-jokingly nicknamed the ‘Gosplanners’ because of their interventionist outlook. In contrast, other government economists, notably Meade and Keynes, were called ‘thermostaters’, indicating that they believed in limiting government intervention to macroeconomic policy to ensure full employment. In this view, microeconomics should be entirely the sphere of private households and firms. Reddaway was a pragmatic economist and a problem solver first and foremost. These attributes, as well as his wartime service, led him to regard the government as part of the solution rather than the problem. Indeed, he took a very active part in government activity and policy making on various occasions. As already noted, Reddaway served the UK and other national governments and the international community in a number of different ways, making notable contributions to the work of each.

8 Conclusion

Brian Reddaway will be remembered as a practical economist. He was a committed teacher and an excellent, dynamic scholar, whose breadth of real world policy experience informed new pedagogical methods and a distinctly non-ideological style relevant to the *practice* of economics in Cambridge.

Reddaway himself considered the work he did at the DAE during his Directorship as his most important contribution, particularly his work on direct investment overseas. This involved a major exercise with survey data on British companies investing in other countries. In this chapter, however, I have, amongst other things, highlighted Reddaway’s highly pertinent argument on the distinction between statistical significance and economic significance. This question has arisen in recent debates about the teaching and research of economics. Indeed, even today, some of the most accomplished economists often fail to make this distinction, and fail to draw out its full implications. The practical significance of Reddaway’s contribution to the teaching of economics is now coming to the fore. With thousands of students all over the world agitating for the practical, Reddaway-type approach to economic analysis, it remains to be seen whether those who control the profession will accept these proposals and draw fresh boundaries for the subject.

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Richard Murphey Goodwin (1913–1996)

K. Vela Velupillai

1 Introduction

Those who had the privilege to attend Richard Goodwin's lectures, at Harvard, Cambridge, or Siena, regularly over a period of half a century, know the wonderful ways in which he used his considerable artistic skills to illustrate (sic!) the geometry of global economic dynamics and multisectoral interdependence. The triptych of 'global behavior, interaction of the parts, dynamics', tamed by ingenious geometry remained the guiding principle of Goodwin's writings, teaching, and thinking.

It was, after all, the geometric way of understanding global dynamics (cf. Palis and de Melo 1982) that enabled him to devise the famous one-sided oscillator, motivated by the economic intuition that the Hicksian 'floor' was

Much of the pure facts of Richard Goodwin's life are taken from one or another of my own earlier writings on Goodwin (particularly, Velupillai 1996, 1998). Goodwin never failed to remind me that his middle name was 'Murphey with an "e"! My indebtedness to the written contributions, and personal friendship, of Ralph Abraham, Richard Day, Geoff Harcourt, Ragu Ragupathy, Otto Rössler, and Stefano Zambelli are too obvious to require explicit acknowledgement. They are, alas, not responsible for the remaining infelicities.

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not a necessary condition for the maintenance of oscillation as a limit cycle.¹ Then there was the geometry of vector–matrix analysis and spectral theory, both linking interaction of the parts with dynamics, to his innovative ways of building global behaviour on the basis of independent local analysis (without the tiresome appeal or non-appeal to analytically arid issues of aggregation).

Others, even without the pleasure and privilege of attending his normal series of lectures, who struggled through to eventual enlightenment by reading, and working through, the classics on non-linear and multisectoral macrodynamics that emanated from his exquisite pen, quite apart from being impressed by the ingenious geometry, were also struck by Goodwin's systematic use of simulations.² He struggled to convince himself and his readers that plausible empirically sensible numbers for key parameters, in the relevant range for crucial variables, made economic sense. Indeed, this was manifested in all his classics, between 'Keynesian and Other Interest Theories' (Goodwin 1943) and 'A Growth Cycle' (Goodwin 1967), including the remarkably original *Elementary Economics from the Higher Standpoint* (Goodwin 1970)—and those that emanated from his pen (and fertile mind), all the way to 'the end'.

Goodwin was also the pioneer, in economics, of using the Perron–Frobenius (cf. Goodwin 1953) results for non-negative square matrices, in conjunction with what he called normalized general coordinates, a method by which interdependence was simplified by a coordinate transformation (cf. Goodwin 1949a; Frazer et al. 1938), which allowed consideration of independent artificial sectors at an analytic stage. This enabled Goodwin to enunciate what I refer to as his precept: aggregate dynamics must be *non-linear*; multisectoral dynamics must be *linear*.³ The coordinate transformation allowed the interdependent multisectoral dynamics to be 'reduced' to independent sectoral analysis, using non-linear methods.

Goodwin was born in Newcastle, Indiana, and attended local schools before going to Harvard College in 1930, to major in political science, to prepare, eventually, to study law, on the advice of his grandfather, a lawyer turned banker. The Depression led to the downfall of his grandfather's bank and, independently, his father became bankrupt. The political science that

¹ Le Corbeiller's letter to Goodwin, dated 29 March 1958 (the original, handwritten version of which was given to me by Goodwin, at the time of his formal retirement at Cambridge, in 1980), illustrates the way he came to the conclusion that a characteristic with only one bend—'one nonlinearity', as he called it—was sufficient to generate a limit cycle on the plane. See also Goodwin (1950, 1990) and Velupillai (1998).

² However, it is not the kind of 'mindless simulation' that is currently fashionable in many frontiers of so-called computational economics, agent-based economics, and even stock-flow consistent modelling of disaggregated economies. Goodwin's simulations were in the mode of what I have come to call the Fermi–Pasta–Ulam method of analysis (Fermi et al. 1955), and its perfect execution was in Goodwin (1947). I have always maintained that this is the authentic, and proper, way to do 'agent-based economics'.

³ It is useful to contrast this with what I call Turing's Precept: 'Description must be *non-linear*; prediction must be *linear*' (quoted in Gandy 1954 [2001]: 266; italics added).

Goodwin continued to study was now given a Marxian twist and a lifelong adherence to left-wing politics.

He spent three years from 1934 as a Rhodes Scholar at St John's College, Oxford—where his Tutor had the foresight to send him to Roy Harrod at Christ Church, with whom he read *The General Theory* in its proof version (cf. Harcourt 1985: 414)—and returned to Harvard where, from 1938 to 1950, he was a member of the economics department, but also taught physics (during the Second World War). It was during this brief period that he met, and befriended, Philippe Le Corbeiller, who taught him the theoretical technology of planar non-linear dynamics.⁴ Fleeing Harvard from the increasing difficulties of the 'McCarthy era', and with the help of Richard Stone, then Director of the Department of Applied Economics, he found refuge at Cambridge, England, in 1952.

He spent the next three decades at Cambridge, as a Fellow,⁵ Director of Studies in Economics (and Wine Steward) of Peterhouse, Girdlers' Lecturer,⁶ and, finally, as Reader in Economic Theory in the Faculty of Economics and Politics.⁷ On retirement in 1980, he took up a Professorship in the Faculty of Economics at the ancient University of Siena, which enabled him to return in a formal way to his beloved Italy. That faculty of economics is now renamed in his honour as the Dipartimento di Economia 'R. Goodwin'.

⁴As Goodwin told me during a personal conversation in September 1977 at my home in Svanshall, Sweden: 'I was walking down the corridor of the Physics Department when I saw the name "Ph. Le Corbeiller" on one of the doors. I knocked, the door was opened by a distinguished looking, elegantly dressed, gentleman. I asked him whether he was the Le Corbeiller who had written on the desirability of using van der Pol's theory of relaxation oscillations in the first volume of *Econometrica* (Le Corbeiller 1933). He answered yes. I asked him whether he would teach me the theory of nonlinear dynamics. He, then, literally took me "by the hand" and taught me nonlinear dynamics'. Goodwin had not only read Le Corbeiller's 1933 piece, but also Le Corbeiller (1931) and, later, mastered the geometry of relaxation oscillations that was a pedagogical masterpiece in Le Corbeiller (1936). This latter method remained his lifelong way of devising, drawing, and explaining the geometry of non-linear macrodynamics. It was this method that enabled him to discover the one-sided oscillator, mentioned above. It is a method that seems to have been 'hollowed out' in this modern age of blind computer simulations. I am, now, the proud owner of Goodwin's copies of Le Corbeiller (1931, 1936).

⁵He told me, in a personal conversation in the mid-1970s, that Dennis Robertson was sent Goodwin's papers for evaluation by the Governing Body of Peterhouse and that the great successor to Pigou and Marshall responded by writing in his official report: 'I do not agree with anything Goodwin writes, but I would not miss them for the world!' Later, it was Harry Johnson who was sent by Richard Kahn and Joan Robinson to 'interview' Goodwin, to 'investigate his suitability to become a member of the Cambridge Faculty of Economics and Politics'.

⁶Following, therefore, in the footsteps of H.O. Meredith, later a distinguished Professor of Economics at Queen's University in Belfast (where, several generations later, this minor author 'succeeded' that legendary scholar), who was himself succeeded by Maynard Keynes. When Goodwin retired as Director of Studies in Economics at Peterhouse, I had the pleasure and privilege of taking over that coveted post, not least due to Goodwin's efforts and influence.

⁷The last time I visited the fourth floor of the Sidgwick site, a decade or so ago, where the Faculty of Economics and Politics of the University of Cambridge is housed, the corridors were adorned by some of Goodwin's paintings which, he told me, had been bought during Professor Aubrey Silbertson's tenure as the Chairman.

Although formal retirement in Siena came at the age of 75, he continued to teach there almost till death intervened, in his 83rd year. Goodwin was an active and interested participant of graduate seminars at the University of Siena,⁸ which were, appropriately, held in the Sala Goodwin, which was tastefully adorned with some of his own paintings.

The rest of this essay is structured as follows. In the next section, Goodwin's pioneering contribution to non-linear macrodynamics is discussed, with some geometric *hilfenkonstruktionen*. The topic in Section 3 is his extraordinarily imaginative and fertile contributions to varieties of ways to study linear, multisectoral, interdependent economics. In Section 4, an attempt is made to summarize his intellectual legacy.

2 Non-linear Endogenous Non-stochastic Aggregate Fluctuations (NENAF)

And that was what I was interested in—trying to formulate a model which would show that Harrod was right and Tinbergen wrong in that rather brutal review he wrote of Harrod's book ... I was concerned for ten years with trying to formulate a model which would show that Harrod was right in his intuition, however preposterous his inept formulation—and it was this almost single-minded pursuit, which finally led me to the formulation of the *one-sided oscillator* (Goodwin to Velupillai, 17 June 1985; italics added).

The non-linear, endogenous, non-stochastic theory of macrodynamic fluctuations did not emerge out of a theoretical vacuum; instead, it came out almost as a natural consequence, given the direction in which business cycle theory was developing in the early 1930s. But the publication of *The General Theory* (Keynes 1936 [1973]) and *The Trade Cycle* by Harrod (1936) were the immediate reasons for Goodwin's 10-year odyssey mentioned above. Harrod (*ibid.*) sought to combine the Keynes–Kahn multiplier with the accelerator—he (Harrod) referred to is as 'the relation' (*ibid.*: 58)—together with assumptions on imperfect competition, resource constraints⁹ and relatively inflexible prices, and interest rates over the business cycle.¹⁰

⁸During my own years at the European University Institute in Fiesole, 1981–1985, picturesquely located in the hills outside Florence, he was a frequent visitor to the graduate and staff seminars there.

⁹Many interpret Goodwin as assuming just a 'ceiling' on employment in the labour market; this was never so. He interpreted Harrod as assuming a ceiling of resources in any advanced industrial economy, but particularly in a largely free enterprise (but not perfectly competitive) capitalist system. These resources could be labour, but not necessarily always the constraining 'ceiling'. Incidentally, it was Harrod, *not* Hicks, who first introduced the concept of a ceiling in an endogenously fluctuating aggregate macroeconomy.

¹⁰See also Ichimura (1955: 217, fn. 28), the book in which this splendid article appeared was the first, to the best of my knowledge, to use the phrase *Post-Keynesian Economics* in its title); in many ways Harrod

For selecting these features, akin to what Kaldor (1957) later came to call the *stylized facts* (albeit in the context of aggregate growth theory), one appealed to reasonably reliable empirical evidence and broad patterns during those turbulent times. These chosen characteristics had to be theoretically substantiated and historically congruent while at the same time encapsulating economic data that was available. Aggregate fluctuations in capitalism's dynamic contours¹¹ were synthesized and summarized, in retrospect, as the following:

1. Evidence for the *persistence* of economic fluctuations;
2. The *asymmetric* nature of economic cycles (i.e. the observed time series of aggregate variables were significantly *non-sinusoidal*);
3. Presence of *multiple equilibria*;
4. *Instability of equilibria*, at least, locally; and
5. Presence of significant *non-linearities* in the relationship between economic variables and/or in the behaviour of the aggregate variables defining the macroeconomic fluctuations.

These five essential features—*persistence*, *asymmetry*, *multiple equilibria*, *instability*, and *non-linearity*—which were chosen as the modelling criteria, in turn, implied an endogenous cycle. The orthodox view of the economy was tied to the idea that it was in a state of equilibrium and stable. Departures from this equilibrium, if any, were eliminated by self-adjusting mechanisms of the market forces. The above desiderata—both in terms of the economic hypotheses that they resorted to (non-linearities in relationships, multiple equilibria, instability) and the *dynamic contours* they relied on and attempted to explain (persistent and asymmetric fluctuations)—also highlight a marked departure from the prevailing orthodox views of the functioning of the economic system in the advanced industrial economies. In this, Goodwin adhered squarely to the Cambridge Keynesian tradition of Kahn–Sraffa–Robinson–Kaldor, but he emphasized, almost always, a *growth cycle* aspect, to which was added the Schumpeterian element of innovations. This spanned his contribution to NENAF theory from 1946 to 1967 (Goodwin 1946, 1967)—15 of these 21 years were spent at a predominantly Keynesian Cambridge; the earlier years at Harvard were very much Schumpeterian, but infused with copious elements of Keynes and Harrod in Goodwin's contributions to business cycle theory (the locus classicus of which was Goodwin 1951a).

(ibid.) is a forerunner of so-called New Keynesian macroeconomics. However, the uncompromising drive to found macroeconomics on futile orthodox rationality—dressed up as mathematical microeconomics—by the New Keynesians makes them uncomfortable bedfellows with Harrod (1951).

¹¹ I prefer the phrase dynamic contours to Kaldor's—now much maligned—notion of *stylized facts*, simply because the former retains fidelity to Goodwin's Schumpeterian vision of capitalism's intrinsically unstable dynamic evolution.

It was in Goodwin (1951a) that the first formal, mathematical model encapsulated all the five desiderata mentioned above; but to these were added an Occamist principle of minimal non-linearities—essentially, one. His insistence on non-linearity as the key and a fruitful way of explaining maintained oscillations was explicit not only from the title of this classic but also by the observation that ‘economists will be led, as natural scientists have been led, to seek in *nonlinearities* an explanation of *the maintenance of oscillation*. Advice to this effect, given by Professor Le Corbeiller in one of the earliest issues of this journal, has gone largely unheeded’ (Goodwin 1951a: 2; italics added).

The results reported in this classic were first presented at the December 1948 meeting of the Econometric Society in Cleveland and summarized in the April issue of *Econometrica* in 1949 (Goodwin 1949b) before the full version was published in 1951.¹² Harrod’s book on the trade cycle claimed that a combination of accelerator and multiplier was the cause of dynamic instability in the Keynesian system. But it was criticized by Tinbergen (1937), pointing out that the first-order system, as Harrod conceived it, is incapable of generating cycles.¹³ Goodwin believed in Harrod’s insight and finally found a way of formalizing it by resorting to non-linearities. His excursion into the world of non-linear models came about through the perseverance¹⁴ and the decisive influence of Le Corbeiller, who, as noted, personally tutored Goodwin on the non-linear theory of oscillations. In his own recollection of how he came towards building his model, Goodwin says:

Both Frisch and Tinbergen had failed to pay adequate attention to a short note in an early issue of *Econometrica* to the effect that to explain a self-generated cycle, it was necessary to have an unstable equilibrium with a pair of non-linearities, in the outer regions of the state space, to convert the instability into global stability. During the war I had the accidental good fortune to work in the same laboratory as the author of the note, Phillippe Le Corbeiller, and he taught me what I needed to know about maintained oscillation (Goodwin 1988: 20; see also footnote 4 above).

¹²That it was not published earlier was entirely due to the fact that Schumpeter’s untimely death, in January 1950, intervened. Samuelson’s handsome acknowledgement of Goodwin’s priority in this field was expressed in his letter to me, dated 12 September 1996 (italics added): ‘I grieve that Hicks got a lot of credit for non-linear cycles, even though he had learned much from *earlier work by Dick* [Goodwin]’.

¹³This criticism was incorrect, as pointed out by Ichimura (1955: 217, fn. 28): ‘Mr. Harrod combines the multiplier and the accelerator, which latter is made nonlinear by reason of the following effects of the changes in the level of output on the acceleration coefficient: (1) the influence of the rising rate of interest in the upswing and that of the falling rate in the downswing; (2) the changes in the relative prices of capital goods; and (3) the variations in profitability due to the law of diminishing returns and the elasticity of demand’.

¹⁴In a footnote in his famous paper, Goodwin says: ‘My debt to Professor Le Corbeiller is very great, not only for the original stimulation to search for the essential nonlinearities, but also for his patient insistence, in the face of the many difficulties which turned up, that this type of analysis *must* somehow be worked out’ (Goodwin 1951a: 2; italics in original).

In Hicks (1950), exogenous autonomous growth factors are superimposed on to a disequilibrium model of fluctuations.¹⁵ Unlike this, or the shock theories which superimposed exogenous, random shocks to an equilibrium model of growth, Goodwin took an alternative path. He realized that the ‘the inherent instability [of] the accelerator principle could be dampened by the multiplier mechanism in a model in which the equilibrium was locally unstable but globally stable’ (Velupillai 1998: 1,439). To achieve this, he combined the dynamic multiplier with a non-linear accelerator and weaved it together with Schumpeter’s theory of how innovations from within the system drive growth. The ‘steady evolution of ideas leads to intermittent, *irregular, but not random*, bursts of expenditure’ (Goodwin 1951a: 3; italics added) and it supplied the energy to the system for the ‘maintenance of oscillation’. The famous Goodwin (1951a, 1967) models have become part of the folklore of mathematical macrodynamics and are very much a part of textbook exposition (the clearest of which, in my opinion, is Gandolfo 1981). Instead, I want to emphasize a different aspect of Goodwin’s *Occamist credo*.

Over a research and teaching career spanning more than 60 years, Goodwin relied on the following systems of non-linear, ordinary, differential equations for his contributions to NENAF theory:¹⁶

1. The van der Pol equation: $\ddot{x} - \tau(1 - x^2)\dot{x} + x = 0$
2. The generalized forced van der Pol equation: $\ddot{x} + f(x, \dot{x})\dot{x} + g(x) = \mathcal{O}(t)$
3. The Goodwin–Le Corbeiller equation:
 $\ddot{x} + \rho(-\exp(\dot{x}) - 2)\dot{x} + x = 0 \quad 0 < \rho < 1$
4. The Rayleigh equation: $\ddot{x} + \gamma\left(-\dot{x} + \frac{\dot{x}^3}{3}\right) + x = 0 \quad 0 < \gamma < \infty$
5. The Lotka–Volterra equations:
 $\dot{x} = (\alpha - \beta y)x, \quad \dot{y} = (\delta - \mu x)y \quad 1 \geq \alpha, \beta, \delta, \mu > 0$
6. The Rössler equations: $\dot{x} = -y - z, \quad \dot{y} = x + \sigma y, \quad \dot{z} = \varepsilon + xz - \vartheta z$

¹⁵The role of an autonomous, exogenously specified level of investment in Hicks (1950), which explains the lower turning when the system hits the floor. But, as Goodwin observed (1994: 77; italics added): ‘When output has fallen, leaving general excess capacity, there is no reason to invest and *the accelerator is dead*. Moreover, the piecewise linear Hicks model is capable of generating unstable, globally attracting, equilibria (cf. Sedaghat 1997: 357).

¹⁶Goodwin rarely—but not never—resorted to non-linear, difference equation, NENAF modelling, reflecting, perhaps, the apprehension he perceptively noted (Goodwin 1950: 319, fn. 6): ‘Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusion as to behavior’.

In the above equations, the lower case Greek letters denote parameters and the lower case Latin letters signify ‘variables’; in 1, 2, 3, and 4, the variable x signifies the level of output; in 5, the variables u and v signify the aggregate share of wages and the unemployment ratio, respectively; and in 6, Goodwin used x and y in the same sense as u and v in 5, whereas z signified some version of a Schumpeterian innovation variable.

Eventually, Goodwin relied almost exclusively on using just the Goodwin–Le Corbeiller and the Rössler systems for all his macrodynamic modelling exercises. This choice was guided and directed by what I have referred to as the *Occamist credo* in his methodology. They can encapsulate the richness exhibited in aggregate economic data whilst relying on only one non-linearity, as he liked to assert. In the case of the Goodwin–Le Corbeiller system, this meant that the S-shaped cubic characteristic of the standard two-dimensional oscillators of the Rayleigh–van der Pol variety was replaced by the one-bend exponential characteristic of Goodwin–Le Corbeiller oscillators.¹⁷ Given the emphasis I have placed on Goodwin’s *geometric* approach, it might be useful to show what a characteristic with one non-linearity (the Goodwin–Le Corbeiller system, above) looks like and the resulting limit cycle it generates (where y denotes the level of aggregate output) (Figures 36.1 and 36.2; see next page).

3 Coupled Dynamics, Optimal Planning, Stabilization Policy, and Iteration

‘If you can get [the World Bank] to use *Eigenvectors* and *Eigenvalues*, I will privately award you a Nobel!! No one ever seems to see the great advantages of *separating dynamics from interdependence in large systems*’ (Goodwin to Velupillai, 5 July 1993; italics added).

Every contribution by Goodwin, to the problem of ‘interdependence in large systems’, from the problem of dynamical coupling in 1947 (Goodwin 1947) to the problem of value and distribution theory in Sraffa-like systems (Goodwin 1974, 1977), and their dynamics, was by means of one or another form of decoupling with the use of coordinate transformations. He used engineering terminology here—normal coordinates or normalized general coordinates,

¹⁷ Curiously, Goodwin never tried to generate, by actual simulation, the limit cycle embedded in the Goodwin–Le Corbeiller equations. Had he done so, he would have realized that the system would have to be integrated backwards in time to generate the limit cycle! This is because it is an unstable limit cycle. Of course, a ‘trivial’ change of sign for the parameter in the system leads to the generation of a stable limit cycle. However, whether the ‘trivial’ change of sign preserves fidelity with the economics underpinning the models he constructed is a much more difficult question to answer, especially in a footnote.

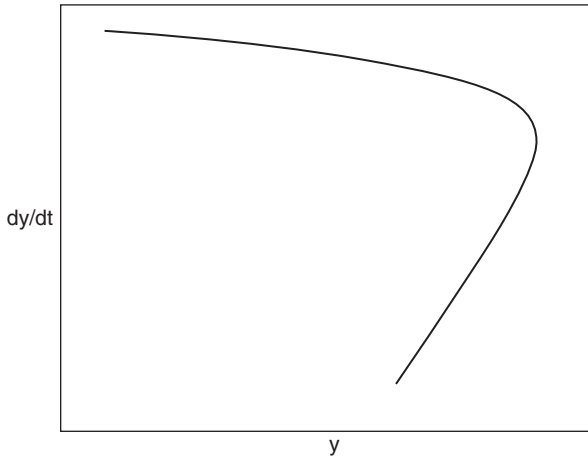


Figure 36.1 The 'one-bend' characteristic

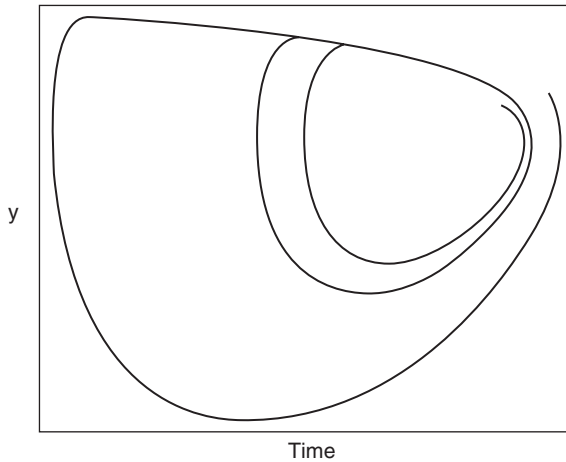


Figure 36.2 A limit cycle generated for a 'one-bend' characteristic

unilateral coupling,¹⁸ and so on—to harness deep theorems in matrix theory (or the theory of linear transformations) to solve, iteratively, for possible stable, equilibrium values of such large interdependent systems.

¹⁸Simon's felicitous use of this concept (e.g. in Simon 1952), to develop a rich repertoire of concepts on causality, identifiability, semi-decomposability, evolutionary dynamics—and much else—is a testimony to the fertility and originality of Goodwin's innovative theoretical contributions, even within the framework of linearity.

Thus, his pioneering invoking of the Perron–Frobenius theorems; the use of the Duhamel, Faltung, or convolution sum; and the mastery of similarity transformations (from Matrix Theory) were all with the aim of making explicit an iterative mechanism that could be given some kind of algorithmic form. Whether it was for the purpose of computing the dynamic multiplier in a Leontief system (Goodwin 1949a), developing an explicit iterative procedure for a meaningful Walrasian *tâtonnement* (Goodwin 1953), or determining the standard system and the standard ratio in a Sraffa-like system (Goodwin 1974, 1977), or, above all, calculating, transparently and pedagogically, stabilization regimes in Keynesian macrodynamic models, using the Phillips Machine (the MONIAC; see Goodwin 2000), it was always for enlightened policy purposes on the basis of rigorously approximate theory.

It was, in fact, in this remarkable paper (Goodwin 1953) linking Walrasian *tâtonnement* with an iteration that would converge, that the Frobenius generalization of the Perron result was first reported, and used (contrary to the statement in Desai and Ormerod 1998: 1,433). It was ‘discovered’ by Göran Ohlin, a student in Goodwin’s graduate classes at Harvard (see Goodwin 1953: 83, fn. 7). The footnote, in Goodwin (1953), referring to Ohlin has the following interesting background. In a letter to Björn Thalberg dated 5 October 1976:¹⁹

I had no idea that you [Thalberg] met Richard Goodwin in Cambridge. He was really a wonderful teacher. My acquaintance with him was unfortunately relatively short since he disappeared to Cambridge [UK] quite soon after I came to Harvard. I did not have any special status as his student, except in one sense – he formulated a problem (I seem to recall that it was about showing that there was only one real root to the characteristic equation for a certain type of matrix) and promised, somewhat jocularly, and in a lighthearted fashion, an *A* to anyone who could solve it, and I did it, which resulted in the footnote about me.

Unlike the dynamic way Goodwin used, for example, the Perron–Frobenius theorems on non-negative square matrices, in conjunction with similarity transformations into normalized general coordinates, economic theorists with a mathematical bent have appealed to these theorems for proving pure existence theorems. Similarly, the geometric basis of Sraffa-like systems was highlighted by the same kind of coordinate transformations

¹⁹ A ‘free’ translation, by me, from the original Swedish version of the letter, which was made available to me by the late Professor Thalberg, in 1976.

to determine conceptually slippery terms like the standard ratio or the composition of the standard commodity.

Two neglected points in relation to Walrasian *tâtonnement* should be observed, and they are best highlighted by Goodwin's stubborn and persistent emphasis on the difference between proving pure existence theorems and coupling them to methods for finding solutions, on the one hand, and whether or not the hypothetical method for finding solutions—the algorithm—was meant to be the actual process by which the heterogeneous agents or institutions 'groped' towards the solution that was proved to exist, on the other. Goodwin never separated these two issues, and in this he was unique even in a Cambridge dominated, in his time there, by the first-generation pupils of Maynard Keynes. Perhaps this was because he remained a Schumpeterian, even while becoming an adherent of Cambridge Keynesian and Sraffian economics and methodology.

In any case, as he noted (Goodwin 1953: 59–60; bold text and italics in original):

[Walras] thoroughly confused two related but distinct questions: the question of the *existence of a solution*²⁰ and *how to find it*, with the question of the reality of a *dynamic process* and its stability ... I incline to the opinion that he *wished merely* to show the existence of a solution and to assert that this is the actual one realized, but **not** the hypothetical process by which we might discover this solution is the same as the motion by which an actual economy reaches equilibrium. I find support for this in Schumpeter's statement that 'I remember a conversation with Walras in which *I tried but completely failed to elicit the slightest symptom of interest both in dynamical approaches and in a theory of economic evolution*.'²¹

Walras, actually, 'thoroughly confused', not two, but *four* 'related but distinct questions': *existence of a solution*, a *method of finding it* (if 'proved' to exist), the 'reality' of the *method* considered as a *dynamic process* and its *stability*;

²⁰ Goodwin was also always careful to choose the phrase 'existence of a *solution*' rather than the more orthodox 'existence of an *equilibrium*'. Had this distinction been as carefully observed by computable general equilibrium theorists, the nonsensical constructive and computable claims they—and their dynamic stochastic general equilibrium (DSGE) followers—have been making would have shown to be the true *non sequiturs* they actually are.

²¹ In a footnote to this Schumpeterian point, Goodwin adds (*ibid.*: 60; italics in original): 'From the unpublished manuscript of his *History of Economic Analysis*'. My reading of this monumental text by Schumpeter (1954) does *not* show any evidence of this important paragraph on his meeting with Walras at least in the *published* version of the *History of Economic Analysis*. Modern readers of this classic should, perhaps, be reminded that Goodwin was the one who 'put together...the material in Part IV, Chapter 7', that is, the chapter on *Equilibrium Analysis* (see the concluding paragraph in the 'Editor's Introduction' by Elizabeth Boody Schumpeter).

the latter could be rephrased in terms of a question about the convergence of a dynamic process to the equilibrium which had, apparently, been shown to exist. The separation between a proof of existence and a method for finding the solution (or equilibrium) shown quite separately to 'exist' is characteristic of the kind of mathematics that dominates orthodox mathematical economics. Goodwin never indulged in this kind of separation. In this sense, he was an intrinsic computable and constructive economist.

A legion of pioneering contributors to economic theory, all the way from Bortkiewicz and Edgeworth, via Pareto and Pantaleoni, to Lange, Patinkin, Morishima, and those two modern scholars par excellence of Walras, William Jaffé and Don Walker, have contributed to furthering the Walrasian confusions. Between Jaffé (1967, 1980, 1981) and Walker (1987, 1988), all the relevant historical references, related to the 'thoroughly confused' issues which Walras raised, are meticulously listed, discussed and interpreted, even if not always with the admirable clarity one expects from these two outstanding doctrinal scholars.

For almost the first decade and a half of his academic life at Cambridge, Goodwin was the custodian of the Phillips Machine, which he maintained and used for his lectures, as Michael Kuczynski (2011: 97, 99–100), a distinguished auditor of his lectures recalled, with considerable affection:

Goodwin's lectures were on the 'national income machine' [the Phillips Machine] and delivered in [what is now called the Meade Room] ... Phillips' bulky machine always stood there, despite its rollers, its brown wooden frame locked in a heavy glass cabinet whose awkward doors Goodwin would prise open at the start of each lecture.

At the start of each lecture Goodwin would set one or other of the levers controlling the functional relationships (tax to national income, imports to reserves, etc.), and then = as the flow got going—he would scamper around stylishly calibrating the others till the flushing and cascading of the liquid flow found its equanimity and the gurgling settled down to a quiet stream.

Above all, the imaginative way Goodwin coupled two Phillips Machines, with the help of Phillips himself, to generate what is now referred to as the quasi-stable paradox, gave transparent and pedagogical content to stabilization regimes and policies. As he wrote in the appendix to a letter addressed to Professor—now Lord—Nicholas Stern (Goodwin 1991; italics added):²²

²²To the best of my knowledge this appendix has never been published in its entirety.

[O]n coming to Cambridge, England from Cambridge, Mass., [imagine] my thrill to find Phillips's extraordinary machine installed; I spent years using it for teaching both linear and nonlinear dynamics—and some time keeping it in reasonable working order (occasionally with help from Phillips).

I had long known about computers and had even written an article (Goodwin 1951b), which no one seems to have read, comparing the price-market economic [sic] to a gigantic computing machine. But I had never had access to a computer, so one can imagine my pleasure and thrill at having under my control that astonishingly effective electronic, mechanical, hydraulic Phillips's contraption. One could draw an arbitrary nonlinear curve and make it *solve it*; also, I found that accidentally the machine embodied what I had formulated as a 'flexible' accelerator in place of the rather unsatisfactory simple accelerator.

Furthermore, I was very excited to find that *Phillips had two of his magical machines* in London, so I could reproduce what I had analyzed back in 1947 in my dynamical coupling paper (Goodwin 1947). If I remember correctly, Phillips did not believe we could produce *erratic behaviour* by coupling his machine – but we did.

Zambelli (2011) has comprehensively analysed the coupled dynamics of Phillips Machines, using macrodynamic models of generalized Goodwin-type dynamic multiplier, flexible accelerator, non-linear interdependent economies linked non-linearly. Goodwin's intuition of being able to 'produce erratic behavior by coupling machines' were fully confirmed by Zambelli's simulation results.

In the same period, mid-1950s to mid-1960s, Goodwin also lectured on Indian Planning, reflecting on, and based upon, his work at the Indian Statistical Institute, constructing India's first input-output tables for use in that country's second Five-Year Plan (cf. Velupillai 2015). He was, as a result of his deep interest in the development of the Indian economy, also the pioneer in the resurrection of Ramsey economics, that is, the application of calculus of variations, and its modern extensions. This came about as a result of his interest in determining the optimal path of development (cf. Goodwin 1961,²³ and for the full details, also Velupillai, op. cit).

I give these three examples—the preoccupation with the practical meaning of a *tâtonnement*, but underpinned by computational, simulational, and rigorously approximate theory; stabilization policy, investigated experimentally and with simulations on an analogue computer (which the Phillips Machine was); and the problems of underdevelopment, particularly of an important emerging nation such as India, based on actual experience of constructing tables of inter-industrial transaction flows—just to highlight that Goodwin

²³In addition, see some of the essays republished in Goodwin (1982).

never worked or speculated on pure theory in a vacuum. Theory was always the servant, especially mathematical theory; applied theory was the master, particularly with practical, macroeconomic, policy in mind.

4 Conclusion

‘I’m a Sunday mathematician’, says Goodwin. ‘I have no aptitude for math’. (‘Ha!’ says Le Corbeiller. ‘Remember, whatever Richard Goodwin tells you about himself, that is by twelve shades an understatement...’)

Richard Murphey Goodwin is leaving Harvard at 38, having spent half his life here...

Beyond a year at Cambridge University his plans are uncertain, but *whatever he will be doing in the future will include teaching* (Ellsberg 1951; italics added).

Mercifully, especially for those of us who had the pleasure and privilege to be his students, Goodwin never stopped lecturing—for the rest of his life. His lectures were works of art. The most intricate of economic propositions were explained with beautiful, freehand, geometric constructions on the blackboard, with mechanical constructions on the floor of the classroom (using, as mentioned above, the celebrated Phillips Machine) or, in the last decade of his life, computer graphics. These innovative teaching methods, far ahead of their time, took even the novice to economic theory literally by the hand to its frontiers.

The student did not often realize that underlying these geometric, mechanical, and computer devices and displays were the sophisticated mathematical theories associated with legendary mathematicians: Frobenius and Perron; Rayleigh and van der Pol; and Pontryagin and Bellman. We felt, as students, that we were being introduced to the art of building economic models to understand the way the great theorists, from Ricardo and Marx to Schumpeter and Keynes, grappled with attempts to devise economic concepts to tame the unruly stylized facts of industrial economies.

Little did we realize that we were being exposed to the arcane and unteachable art of pure research.²⁴ But we were left in no doubt that here was a master who combined respect for the wisdom of the classics with an understanding of the contours of experience to temper the forging of tools to learn from

²⁴ As Solow (1990: 32; italics added) recalled, with charm and fondness: ‘I may be inventing this, but I seem to recall that [Goodwin] sometimes suggested that, well, one could not actually believe this or that, but it was an ingenious line of thought, perhaps worth following just to see where it came out. One could always reject it later, and then one would have a better idea of what one was rejecting. *If that actually happened, then I was getting my introduction to the theorist’s frame of mind*’.

them. These three pillars, the wisdom of the classics, the contours of experience, and the mastering of the use of tools, were the foundations on which legions of students from all corners of the world were encouraged to build their education as economists.

In ending the Preface to his remarkably original *Elementary Economics from the Higher Standpoint*,²⁵ Goodwin (1970: x) wrote:

Anyone interested in the intellectual origins of the work will find more sources in my teachers than in my erratic and scanty reading. Particularly to be mentioned are Schumpeter, Leontief, Le Corbeiller, and Joan Robinson, who have all, each in a very different way, had a strong influence on my thinking, though never quite to the point of emulation.

That was in 1970; by the end of his life, a little more than a quarter of a century later, he would—I am sure—have added Ralph Abraham, Richard Day, and Otto Röessler. These three distinguished additions to the illustrious four above are themselves a sign of Goodwin's almost complete absorption in the mathematics and economics of chaos—as anyone who reads his works on non-linear dynamics in his post-Cambridge period would realize.

Richard Goodwin's intellectual legacy seems, alas, to be confined to analysis and a variety of extensions to the *Growth Cycle* (Goodwin 1967) in the share of wages and the unemployment ratio. Few know the origins of the way he was led to formulate his Lotka–Volterra model (cf. Velupillai 2015). Not too many who wishfully and enthusiastically use the *Growth Cycle* as the starting point for their analysis of the dynamics of class struggle have any knowledge of his *Occamist credo* which led to his adoption of the Röessler Band—and three dimensions—as his chosen framework for modelling the dynamic contours of capitalism's complex evolution.

Goodwin was fully familiar with the modern contributions to dynamical systems theory; even more importantly, he read, worked through, and 'internalized' the important analytical messages on structural stability (Andronov and Pontryagin 1937) and the way Kolmogorov (1936) generalized the Lotka–Volterra model to remove its infelicities.²⁶

²⁵In my personal copy of this wonderfully original book, he wrote: 'Vela, his very own copy, not presented by, but blessed by me the poor scribbler of it. Dick Goodwin, Sept 29 1977'. This inscription was written when he was staying with me, at my home in Svanshall, at the time of the Wicksell Symposium, held in Frostavallen.

²⁶I know this because it was I who provided him with, first, the Italian original of Kolmogorov (1936), and, then, an English translation of this classic prepared for me by my good friend, and a fellow Goodwin pupil, Professor Guglielmo Chiodi.

My own opinion of his lasting legacy is based on a quarter century of friendship—first as a student, then as a friend and mentor and, finally, even as a colleague. They are based on his firm belief, and guiding principle in modelling, of the *Occamist credo* and amount to the following four items, which span and delineate what can be called a research programme: the importance of the flexible accelerator in macrodynamic modelling; the significance of a Schumpetrian theory of innovation; the role of demand, in a strictly Keynesian sense, in dynamics; and the coupled oscillatory behaviour of interdependent economies, studied simulationally—and, hence, experimentally in the sense of Fermi et al. (1955). I believe that Zambelli (2010) is the only exercise that focuses towards a beginning of the realization of Goodwin's innovative research programme.

Goodwin was a painter of considerable talent and reputation. His paternal aunt, Helen Goodwin, was an impressionist painter of distinction, from whom as a young boy he first learned the rudiments of painting. Later, during his years as a Rhodes Scholar, he also spent one year at the Ruskin Art School in Oxford. His paintings, reflecting perhaps his passion for the vividness of Tuscany and parts of North India, as well as his own beloved home in England, were notable for the dominant effect of colour and abstract, almost mathematical, form. They could, perhaps, be described as abstract expressionist paintings, although he would have resisted any such classification.

One of the greatest pleasures of his life, he once told me, was to have as students in his class on business cycle theory, in the Harvard of the late 1940s, two of the giants of the subject: Joseph Schumpeter and Gottfried Haberler. When the issue of his tenure came up, Schumpeter is reputed to have told him that he could count on only two 'sure votes': his own and that of Haberler; the two European émigrés, not known for being particularly liberal in their political beliefs, supporting the active, passionately left-wing academic out of a commitment to intellectual freedom. He was denied tenure.

Goodwin was a modest man with exquisite tastes and wide interests. He read German, French, and Italian with great ease. He was a celebrated wine connoisseur, had a magnificent personal cellar, and also managed the cellars at Peterhouse with distinction for many years.²⁷ On the other hand, he had, as John Kenneth Galbraith poignantly noted, 'a morbid lack of interest in the ordinary manifestations of material well-being'.

He divided his last years between winters in India, summers in England, and springs and autumns in Italy. His years in Italy, after retirement from Cambridge in 1980, were perhaps the happiest ones. It was fitting that death, when it came, on 6 August 1996, found him in Siena.

²⁷When I had successfully completed my doctoral dissertation, under his supervision, at Cambridge in 1979, he presented me with a Château Léoville-Barton, 1949.

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Richard Stone (1913–1991)

Terry Barker

1 Introduction

Richard Stone was an applied economist and econometrician of great distinction, who is regarded as the father of the international system of national accounts (SNA). He was awarded the Nobel Prize in Economics in 1984 in recognition of his ‘fundamental contributions to the development of national accounts’ that ‘greatly improved the basis for empirical economic analysis’. His work combines theory and data, and he emphasised that the essential feature of economics as a discipline is it should be grounded in both. The national accounts in their modern systematic form are the basis of the empirical modelling, testing, and understanding of Keynesian macroeconomics.

Stone is an economist in the Cambridge tradition in that his work developed the theories of John Maynard Keynes, Richard Kahn, and Nicholas Kaldor by building and estimating macroeconomic models. He was based in Cambridge throughout most of his professional life. He was the first Director of the Cambridge Department of Applied Economics (DAE), 1945–1955, the P.D. Leake Professor of Finance and Accounting, 1955–1980, founder

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of the Cambridge Growth Project (CGP) with Alan Brown in 1960, and Honorary President of Cambridge Econometrics,¹ 1979–1991.

His Life²

John Richard Nicholas Stone was born in England on 30 August 1913, and died on 6 December 1991. He was educated at Westminster School, 1926–1930, and Gonville and Caius College, Cambridge, 1930–1935, initially studying law, before switching to economics in his second year, graduating with a First in 1935. He was an undergraduate at a time when the ideas underlying *The General Theory* were being discussed and formulated. His supervisors were Richard Kahn at King's for two terms, then J.W.F. Rowe of Pembroke, and in his last year Gerald Shove of King's. He eventually became a Fellow of King's College in 1945. One of his lecturers was Keynes when he was developing the draft of what became *The General Theory*. He was invited by Keynes to join the Political Economy Club and presented a paper on effective demand versus production frictions. He remarked that, 'Of my teachers, the two best on the theoretical side were Richard Kahn and Joan Robinson, but without doubt the greatest influence on me came from Colin Clark, who at the time was teaching statistics to economists in Cambridge' (Stone quoted in Pesaran 1991: 88). Clark became a good friend, one of many over the years, and as well as teaching he was working on estimates of the UK national accounts, with his major book on the topic published in 1937.

After university, Stone started his career in the City of London working for Heath and Company, an insurance broker at Lloyds, 1936–1939. Outside the office, on the suggestion of Colin Clark, he took over, with his first wife, Winifred Mary Jenkins, the production and editing of an economic commentary *Trends*, which appeared in the monthly *Industry Illustrated* [2, 4, 7].³ He divorced his first wife in 1940 and married Feodora Leontinoff in 1941. When the Second World War started, Stone was recruited to work in government in the Ministry for Economic Warfare and later in the Cabinet Office. He settled in Cambridge after the war but his second wife fell ill and died in 1956. He married Giovanna Croft-Murray in 1961. They had earlier

¹ Cambridge Econometrics is a successor company to one founded in 1976 by members of the CGP to provide a commercial service based on the application of the Project's economic model. It is now owned by the Cambridge Trust for New Thinking in Economics, founded in 2005; see www.camecon.com.

² His life and contributions are documented and appreciated in the many sources consulted for this review; see Johansen (1985), Deaton (1987, 1993), Pesaran (1991), Pasinetti (1992), Pyatt (1992), Goodwin (1995), Pesaran and Harcourt (2000), Weale (2004), and Baranzini and Marangoni (2015).

³ All references to Stone's work are shown in square brackets, for example, as [1], where the number, here 1, refers to the chronological list in Pesaran (1991: 112–123).

co-authored the book *Social Accounting and Economic Models* [67] published in 1959. Giovanna continued to support him in his work, and their home on Millington Road, Cambridge, was a haven for scholarship, good living, and lively conversation. After his death, she prepared for publication his final set of lectures, the magnificent *Some British Empiricists in the Social Sciences, 1650–1900*, published by Cambridge University Press in 1997 [198].

One incident in 1940 would have established Stone's reputation as a statistician worth knowing. After he was given the job of recording shipping imports of neutral countries, he recounts:

[T]he only ships worth recording were tankers. One knew their speed and capacity and they carried a limited range of products of great importance in war time ... We filled in my tanker index day by day and it became clear that Italy, which had not yet entered the war, was a big importer. Suddenly, in the second half of May a dramatic change took place. The Italian tankers, which up to then had been moving in a predictable way, changed course and began to steer north or south. They must be making for the nearest neutral ports, we thought ... We had to guess which port each ship was making for but we knew their speeds and decided by the end of May that they would all have reached their chosen destination by 10 June. On that day we thought that Italy would declare war ... The Italophile section of the Foreign Office refused to believe me and I was reprimanded for my presumption. Unfounded suspicions, they said. Italy was a delightful country and a firm friend. What if it imported a lot of oil; it was a Catholic country and needed a lot of paraffin for altar candles. However, Italy did declare war on the day we had foreseen (Stone quoted in Pesaran 1991: 91).

The UK National Accounts

In 1940, at the suggestion of Austin Robinson, Stone transferred to the Cabinet Office to work with James Meade to produce national accounts to underpin the wartime finances. At one point, his superior moved him from working on the prototype National Accounts to oil statistics. Stone had the opportunity of telling Keynes about this move, who said, 'I'll soon stop that', and subsequently took on Stone as his statistical assistant. Stone comments that 'this saved the national income [work] and in all respects was a very good arrangement since Keynes showed immense interest in the development of the work' [167: 70–71].

By the end of 1940, Stone and Meade had prepared estimates for 1938 and the four quarters of 1940. Keynes at the Treasury became interested because earlier in the year he had included estimates of national income for 1938–1939 in his 1940 report on *How to Pay for the War*. It was during this period that Stone got to know Keynes well, working with him through the rest of the wartime

period and he was with him in the USA during the American loan negotiations. The Meade–Stone estimates became the first UK National Accounts and were published alongside the 1941 Budget, with a paper on them in the *Economic Journal* in 1941 [8]. The Central Statistical Office (CSO) was established in 1940 and Stone continued there to develop the accounts. He soon linked up with statisticians in the USA, who had a longer history of compiling national accounts, and he reconstructed the US data to compare with the UK accounts and definitions that he and Meade had established.

The Foundation of the DAE

Stone was appointed the Director of the newly founded DAE in the University of Cambridge in 1945. The DAE had been established by statute in 1939 with a budget of £2,500, after the University accepted a proposal put forward by a committee consisting of Keynes, Piero Straffa, Joan Robinson, Austin Robinson, Gerald Shove, and David Champernowne. The purpose of the new department was ‘to foster economic research, particularly in the descriptive and statistical fields’. However, the start of the war led the University to delay the appointment of a Director and staff until the end of hostilities.

The University’s General Board established the DAE in a report dated 9 May 1945, with the objective of the Director being ‘to advance knowledge in his subject, to promote and direct research in it and to supervise the work of the Department under the general control of the Committee of Management’. The Faculty Board proposed Stone as Director with the strong support of Keynes and that ‘in view of his special qualifications, the appointment should be until the retiring age’. The General Board accepted this and made the appointment. The post had the status of a University Officer, with the salary of a Lecturer, and the residential and teaching conditions of a Reader or Professor. Stone was 32-years-old but he was appointed until retirement with no explicit restriction on him becoming a Professor at a later stage, as was normal with sufficient academic distinction for a University Lecturer. The ambiguity inherent in the status of the post and these conditions was to lead to problems later.

He brought to the new DAE some of the projects he had started at the National Institute of Economic and Social Research (NIESR), founded in London in 1938, and so began the Cambridge work on national accounting and econometrics, as discussed below. The funding for new research came from the Rockefeller Foundation, which had been approached by Keynes in 1942.⁴ The Foundation offered to ‘give prompt and preferred examination to a pro-

⁴ Keynes to Kittredge, Rockefeller Foundation, New York, 1 May 1942, E.A.G. Robinson Papers, Marshall Library, Cambridge University: Box 36.

posals sponsored by professor [sic] Keynes and to be submitted by Cambridge University upon the approximate termination of the war'.⁵ As it became clear that the Axis powers were being defeated, negotiations were reopened and funding was forthcoming.

Stone's recruitment policy was to build up staff slowly and carefully and to cast the net widely. He spent three months at the Institute of Advanced Studies in Princeton in 1945 just before taking up the DAE post, working on national accounting problems and establishing links with others interested in the issue. In July 1948, he visited the East Coast of the USA⁶ to arrange the first General Conference of the International Association for Research in Income and Wealth (IARIW), founded in September 1947,⁷ which was to be held in Cambridge in 1949. Stone took the opportunity to meet many economists who were or became highly distinguished in the discipline. On his 1948 visit, he interviewed Tom Schelling at Harvard, met Paul Samuelson at the Massachusetts Institute of Technology (MIT), Simon Kuznets and Morris Copeland in New York, and visited Arthur Smithies in Washington, D.C. By 1948, he had built up the size of the DAE to six senior and four junior research staff and seven assistants.

As Director, Stone made the new DAE a renowned international centre, attracting young statisticians and economists as staff⁸ or visitors.⁹ The intellectual atmosphere of the fledgling Department was innovative and vigorous. The venture was a new beginning after the war, with plans to establish applied economics as a scientific undertaking. Stone was able to recruit some of the most able, energetic, and talented of the new generation of economists and statisticians. However, the Department was physically separated from the rest of the Faculty of Economics and Politics and there was not much interaction between the different groups.¹⁰ Typically, Stone did not take sides

⁵ Willits to Keynes, Rockefeller Foundation, New York, 18 June 1942, E.A.G. Robinson Papers, Marshall Library, Cambridge University: Box 36.

⁶ Richard Stone Papers, King's College Archive Centre, Cambridge University: Box JRNS 4/11.

⁷ Stone was a founding member of the Council of the IARIW and the first Chairman, 1949–1951 (see Carson 1999).

⁸ Staff in the DAE during Stone's tenure as Director included Sydney Afriat, Mike Farrell, T.W. Anderson, J.S. Cramer, Phyllis Deane, James Duesenberry, Roy Geary, Alan Prest, Richard Brumberg, Sig Prais, Hendrik Houthakker, James Durbin, Geoff Watson, Don Cochrane, and Guy Orcutt.

⁹ Visitors included James Tobin, T.W. Anderson, Larry Klein, Wassily Leontief, Tjalling Koopmans, Ragnar Frisch, Gerhard Tintner, and Geoff Watson (of Durbin–Watson fame), an Australian writing a thesis as a student of the University of North Carolina.

¹⁰ Geoff Watson remembers: 'There were no departmental boundaries where I was concerned, but the joke used to be that this was an applied economics group. We weren't allowed in the door of Economics. All the economists were anti-mathematical. They believed you had to do it with words, which was bloody hard. You have to be very clever to say all these things, for example marginal utilities—quite hard to define in words but mathematically trivial. In fact, the economists thought that Richard Stone was so subversive they made this little extra Department to keep him out of theirs. There were two or three

or engage with controversies that continued to divide the economists of the Cambridge Faculty.¹¹

When there was an offer of funds for Stone to occupy the P.D. Leake Chair in 1952, this was taken as an opportunity by some in the Faculty Board to impose a new condition on the Director, that he or she could not also hold a chair. Stone strongly objected, arguing that he would not have accepted the post under this condition. He was forced to choose between remaining Director and taking the Chair. He took the Chair, resigning as Director, but continued to work with a team in the DAE until he retired in 1980.

An Outline of This Chapter

This chapter continues with a discussion of Stone's approach to theory and data in Section 2, including his contribution to national accounting for which he won the Nobel Prize. A short review of his contribution to the economics of consumer demand follows in Section 3 and to macroeconomic model building in Section 4. Section 5 is an assessment of his specific contribution to economics at Cambridge and Section 6 concludes.

2 Economic Theory and Data

The Role of Theory and Data

Stone's approach was that economics is an empirical science:

It seems to me that the development of a science requires attention to both facts and theories and I agree with Marshall that economic theory is as mischievous an imposter when it claims to be economics proper as is mere crude unanalysed

people in Stone's group who subsequently got Nobel Prizes and every one of the first 15 Nobel Prize winners in economics visited Applied Economics' (Watson quoted in Beran and Fisher 1998: 77).

¹¹ Alan Prest was Lecturer in the Faculty and Fellow of Christ's College after joining the Faculty in 1945 as a founding researcher in the DAE. His brother Wilfred Prest, visiting him in 1953, wrote in a memorandum: 'I was rather shocked to find the Faculty there deeply divided on doctrinal, political and, indeed, racial grounds. On the one hand there is the Robertson party comprising, in addition to Sir Dennis himself such diverse characters as Guillebaud, Richard Stone, R.F. Henderson and S.R. Dennison. This group has never quite accepted Keynes without reservation and its members are inclined to be conservative, politically and socially. On the other hand, there is the Kahn party comprising, in addition to Professor Kahn, Sraffa, Kaldor, Dobb, Rostas, Joan Robinson, Ruth Cohen and Harry Johnson. This group is neo-keynesian [sic] in outlook and is well to the left politically ... The conflict between the two parties is deep and bitter' (report of visit to Britain, January–March 1953, Wilfred Prest Papers, University of Melbourne Archives, quoted in <http://www.vu.edu.au/sites/default/files/cses/pdfs/millmow-paper.pdf>: 3).

history. I cannot imagine why anyone should think otherwise or why economists should tend to put theorists on a pedestal. The real difficulty is to combine the two so that theory can be used to interpret facts and facts can show what has to be interpreted (Stone quoted in Pesaran 1991: 89).

Stone made his own approach to data and theory clear in his proposals for the research to be undertaken under his direction at the foundation of the DAE. He proposed that the research should concentrate on the ‘three tasks of applied economics: the work of observation, i.e. the discovery and preparation of data; the theoretical appraisal of problems, i.e. the framing of hypotheses in a form suitable for quantitative testing; and the development of statistical methods appropriate to the special problems of economic data’. He added: ‘I do not believe that applied economics can be fruitfully developed without a synthesis of all the three types of study ... The most successful observational work is done under the guidance of theoretical ideas’. However, ‘the reformulation of theory to provide hypotheses for quantitative testing is in a very rudimentary state’.¹² The great achievement of the Department, and Stone’s own work on the behaviour of consumer demand, was in such a reformulation and testing.

Later he stated his own position in relation to others in the field:

Practitioners [of economics] can be grouped into three types: the speculative, the active and the inquisitive. The speculative look at the machine and try to interpret its workings from the signals it emits; the active do not like the way it appears to work, and devise improvements; the inquisitive try to take it to bits and see how it actually works ... If I were inclined to speculation, I would talk about advances in theory. If I were an activist I might talk about welfare economics or taxation policy or Keynes or perhaps even Marx. Being merely one of the inquisitive ones, I shall talk about the areas I have enquired into and the tools I have used for the purpose: more specifically about quantitative analysis, economic dynamics and what, for want of a better term, I shall call social econometrics [179: 723].

Econometrics and Keynesian Economics

Stone had many interactions with Keynes during the war when he was working on successive budgets and national accounts at the newly formed CSO,

¹²Note by Stone dated 13 June 1945, in the E.A.G. Robinson Papers, Marshall Library, Cambridge University: Box 36.

which sprung out of the Cabinet Office in 1943. Unlike many other economists in government at the time and in Cambridge later, he was concerned with measurement and model building rather than short-term economic advice on the issues of the day. In this, he was very unlike Keynes and was much more interested in providing the basis for policy formation rather than the advice itself. Indeed, his work in government and later on national accounts provided the statistical foundations of Keynesian macroeconomic policy. He was very reluctant to get involved with the theoretical debates and battles of the day, preferring to work on positive economic issues such as the determination of consumers' expenditure or the large-scale modelling of national economies. Stone was one of the founders of econometrics, in sharp contrast to the prevailing dismissal of the subject in Cambridge, most famously by Keynes in his critique of Tinbergen in 1939. Unfortunately, this hostility continued as a position for many Cambridge Keynesians of Stone's generation.

Therefore, he was in an ideal position to make a judgement on Keynes's attitudes towards economic statistics and econometrics; see Pesaran and Smith (1985). In his Keynes Lecture in 1978 [167], Stone assessed the attack on econometrics in Keynes's review of Tinbergen, the attack that became the received wisdom of some of Keynes's followers after the war. Stone puts forward three suggestions for the virulence of the critique. First, 'Keynes suffered from an irresistible urge to overstate ... Both by temperament and by training he was heir to the great rhetoricians of the nineteenth century'. Second, '[B]y the thirties Keynes's mathematics had become pretty rusty ... Keynes never seems to have relied much on his mathematics and when it came to econometrics he can hardly be said to have been conscious of doing any'. And third,

In my experience Keynes's reaction to anything new was to look for the weak spots and shoot them full of holes. This was not the end of the matter but only a way of gaining time, as he usually thought things over and either came up with some really good arguments or changed his mind [167: 62–63].

In fact, Keynes was a founding member of The Econometric Society in 1930 and became its President through 1944 and 1945. When he was invited to become President by Alfred Cowles, in mid-1943, he replied, '[W]hilst I am interested in econometric work and have done something at it at different times in my life, I have not recently written anything significant or important along these lines, which would make me feel a little bit of an imposter' (Keynes quoted in Pesaran 1991: 99). In his last letter to the Cowles Commission, dated 23 July 1945, Keynes expressed his great regard for Tinbergen, who had visited him in Cambridge: 'I felt once more as I had felt

before, that there is no-one more gifted or delightful or for whose work one could be more anxious to give every possible scope and opportunity' (ibid.). What a contrast these remarks are to Keynes's ill-tempered and rude review of Tinbergen's econometrics contributions to the League of Nations' report in 1937. As Stone remarks: 'Nothing could show better the difference between Keynes's first impersonal impressions and his considered view based on personal experience' [167: 64]. There was also the practical effect when Keynes had to grapple with the interpretation of data to understand behaviour: 'What Keynes wrote when he contemplated the literature as a non-participant and his reactions when he himself was on the breach were quite different' [ibid.: 72].

Leif Johansen (1985: 8) comments:

Stone was obviously quite aware of Jan Tinbergen's, Lawrence Klein's and others' constructions of macroeconomic models and the potential significance of national accounts in this context. In another early article co-authored with E. F. Jackson [22], Stone discussed the use of macroeconomic models as an aid in the analysis of economic policy issues. He illustrated this by constructing a model reflecting the ideas and numerical relations found in Nicholas Kaldor's well-known appendix to W. H. Beveridge's report *Full Employment in a Free Society* (1944).¹³

The Cambridge economists closely associated with Keynes in the 1930s were fully behind the initiatives that led to the creation of the Department of Applied Economics (DAE), and the appointment of Stone as its first Director. The initiatives first took the form of the Cambridge Research Scheme, sponsored by the NIESR, which had as one of its original functions 'to act as a channel between foundations which supplied finance for research, and individuals, universities and other institutions needing such support' [167: 85]. The Cambridge Research Scheme was chaired by Keynes and had as its members Kahn, Sraffa, Champernowne, Joan Robinson, Austin Robinson (as Secretary), and Michał Kalecki (as statistician). The Scheme was awarded a grant of £600 for the year 1938–1939 on the general research topic of 'the process of economic change in the United Kingdom'. However, the Scheme came to be seen as too loose and informal, and its research programme was subsumed in the proposals for the DAE in 1939. Stone was working as Keynes's assistant during the war, but also had projects in the new NIESR, projects which eventually moved to Cambridge after the war. He also had a personal connection since Feodora Leontinoff, who became his second wife in 1941, was Secretary of the Institute.

¹³ See Kaldor (1944).

Although the war intervened and delayed the process of establishing the DAE, the direction of Stone's interests were clearly in using and developing econometric techniques for applied economic work. His first paper, with W.A. Tweddle, was published in 1936 in *Econometrica* [1]. The paper published in 1938 [3] with his first wife, W.A. Stone, was on estimates of the marginal propensity to consume and the multiplier. The paper used and developed econometric techniques to analyse data and provide the estimates. The paper references the pioneers of econometrics, Tinbergen and Frisch, and adopts correlation and regression techniques. Stone was later to say, 'I think it can be fairly said that if econometrics struck firm roots in post war Britain and has grown in spite of adverse winds into a sturdy tree, our thanks should go in large measure to Maynard Keynes' [167: 87].

The System of National Accounts

Richard Stone is regarded as the father of the United Nations' (UN) SNA that has become the basis for national accounting across the world today. It was for this contribution that he was awarded the Nobel Prize in Economics in 1984. His outstanding and path-breaking work was made possible in part by his personal characteristics. He was reluctant to be drawn into economic debate and controversy, preferring to devote himself to open up new areas of enquiry, especially on the boundaries between economic theory and data, making him ideally suited to international collaboration and the essential development of consensus. He was meticulous in his scholarship; he had a systematic logical theory underlying accounting for the national economy; he read widely; he had a gift for writing clear, cogent, and easily understandable English. In addition, he had worked with Keynes, the most famous economist in the world at the time, he developed an extensive network of economists interested in measurement, and he was prepared to lead by example, preparing drafts for discussion, obeying the rules, and achieving agreement.

The international acceptance of the concepts, classifications, and accounting rules in the 20 years after the war was closely related to the Keynesian understanding of how the economy operates. This was the golden age for Keynesian policy. The accounts laid the basis for government intervention to achieve full employment, with the linking of public spending, industry, and private consumers and the distinction between investment and consumption. The adoption of standards across countries allowed for the development of international models and the calculation of cross-border macroeconomic effects. With the National Accounts, those who argue against Say's Law, that

supply creates its own demand, have a quantitative basis for assessing the potential loss in marketed resources after a financial crash and the fiscal stimulus necessary to prevent a downturn or depression.

Ward (2004: 72–83) documents the development of the UN's accounts and the role of Stone and the CGP. The ideas and theory originated in the work by Stone and James Meade on the UK accounts during the Second World War [8, 16]. It was continued by Stone in a paper on the National Accounts for the League of Nations written in 1945 and subsequently adopted by the UN in 1947 [24]. Stone can be seen as the pioneer of systematic aggregation in the national accounts. Transactions are treated as in double-entry accounting, made explicit from both sides of a transaction. The purchase by a consumer is also the sale by the retailer; the wage received by the worker is also the payment made by the employer. Here we find Stone's distinctive contributions. National income is explicitly shown as being derived by different combinations of elementary transactions. The interdependencies between groups of transactions are made clear [26]. Instead of single estimates, a framework for social accounting is provided. Also, institutional sectors are distinguished and defined. Unifying principles are proposed concerning the invariance of concepts such as national income to different aggregations in the underlying accounts [30]. For example, the Material Products System as developed and used by the Soviet Union had a gross estimation of material production, which changed depending on the vertical integration of production, whereas the SNA has a net concept of production that excludes intermediate production and so it is invariant to industrial integration. Stone went on to chair several UN committees developing the accounts, culminating in the 1968 UN's SNA.

Leif Johansen (1985: 6–13)¹⁴ has the most comprehensive description and review of Stone's contributions to national accounting. He emphasises that 'from the outset, Richard Stone clearly perceived accounts systems in the context of a more abstract and general "model." He did not choose his concepts, classifications, relations and propositions in a routine manner, on the basis of generally accepted accounting methods' (ibid.: 7). In other words, Stone regarded the national accounts as the fundamental data set required to build and estimate an economic model, rather than as a set of accounts following rules more suitable for managing and auditing a commercial company or government institution. He recognised the importance of sampling [41], the reconciliation of the inevitable discrepancies between different measures of

¹⁴Johansen can be considered as the first to have developed a general equilibrium model in his PhD thesis partly written whilst he was visiting Cambridge in 1959.

key variables such as GDP [10], and the need for repeated revisions to past estimates of the accounts as new data or methods became available.

3 Consumer Demand

Consumption is the largest component of GDP, and the consumption function is one of the key structural relationships in the Keynesian model. As noted, one of Stone's earliest publications in 1938, with his first wife Winifred, was estimating the marginal propensity to consume and the multiplier [3]. Their work was a direct econometric estimate of the postulated stable relationship in three chapters of Book III of *The General Theory*. Keynes had made some back-of-the-envelope calculations, but the Stones sought to estimate the function and the multiplier for more countries over longer time periods. They fitted aggregate consumption functions with a graphical analysis using data on consumers' expenditure and income with time trends for interwar periods for Germany (two periods), the UK, Sweden (two periods), the Netherlands, and the USA. They concluded that the functions were reasonably stable over time, but varied across countries. They calculated leakages with implied multipliers of between 1.7 and 4.9, depending on the openness of the economy [3: 15].

However, when Stone returned to modelling consumers' behaviour after the war, it was to explore detailed spending for the UK and it was this work for which he is celebrated by the profession, for its meticulous attention to statistical detail, for its integration of theory and data, and for its use of econometric techniques. Two of his most cited works were in this area, published in 1954. The most cited with 155 citations over the period 1966–1984 (Garfield 1985: 473) is the article in the *Economic Journal* on the Linear Expenditure System [53], which is path-breaking in that it was derived from utility theory, imposing the critical theoretical constraints of adding-up, homogeneity, and symmetry algebraically and adopting a method of simultaneous equations to estimate the system. Note that the dependent variables are nominal expenditures, and one of the independent variables is total expenditure, so that the adding-up property is imposed by linear regression. The next most cited work (97 citations) was Stone's book published in two parts in 1954 and 1966 [56, 123] (with Derek Rowe and others) called *The Measurement of Consumers' Expenditure and Behaviour in the United Kingdom, 1920–1938*. Angus Deaton states of the first volume: 'There is a masterly exposition of the theory of demand and of revealed preference, and there is a chapter on econometric methodology that reads like a text until one realizes that this is where the texts originated' (Deaton 1987: 510).

4 Macroeconomic Modelling and Policy

Stone's interest in macroeconomic modelling and policy flowed naturally from his interest in measuring national income and estimating the multiplier. His aim was to understand the workings of the economic system, integrating observations of economic behaviour and economic theory. His views about the use of the national accounts in formulating economic policy are made clear in a paper published in 1951 [45]. Here, he put forward three 'social ideals' that should be pursued in national economic planning, avoiding both extremes of the free market 'laissez-faire' ideology and totalitarian central planning. First and foremost was economic stabilisation, meaning the avoidance of unemployment and inflation. The second ideal was the equalisation of the distribution of incomes. Third, there is the intervention in the market economy to plan for social objectives. In his work, Stone repeatedly emphasised the need for policy makers to supplement market mechanisms by allowing for values not priced on any market, such as uncongested streets, peace and quiet, and an unpolluted environment. His fundamental objection to laissez-faire is that it works with limited values, those of the market place, and limited information, that of current prices and a small number of forward markets.

The Foundation of the Cambridge Growth Project (CGP)

The new Labour government that came to power in Britain after the Second World War had full employment as its main economic objective and the policy was successfully continued by Conservative governments in the 1950s. The overall economy grew at about 2% a year between 1950 and 1960, with inflation generally below 3%. Economic policies were a mixture of fiscal and monetary management. This was the golden age of Keynesian policy making, remarkable in hindsight for its stability and prosperity.

However, there was a concern that the UK was lagging behind with lower long-term growth compared to other large European economies. The CGP was founded by Stone and Alan Brown in 1960 in part as a response to these concerns. The project was intended to study quantitatively the structure and future prospects of the British economy, the possibilities of stimulating its rate of growth, and the problems to which this would give rise [70, 73, 80]. The original suggestion came from Brown to combine three strands of the work that the DAE had been engaged on in the 1950s. These were: (1) the

national accounts, (2) input–output tables, which became embedded in the matrix of social accounts and which provided the framework for double-entry accounting, and (3) on the demand side, the data and modelling of consumers' expenditure. Stone [194: 194] has commented on Brown's contribution: '[T]he successful launching of our venture and the productiveness of the group in those early years were in large measure due to his drive, enthusiasm and devotion to work, qualities made even more effective by his sweetness of temper and his sense of humour'.

The CGP publications were initially in the form of 'Green Books', designed for rapid publication of material that was awkward for a publisher, in the sense that many large tables were included as well as complex mathematical equations within the text. In the first volume, by Stone and Brown in 1962 [85], they noted that the peacetime British economy had grown by an average of 1.8% a year between 1910 and 1960, and at 2.0% a year between 1950 and 1960, which they contrasted with much higher rates in the 1950s for France, Italy, and West Germany, but lower rates for the USA and Canada. Why was this so? Could the rate be raised?

The approach was to argue that action could be taken to improve the standard of living by economic policy, but that this would involve understanding the economic system, then diagnosing the problems, and finally suggesting solutions by way of changes in policies. The first Green Book, 'A Programme for Growth', was co-authored by Stone and Brown as 'A Computable Model of Economic Growth' and was based on a course of lectures given in Cambridge in 1962 [85]. It set out the structure of the model to be estimated for the British economy, combining the input–output tables with a system of demand equations for consumers' expenditures. The objective was to explore the effects of raising the growth rate of the British economy, essentially to find out if a higher rate of long-term growth was feasible and to discover the obstacles to raising it. The method was to build a Social Accounting Matrix (SAM) of the economy in a base year and then to make projections under different assumptions about the economy, measured in terms of the SAM for a future year. The problem of the economic cycle was set aside by assuming that consumers' expenditures in a future year grow at constant rates, so that the overall solution is said to be 'steady state'. The model is solved for different assumptions about these rates of growth, given the fixed labour force and the requirement that investment must be sufficient to maintain the growth rates of consumption. A second model, to represent the transition to the steady state, was proposed but never implemented.

The CGP Model

The theoretical basis of the CGP model was eclectic: The economy is seen as a complex system and the role of economic theory was to suggest relationships between variables in the system, but under assumptions, such as fixed consumer preferences and industrial technologies, that are recognised as unrealistic but which simplify the problem. Both extremes of free market and central planning are rejected in favour of consumer sovereignty and planning by government and industry to achieve social and political objectives. A context of Keynesian fiscal policy to maintain full employment and low inflation from year to year is assumed, but additional policies are seen as necessary to influence the long-term growth rate. The model is Keynesian with some neoclassical components but distinguished by being structural. The model was highly disaggregated compared to the macroeconomic models being developed at the same time.¹⁵ The first version had 253 accounts arranged as a matrix to embody the double-entry characteristic of the accounts, with outgoings down the columns and incomings along the rows [86]. An aggregate consumption function is replaced by an assumption about the growth of total private consumers' expenditure, with detailed consumer spending given by the Linear Expenditure System. Aggregate income is replaced by neoclassical production functions for 31 industries, with a modified Cobb–Douglas functional form, an assumed average wage rate, and a common rate of marginal physical product for labour and capital across all industries. Clearly, the solutions could be unbalanced in that they could show unemployment of labour or insufficient labour to meet the demands of industry.

Indicative Planning

One interesting aspect of the project and the model was that it supported indicative planning. This is the idea that the provision of consistent detailed information on outcomes for industrial output, investment, and employment to governments and industry was sufficient in itself to identify constraints to growth, such as insufficient investment, labour, or foreign exchange reserves. Government and industry could then react by corrective action and hence this would lead to faster growth.

This was certainly in accord with the political thinking of the time. The Conservative government set up the National Economic Development

¹⁵ See for example the work of the Brookings Institution reported in Duesenberry et al. (1965).

Office (NEDO) in 1962 to support a council bringing together the government, employers, and trade unions to develop consensus economic policies. Both the main parties in the 1964 General Election campaigned with promises to raise the rate of growth by some form of economic planning. In the event, Labour won and Harold Wilson became Prime Minister, with James Callaghan as Chancellor of the Exchequer and George Brown as Deputy Prime Minister. An earlier Labour government had Stafford Cripps as Minister of Economic Affairs and Wilson wanted to hive off into a new department the long-term planning functions of the Treasury from its short-term management of the economy and government finances. So came about the outstanding example of indicative planning in the UK with the creation of the Department of Economic Affairs in 1964 under George Brown. It was, however, to be short-lived.

The theory of the CGP model, and its application in the form of the 1965 publication *Exploring 1970* by Alan Brown, correctly identified an increase in imports and a consequent deficit in the current account of the balance of payments as one of the constraints to faster growth (ibid.: 59–60). Devaluation is mentioned as a short-term solution and improving price, design, and quality competitiveness as long-run solutions. However, the model did not include price effects, so the restriction on imports (exports were exogenous) had to be imposed in the solution that met the balance-of-payments constraint. The failure to fully recognise this constraint was fatal to the policy experiment of The National Plan of 1965, with the failure exacerbated by political infighting at the Treasury. The Plan lacked strong and long-term policies, such as import controls or devaluation of sterling to offset the increase in imports that accompanied faster domestic growth. The new government emphasised the importance of the current account deficit, which it blamed on its predecessor, but decided not to devalue sterling. Instead, the response was to introduce the Temporary Surcharge on Imports in October 1964 as an emergency measure. Economic policy became focused on defending the pound, irrespective of the depressing effects on investment and growth. The Plan was effectively discarded when deflationary measures were introduced in July 1966. But these too failed to save the pound, which was devalued in 1967.

Indicative planning was discredited because the crucial message it gave in 1964, that balance-of-payments deficits would increase as a consequence, was politically unacceptable. The academic work of the CGP, however, flourished. Stone continued to chair the weekly seminars in term time; he supported new areas of research into the financial and energy sectors, and foreign trade; and he edited most of the Green Books. The CGP responded to this failure of indicative planning by extending the modelling to allow for a better

understanding of how the constraints on growth might be relaxed through changes in economic policy. Econometric equations for exports and imports were introduced to allow for price responses in the model, so the effects of tariffs and exchange rate changes could be simulated. Eventually, in the 1970s, the idea of transient and steady-state versions was abandoned and one version was estimated as an annual dynamic simulation model, with various options for closure. A consumption function was introduced to allow the incomes for industries and government to affect total consumer expenditure.

Input–Output Modelling

Stone made a substantial contribution to input–output analysis beginning in 1951 (see Marangoni and Rossignoli 2014). The input–output approach had been pioneered by Leontief over a decade earlier. The CGP developed and extended the input–output representation of industrial structure in two ways, both of which were adopted in subsequent literature and in practice. First, commodities were distinguished from industries to allow each industrial group to produce a number of products or commodities. This is important for integrating the input–output tables and the national accounts because it makes the institutional accounts more logical and transparent. Industries produce not only their own characteristic products but also the products of other industries and this can be represented in the accounts. This treatment is later followed in the UN’s revised SNA published in 1968. Second, the ‘RAS’ method (see Bacharach 1970) was developed to extrapolate input–output tables when data on inter-industry flows were not available, but row and column totals of the flows could be calculated from national accounting data. This method has proved to be very useful and has been adopted extensively as a means of updating input–output tables ever since.

5 Richard Stone’s Influence on Cambridge Economics

Stone’s specific contribution to Cambridge economics came from his time as the first Director of the DAE and his subsequent time as Director of the CGP in the DAE. His influence can be seen in three areas that flourished throughout the life of the DAE: the historical research into UK national income statistics, the development of econometrics as a discipline, and the modelling work of the CGP.

The historical research¹⁶ started by Stone in the National Institute and continued in the collection of statistics on consumers' expenditures in the DAE [56] was later supported by his invitation to Phyllis Deane to join the DAE in 1950. She worked on UK regional statistics, and went on to publish two landmark texts in 1962, on *British Economic Growth, 1688–1959* with W.A. Cole and the *Abstract of British Historical Statistics* with B.R. Mitchell. The work on historical national accounts was continued by Charles Feinstein who joined the DAE in 1958 and who was largely responsible for *National Income, Expenditure and Output of the United Kingdom, 1855–1965*, published in 1972, which became the standard reference work on the period.

Stone's influence on econometrics was profound. The DAE under his Directorship soon became a leading international centre for econometrics in the 1940s and 1950s. The Durbin–Watson test for serial correlation in the residuals of regression equations was devised when he was Director of the DAE, as was the Cochrane–Orcutt method for reducing the risk of spurious correlations between variables. The development of econometrics in Cambridge stemmed from this time (see Smith 1998: 88–103). Members of the CGP went on to become internationally acclaimed in applied econometrics, most notably Angus Deaton for demand and inequality analysis and Hashem Pesaran for panel data analysis.

In model building, with the formation of the CGP, Stone and Brown began a distinctive approach that continues to the present day, with five characteristics: (1) structural detail allowing for institutional aspects of behaviour to be represented, (2) consistent aggregation in basing the model on a system of accounts in the form of the SAM, (3) a combination of optimisation and simulation by integrating limited optimisation in production, for example, minimisation of costs, with time-series estimation of the model's parameters, (4) computable solutions, and (5) both balanced and unbalanced solutions of the models. Although disaggregation is costly in terms of data collection, data processing, computing, and model builders' time in checking solutions and results, it has the great advantage of representing institutional structures, such as the particular features of agriculture or the electricity industry. The advantage of a computable model is that the results are reproducible and the effects of alternative assumptions can be readily calculated consistently and in great detail.

The original CGP two-model structure of a transient and static model was eventually replaced by a single dynamic model, the Multisectoral Dynamic Model (MDM), developed in the DAE in the late 1970s. The MDM did

¹⁶ See Pesaran (1991: 99–101) for Stone's own account of the work.

not rely on the concept of equilibrium (see Barker 1977); rather it relied on simulating the economy based on sets of equations estimated using time series data. After the CGP closed in 1987, one version of the model became, after many theoretical and econometric improvements (including an extension to 20 world regions), the Energy–Environment–Economy Model at the Global level (E3MG). This was developed and used by the Cambridge Centre for Climate Change Mitigation Research (4CMR)¹⁷ group in Land Economy at Cambridge to generate scenarios for mitigating climate change (see Barker and Crawford-Brown 2015). E3MG is a Post Keynesian, structural model with endogenous technological change.

The work of the CGP also developed into the two Post Keynesian models (MDM and E3ME) at the UK and global scales maintained by Cambridge Econometrics and continuing to the present day. Stone agreed with this evolution: ‘I think growth models should be at least capable of being stable but should not be based on the assumption that equilibrium conditions are always fulfilled’.¹⁸ In their use of formal econometric techniques to estimate the model, and assumptions of variable degrees of competition and economies of scale across industries, the Cambridge models offer an alternative to the prevalent Computable General Equilibrium methodology based on neo-classical economics.

6 Conclusion

Richard Stone was charismatic and inspiring as an example of scholarship and intellectual leadership. He avoided the political and theoretical disputes that characterised the Cambridge Faculty of Economics and Politics during most of the period after the Second World War, instead preferring to continue with his own approach to economic theory and data, using mathematics and statistics where appropriate. His influence on Cambridge economics is to be found in the tradition of model building and use that developed in the DAE and continues to this day in the company Cambridge Econometrics. It is a distinctive approach that is Post Keynesian but highly structural, based on economic time series and cross-section data and simulating the economy rather than optimising a social welfare function.

¹⁷The 4CMR was formed after the DAE was merged into the Faculty of Economics in 2005 and those working on energy–environment–economy modelling moved to the Department of Land Economy.

¹⁸Stone to Tinbergen, 12 February 1981, Richard Stone Papers, King’s College Archive Centre, Cambridge University: Box JRNS 4/10.

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Polly Hill (1914–2005)

Robert W. Dimand and Kojo Saffu

1 Introduction

The economist and anthropologist Mary Eglantyne Hill (always known as Polly) was a leading figure in development studies and outspoken critic of development economics. Her work crossed disciplinary boundaries between economics, economic and social anthropology, entrepreneurship studies, and development studies: her first degree (at Cambridge) and first academic position (at what became the University of Ghana) were in economics, her doctorate was in social anthropology but supervised by the economist Joan Robinson, and her later appointments were in African Studies at the University of Ghana and in Commonwealth Studies at Cambridge (see Austin (1997), Hart (2005), Gregory (2006), Hammond and Mwanasali (1994), and especially the interview by Macfarlane (1996)).

Polly Hill was born in 1914, the eldest of four children of the Cambridge physiologist A.V. Hill, later winner of the 1922 Nobel Prize in Physiology

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or Medicine and one of the founders of operational research (see A.V. Hill 1960), and of Margaret Hill, née Keynes, active in social work and later a Royal Commission member. Polly read economics at Newnham College, Cambridge, but did not study directly with her uncle Maynard Keynes, despite her wish to be supervised by him (Skidelsky 2003: 527). She graduated with a 2.I, that is, in the upper half of a Second Class Honours degree at a time when a First Class Honours degree was required to proceed to doctoral study (Joan Robinson, later Hill's dissertation adviser, was exceptional in achieving a Cambridge chair in economics despite a Second Class degree—see Aslanbeigui and Oakes 2009). Hill's first employment after graduation was as editorial assistant to her uncle Maynard, the editor of the *Economic Journal*. From 1938 to the end of 1939, she was a researcher for the Fabian Society, writing a book-length report on *The Unemployment Services* (Hill 1940), concerning 'the changes that should be made in the unemployment insurance scheme and the Unemployment Assistance Board in a Labour Government's next term of office' (ibid.: xi). Although by its terms of reference the book was 'concerned with the relief and not the cure of unemployment', Hill (ibid.: 182) sounded a characteristically Keynesian note that '[e]ven so it should be emphasised as strongly as possible...that a Labour Government would endeavour to keep unemployment within manageable limits through a policy of public loan expenditure (and rearmament would not be the only possible form of peace-time public loan expenditure as under the National Government)' (ibid.). Contrary to Sir William Beveridge (then chairman of the Unemployment Insurance Statutory Committee), Hill supported proposals to improve the position of women by equalizing unemployment insurance contribution and benefit rates, noting that in 1936 men received unemployment benefits equal to 72% of their contributions to the insurance fund while women received benefits amounting to only 41% of their contributions.

From 1940 to 1951, she was a civil servant in London, first at the Treasury, then at the Board of Trade, and finally in the statistics department of the Colonial Office. From September 1940 to May 1941 she lived in the basement of 46 Gordon Square, the home of Maynard Keynes and his wife Lydia Lopokova (whose letters she later co-edited, Hill and Keynes 1989), and later during the war in the studio of the sculptor Henry Moore. 'On 18 September [1940] he [Keynes], Polly, Fred Woollard (their chauffeur), Mrs. Stephens [Keynes's secretary] and Mary (their maid) were eating a duck from Fortnum and Mason for supper when a landmine burst opposite their house. The shutters saved them from injury, but all the windows were broken and the front door unhinged' (Skidelsky 2003: 604). Hill wrote a volume of poetry (Hill 1945) but later told Alan MacFarlane (1996) that she 'languished' as a civil servant.

In 1951, Hill became commercial editor of the weekly magazine, *West Africa*, which posted her to the Gold Coast (independent as Ghana from 1957) the

following year. For eight years, from 1953, she was married to Kenneth Albert Curwood Humphreys, registrar of the West African Examinations Council, but always published under her maiden name. Their daughter Susannah was born when Polly was 42. In 1954, she became a Research Fellow in economics at the University College of the Gold Coast (later the University of Ghana), with a grant from the West African Institute of Social and Economic Research to study cocoa farmers. Ten field surveys distributed in the 1954–1955 crop year, nine of them distributed with the assistance of co-operative societies associated with the Gold Coast Co-operative Marketing Association, provided the material for *The Gold Coast Cocoa Farmer: A Preliminary Survey* (Hill 1956): ‘The survey technique was unorthodox, and much is tentative and conjectural; but I hope that the book will be received in the spirit it was written in—as a first contribution towards the understanding of the social organization of a fascinating industry’ (ibid.: v). Hill was a Research Fellow, and later Senior Research Fellow, at the University of Ghana until 1964 (see Hill 1957–1960), after spending the 1960–1961 academic year on leave from the University of Ghana as a Smuts Visiting Fellow at Cambridge University’s Department of Social Anthropology at the invitation of Professor Meyer Fortes (who wrote the foreword to Hill (1963)), and, as ‘one of my two great teachers in the two disciplines’, shared with Joan Robinson the dedication to Hill (1986a)).

When she returned to the University of Ghana in 1961, she moved from Economics to the Institute of African Studies. As Fortes remarked in his foreword to Hill (1963), ‘Miss Hill calls herself a “field economist” to distinguish her methods from those of the more normal variety of economist who nowadays swoops down upon an “underdeveloped country” for a week or a month, peruses the files and the blue books, and presently produces a plan or a treatise’ (Fortes 1963: v). Contrasting Hill (1963) with W.H. Beckett’s *Akokoaso: A Survey of a Gold Coast Village* (1944), ‘still the most authoritative description of the social organization of cocoa-farming available to students’ (Fortes ibid.), Fortes wrote that

[w]hile he [Beckett] confined his attention to one long-settled village, she ranges extensively over a large area of the hinterland of Accra where cocoa was first established at the turn of this century; where Beckett’s interests are those of the agricultural economist concerned with the statistics of finance, production, acreages, labour and marketing, hers are closer to those of the economic anthropologist concerned with such matters as the leadership, enterprise, social composition, legal framework and historical development of cocoa-growers’ undertakings. Her principal sources are the people themselves, interviewed, as in anthropological fieldwork, in their villages; but she also draws brilliantly on the scattered documentary material she has been able to ferret out to give the historical depth to her narrative which clinches the case for her main thesis (ibid.).

As Beckett (1944) studied a single, long-settled village, it was he who discovered the role of migrant farmers.

2 *The Migrant Cocoa-Farmers of Southern Ghana: A 'Field Economist' at Work*

The migration of Asante cocoa farmers in Ghana started in 1892 and continued through the 1990s. The focus of Polly Hill's path-breaking 1956 and 1963 books was migrant cocoa farmers as rural capitalists: merging farmer-entrepreneurs, economic agents who looked for opportunities and marshalled resources which they did not own to realize their goals. The field of entrepreneurship research has largely failed to develop an indigenous theory (Suddaby 2014), and so what may be described as Hill's theory of indigenous entrepreneurial development in a rural setting is worth revisiting. As Keith Hart (2005: 31) wrote in his *Guardian* obituary of Hill, she

was an academic who pinpointed a unique class of African farming entrepreneurs ... She was able to show that the cocoa farmers were an authentic modern class, migrant entrepreneurs opening up virgin forest often in companies capable of hiring Swiss construction firms to develop the infrastructure that they needed and the colonial authorities could not provide. The latter were at first ignorant of what was going on. They thought the Gold Coast was a mining colony and only found out about the country's cocoa exports when they had to account for a sudden rise in the level of imports.

Despite the varied definitions of entrepreneurship and entrepreneurs, consensus in entrepreneurship studies stresses the purposeful and emergent action of entrepreneurs creating and taking opportunities in pursuit of wealth formation,¹ and the migrant cocoa farmers studied by Hill clearly fit this characterization. They were self-reliant pioneers who not only opened up virgin rainforest (see Ruf (1995) on 'forest rent', the value of non-renewable soil fertility of land taken from the forest and newly planted with cocoa), but also single-handedly financed infrastructure development that they needed by contracting foreign firms to build roads and bridges that helped open up the rainforest and also made possible the shipment of cocoa from the hinterland to the port on the coast. Without the assistance, or even the knowledge, of

¹ See Schumpeter (1911 [1934]), Shane and Venkataraman (2000), Alvarez and Barney (2007), Dimov (2011), Alvarez et al. (2013), and also Margaret Katzin (1964) on the role of the small entrepreneur in Africa and Rachel Simms (1981) on African women as entrepreneurs.

the British colonial government of the time, the cocoa farmer–entrepreneurs developed a global industry that by 1945 made the Gold Coast the largest cocoa producer in the world (Hill 1956, 1957–1960, 1963, 1970). Gareth Austin (2005: 7, 457) cites Hill (1963) as the most famous and one of the earliest examples of work on the economy and economic history of West Africa that ‘tends to emphasize the rationality, agency and enterprise of indigenous economic actors, both before the colonial occupation and within the constraints of colonial economies’ (see also Hopkins 1973 and Austin 1997).

Hill’s emphasis on the migrant cocoa farmers as successful capitalist entrepreneurs was out of step with both the Pan-African socialist policies of Kwame Nkrumah in Ghana in the early 1960s and with many of her colleagues in the University of Ghana’s economics department in the Nkrumah era, as exemplified by Reginald Green and Ann Seidman’s *Unity or Poverty? The Economics of Pan-Africanism* (1968; see also Seidman 1978). The Nkrumah government’s disapproval of capitalists, its need for revenue for development projects, and its political base in ethnic groups other than the Asante (from whom the migrant cocoa farmers were primarily drawn) led to a policy of depressing the prices paid by the government’s monopoly cocoa marketing board well below the world price of cocoa, creating incentive problems that reduced Ghana from being the world’s largest producer of cocoa to its third largest.

As John Tosh (1991: 20–21) observes (citing Gray and Birmingham 1970),

When Britain’s African empire was at its zenith during the early twentieth century, her imperial morale was sustained by the notion that Africans were economically feckless and passive. In fact in the late nineteenth century the continent had been traversed by far-flung indigenous trade-routes linking the interior with the coast, and most of the food requirements of the towns which later sprang up in the wake of the colonial occupation were met by African peasant farmers responding quickly to market opportunities. Yet the mythical stereotype of economic ineptitude persists, and it contributes to the often specious authority with which outside experts pronounce on Africa’s development needs today, with such disastrous results.

Save that she firmly rejected the description of African farmers as peasants rather than capitalist-entrepreneurs, Hill (1956, 1962, 1963, 1970, 1986a) similarly (and starting earlier) made a strong empirical case for the active agency of African farmers, for their responsiveness to market opportunities, and against the stereotypes held by outside experts (see also Garlick (1959, 1971), Meillassoux (1971), and Ruf (1995)). She gave particular attention and recognition to the active economic agency of African, and in particular Ghanaian, women (Chapter 13 of Hill (1986a) denounced ‘The Neglect

of Women'), a theme developed further by subsequent scholars (e.g. Simms 1981; Robertson 1984).

Contrary to the claim by W. Arthur Lewis (1953) that indigenous systems of property rights impeded economic development, Hill argued that the flexibility of the indigenous land tenure system, especially in the Eastern Province of the Gold Coast colony (less so in Asante itself), supported the growth of cocoa farming. Firmin-Sellers (1996) contended that the Gold Coast failed to establish secure property rights, and that, as Lewis had claimed, this impeded agricultural investment. Austin (2005: 344–345) concludes in favour of Hill rather than Lewis and Firmin-Sellers, because

there was no compelling economic need for stranger farmers to be able to buy land outright when they could obtain use rights for land on which to plant trees that would last for decades ... Paradoxically, the more complex political economy approach proposed by Firmin-Sellers seems to miss the main story: one of economic success.²

3 Return to Cambridge, Between Disciplines

Concerned that her daughter was at risk from malaria, Hill left Ghana in 1965, becoming a non-stipendiary Research Fellow at Clare Hall, Cambridge, and obtaining small research grants that funded her fieldwork in Hausaland in northern Nigeria from 1965 to 1973 (Hill 1972, 1977, 1982). Her major work, *The Migrant Cocoa-Farmers of Southern Ghana* (1963), was accepted by Cambridge in 1967 as a PhD dissertation in social anthropology supervised by Cambridge's radical Keynesian economics professor, Joan Robinson (who had been cited, together with Keynes, by Hill (1940: 195, fn. 1); see Robinson (1979) for her views on development and underdevelopment). Hill held a prestigious, but fixed-term, appointment at Cambridge as Smuts Reader in Commonwealth Studies from 1973 to 1979, but never received a permanent academic position despite publishing nine books and 50 scholarly journal articles in anthropology and economics. Another student of West African trade and markets, Peter Bauer (1954), politically more conservative than Keynes, let alone Robinson (although he had been in Keynes's Political Economy Club as a student), had gone on from the Smuts Readership to become a Professor at the London School of Economics, a Fellow of the British Academy and a

² See Austin (2005: 532, fn. 74) for references to the studies on which he based his conclusion.

peer, and receive several Festschriften and, just before his death in 2002, the Cato Institute's inaugural Milton Friedman Prize (worth \$500,000) for 'the advancement of liberty' (see *The Economist* (2002: 76) deploring the failure to give Lord Bauer a Nobel Prize). However, no such academic appointments or honours followed Hill's term as Smuts Reader, apart from an honorary doctorate from the University of London's School of Oriental and African Studies in 1996.

In his Foreword to Hill's *Studies in Rural Capitalism in West Africa* (1970: xviii–xix), Yale development economist Stephen Hymer reported that

Polly Hill's major discovery is that, contrary to the usual view of an amorphous peasantry, the accumulation of capital in indigenous West African economies has been accompanied by the emergence of specialists who own and manage the capital stock—a 'class' of rural capitalists. She has, therefore, concentrated on the behavioural characteristics of these capitalists; on the problems they face and their methods of solving them; on their attitude towards wealth and their notions of the correct ways to invest money, to manage labour, and to preserve capital through time

and especially

the rules of thumb used by businessmen to make decisions in an environment they only partially understand ... The analogy is not water running down a hill but mice running through a maze ... The major problem of capital accumulation in underdeveloped economies is not so much a shortage of savings but a lack of institutions to channel the existing or latent surplus into productive investment.³

Hill thus looked at subject matter that might be expected to interest economists rather than anthropologists, but asked questions of the material that would occur more naturally to anthropologists than to economists. Her academic career was not helped by not being easily recognizable as a practitioner of one discipline or the other. Her sharp criticism of economics and economists, illustrated by the title of her book *Development Economics on Trial: The Anthropological Case for a Prosecution* (1986a), did not endear Hill to economists, while her account of the migrant cocoa farmers as entrepreneurial wealth creators was ignored by writers in other social sciences who treated all migrants as victims of capitalism (e.g. Samers 2010, a volume surveying migration in the Key Ideas in Geography series). Hill stressed what Africans and other people in developing countries had and could achieve for themselves, whereas, in the words of Hart (2005: 31), 'The economists have been

³ See Green and Hymer (1966) on the relations between Ghanaian cocoa farmers and foreign agricultural experts, and Rouch (1956) for a contemporaneous study of Gold Coast migration.

too busy telling them what to do and the anthropologists prefer to be their self-appointed spokespeople’.

4 Development Economics on Trial: The Anthropological Case for a Prosecution

Hill (1970: 6) lamented that

[t]he Beatrice Webb tradition of qualitative field observation and experiment has been out of fashion for some half a century, so it is no wonder that field enquiries were neglected in the British colonies. But even had this spirit of scientific enquiry not fallen into desuetude, it is likely that conventional British assumptions about West African economic behaviour would have inhibited field investigation.

She ridiculed at length three of these conventional British assumptions: that West Africans had learned the elementary facts of economic life from expatriate traders, that indigenous economies are too simple to be of interest to Western economists, and, notwithstanding the belief about simplicity, that indigenous economies are too complex to be of interest to economists: until modernization eliminates such complexities as local systems of land tenure, kinship, inheritance, and communal work, ‘indigenous economies operate on too small a scale, or on too local a basis, to be of interest to economists—and are anyway incomprehensible’ (ibid.: 7).

These beliefs were transmitted to African economists: ‘Convinced as they have been of the simplicity of indigenous economic organization, few of them [visiting economic advisers], or, indeed, of the growing band of prominent West African economists (most of them Nigerians), have used their influence to urge the need for more “grass roots research”’ (ibid.: 9)—a remark made in a chapter (‘A Plea for Indigenous Economics’) first presented to a conference of the Nigerian Institute of Social and Economic Research in 1962. She proceeded, with equal tact, to rebuke (future Nobel Laureate) W. Arthur Lewis’s ‘much-read’ 1953 report on industrialization in the Gold Coast for ‘an over-simplified image of agricultural organization based on the presumption that a simple technology necessarily implies simple socio-economic organization, “inefficiency” (in some absolute, though undefined, sense), and small-scale production—though none of these notions is necessarily true or meaningful’ (Hill 1970: 9).

Hill (ibid.: 12–13) also criticized the ‘mystique about official statistics which withstands much battering’, particularly ‘occupational statistics, which appear so accurate because they are so detailed’, such as the 1960 Ghana population

census, which failed to distinguish cocoa farmers from their labourers and which reported nearly all of the wholesalers of Ghanaian foodstuffs in southern Ghana to be men when most were women (but she praised others of the published reports on the 1960 census as ‘real landmarks of organization and scholarship’ (ibid.: 13)). Perhaps, she suggested, the census enumerators had counted as wholesalers ‘only those who handled imported goods or who had office premises’ (ibid.: fn. 2). Chapter 3 of Hill (1986a), the second of three chapters on ‘The Vain Search for Universal Generalizations’, was subtitled ‘The Poor Quality of Official Statistics’, and had an appendix on a 1981 World Bank report on accelerating development in sub-Saharan Africa, ‘a good example of a recent tendency to base weighty discussions on official statistics which are simultaneously admitted to be of doubtful quality’ (ibid.: 49). This critique of the quality of African official statistics has been pursued at greater length by Morten Jerven, who cites Hill (1986a) on the United Nations Food and Agriculture Organization’s use of inappropriate statistical categories and methods in developing countries (Jerven 2013: 77–78) but not her earlier work, which he would find congenial.

Hill (1986a: 21–24), arguing that ‘the grand development theories are far too generalized to be testable in the field’ (ibid.: 21), was sharply critical of a striking piece of long-term fieldwork in an Indian village by leading economic theorists Christopher Bliss and Nicholas Stern, who began believing that ‘theories of development economics should have the power to explain what one finds in a village such as Palanpur’ (Bliss and Stern 1982: 4) but concluded that ‘We are unable to confirm that neo-classical economics is alive and well and residing in Palanpur, at least in its simplest form’ (ibid.: 291; see also Lanjouw and Stern 1998). Hill (1986a: 22, fn. 16) remarked that ‘So far as I can tell, there are no publications by anthropologists in this book’s bibliography’. Such neglect was in keeping with such precedents as Frank Knight (1941) and Richard Posner (1980), who viewed any interaction between economists and anthropologists as almost exclusively a matter of economists lecturing anthropologists on basic microeconomic theory (cf. Herskovits 1941, a reply to Knight 1941). However, the Palanpur project and reactions to it (including Hill 1986a) ultimately stimulated more balanced and fruitful conversations between anthropologists and development economists (see Bardhan 1989; Bardhan and Ray 2008).

Hill (1970: 15, cf. 1986a) objected that (non-indigenous) economists ‘are apt to be so much less sophisticated than their subjects, the farmers’, insisting that ‘the Marxist textbooks are no more relevant than the capitalist variety’. As befitted a student of Ghana’s migrant cocoa farmers, Hill rejected the myth that shifting cultivation was ‘a ramshackle, wasteful, obsolete state of affairs’ (Hill 1970: 15), insisting that on the contrary, ‘systems of recurrent

cultivation, if understood, could often be represented as respectable, even scientific—they usually, of course, involve crop rotation’ (ibid.). Nor did she accept that it was inefficient for West African farmers to grow food for their own consumption: ‘[C]ocoyam and plantain are necessary cover crops for young cocoa seedlings, so that cocoa-farmers with new farms cannot avoid producing these foodstuffs’ (ibid.: 16) and, more generally, ‘labour, rather than land, may be the scarce factor, so that to get the maximum output farmers must distribute the available manpower between crops with different growing cycles’ (ibid.) (see also Hill 1992a). Celebrating ‘those enterprising and wily migrant cocoa farmers of southern Ghana’ (Hill 1986a: 12; see also Hill 1957–1960, 1963, 1966, 1970), and later the Ewe Seine fishermen and shallot farmers (Hill 1986b), Hill always stressed the importance of local knowledge and upheld the rational agency of people in developing countries against the presumptuous ignorance of outside ‘experts’. As Hart (2005) wrote in his obituary of Hill, ‘It would be hard to exaggerate the contrast between her Ghanaian findings and the conventional thinking of development economists and administrators at the time and since. Her work has barely been absorbed into mainstream anthropology because it contradicts deep-seated convictions about western economic leadership and African backwardness’.

Her critical attitude towards the prescriptions of mainstream neoclassical economics was matched by an equally critical distrust of socialist panaceas. Hill (1986a: 172, fn. 3) emphasized that

contrary to common prevailing belief, there is no evidence that communal arable farming (as distinct from communal grazing) has ever found favour in Africa or south India, and that ‘socialist communal farming’ has always failed wherever it has been imposed on African people—even, on [long-time Tanzanian President Julius] Nyerere’s own admission, in Tanzania.

5 Conclusion

While Smuts Reader at Cambridge, Hill spent 1976–1977 in Sri Lanka and India, working on a major comparative study of dry-grain-farming families in northern Nigeria and southern India (Hill 1982). Four years of earlier fieldwork in Hausaland in northern Nigeria, leading to Hill (1972), was funded by the University of Michigan’s Center for Research in Economic Development, headed by Wolfgang Stolper (of the Stolper–Samuelson theorem in international trade theory). Hill engaged in fieldwork until the age of 67, and remained a productive scholar after the expiry of her Readership (Hill

1982, 1984, 1986a, b, 1992a). She returned to her earlier Ghanaian research, editing material on indigenous traders and markets that she had collected in Ghana from 1962 to 1964 to be published by a Nigerian university's history department (the University of Jos) two decades later (Hill 1984).

In her later years, Hill looked back to her family, academic, and regional roots. She settled in the Fens in southern England, 'where she lived in a splendid modern house complete with Moore sculptures and willow trees' (Hart 2005: 31), where she wrote an article, 'Who were the Fen People?' and a privately published monograph on *The History of the Isleham Fen in the 1930s* (1990, 1992b), as well as resuming the writing of poetry. Her last book, a history of female students at Cambridge, was never published (Mills 2009). She reflected on her life and career, and on the disciplines of economic anthropology and development economics, in a fascinating videotaped interview with Alan Macfarlane (1996). Together with her cousin Richard Keynes, Hill edited the letters that Maynard Keynes and Lydia Lopokova exchanged from 1921 until they married in 1925:

For four years, from 1983, Polly Hill was partly occupied in personally transcribing Lydia's entire text from greatly enlarged photo-copies; in the final stages she received invaluable help from her daughter, Susannah Burn, who read all the letters aloud, with particular attention to punctuation and spelling, as a check on the transcription (Hill and Keynes 1989: 9–10),

while Richard Keynes edited their uncle's letters. The result, *Lydia and Maynard*, was a delightful and affectionate celebration (*The Economist*, 6 January 1990, entitled its review 'Spring-leap-gobbles', quoting one of Lydia's letters), and also a testament to the roots that Polly Hill retained however far afield her studies took her.

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Phyllis Deane (1918–2012)

Heinrich Bortis

1 Introduction

Phyllis Deane was a Cambridge economist par excellence, forged in a continuous association with her University from 1950 onwards. As early as the late 1930s, when she was an undergraduate at Glasgow, Keynes and Marshall initiated her thinking in economics, which she combined with history from the beginning. Subsequently, Deane went her own, very specific and original way. In the 1940s, she started from empirical groundwork on colonial accounting, inspired by Keynes, went through fundamental work in economic history, then on to the history of economic ideas, and ended up in profound reflections on *economic science* and the *art of political economy*. Her work brought out into the open two important hallmarks of the Cambridge economist, the comprehensive and balanced way of thinking, and the endeavour to get down to the fundamentals.

Following up some biographical notes, Deane's life work will be presented under the three headings: first, *economic history and economic development*, second, *history of economic ideas and political economy*, and, third, *political economy and economic science*.

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Phyllis Mary Deane was born in Hong Kong shortly before the end of hostilities in the First World War. Her father settled in Glasgow after the war, and Phyllis attended Hutchesons' Girls' Grammar School in the city. She then went on to Glasgow University, where in 1940 she gained an MA in economics and history (see Davison 2012). This most fruitful combination of subjects would crucially shape her academic career.

At Glasgow, Deane was strongly influenced by two teachers: first, Professor W.R. Scott, significantly an economist and a historian, famous for his three volumes on *The Constitution and Finance of English, Scottish and Irish Joint-Stock Companies to 1720* (published between 1910 and 1912), a work she considered a treasure. Second was

Alec Cairncross, who then was very young [b. 1911, captivatingly brilliant] and fresh from Cambridge with *The General Theory* clutched in his hand which he introduced to us at a fairly early stage. This was in 1938–39. My interest was stimulated by this, and so I did more economics and economic history, although there was a steady stream of political and constitutional history in the course (Deane in Crafts and Humphries 1996: 3).

Moreover,

I think that living through the 1930s made one much more receptive to the sort of economic policy which was emerging from, say, the *General Theory*, and made one understand what the high level of unemployment meant for a place like Glasgow, where in some areas something like 60% of the people—in Clydebank, for example—were out of work and you saw it actually happening. So I suppose it gave me a rather strong interest in the importance of income distribution as well as growth. And of the importance of economic policy and the influence of ideas on policy (ibid.: 5).

Deane took her finals at Glasgow at the beginning of June 1940. Then she went straight into a research project run by Professor J.R. Bellerby—on post-war economic reconstruction. There was a general belief at the time that the end of the war was just a year or two away, and that everybody had to be ready for economic reconstruction after the war. Deane was invited to London to join the National Institute of Economic and Social Research, where she undertook a project inspired by Keynes, Stone, and Meade who had just set up a system of social accounts for the UK. What Keynes wanted was to apply this system to a completely different economy than the UK, for example, to colonial territories. Subsequently, Deane worked in London through most of the war using the Colonial Office library and other such sources, trying to

work out national income for Northern Rhodesia and Jamaica and attempting to set the results into a system of social accounts. Here, she was very fortunate in having Austin Robinson as a supervisor. Many well-known economists were working in Whitehall for the war effort, including Robinson, Stone, and Arthur Lewis who regularly came over to the National Institute to advise her on her work. So Deane started out with an advantage that few research students have. At the end of the war, she visited developing countries to find out what conditions were like on the ground. In 1946–1947, she went to Northern Rhodesia and Nyasaland to produce national income accounts for both colonies (see *ibid.*: 4).

On this solid theoretical and empirical basis, her career went on smoothly. In 1950, she was invited by Stone to join him at the Department of Applied Economics in Cambridge (established with Maynard Keynes in mind as Director before he died in 1946). The next step was teaching, ‘and as a clear and direct thinker and speaker, [Deane] did very well. She became a Lecturer in Economics in 1961, Reader in Economic History in 1971, and Professor of Economic History for the two years before she retired in 1983’ (Harte 2012).

Deane was an excellent and efficient organiser:

She edited the *Economic Journal* from 1968 to 1975, and she was President of the Royal Economic Society in 1980–82. For many years she represented the Royal Economic Society on the Council of the Economic History Society. She worked hard for Newnham College in Cambridge, where she was an active Fellow from 1961. She was made a Fellow of the British Academy in 1980. [‘In 2010, she was made Distinguished Fellow of the History of Economics Society’ (Harcourt 2012).] Direct, careful, caring, sensible, brisk when necessary, non-nonsense; she had a look in her eyes that was [sometimes] sceptical, certainly undermining of pomposity in any form (Harte 2012).

2 Economic History and Economic Development

Deane’s work in economic history—Deane and Cole (1962) and Deane (1965)—grew out of her fieldwork on colonial national accounting (Deane 1948, 1953). Both are pieces of primary empirical research in the most difficult conditions, representing truly pioneering work, as emerges from a review of her main publication on the subject (Deane 1953): ‘With the aid of a clear style, great powers of analysing, selecting and presenting economic data, and an admirable appreciation of social factors in the motivation of economic

behaviour, she sets out the kind of investigation which can guide us to a more efficient general system of colonial social accounting' (Trent 1954: 170).

Indeed, to

compile social accounts with the emphasis on money transaction for an economy which is wholly or partly subsistence tells us only part of the story and must give a distorted picture. No general rules have yet been devised for the measurement of subsistence output. In this connection Phyllis Deane's pioneering work deserves the greatest admiration. With the aid of anthropological information she managed to compile social accounts for three Central African semi-subsistence societies: the Tonga, Lozi and Ngoni. She formulated a comprehensive questionnaire and used it to collect household budgets of all income, output and expenditure in money as well as subsistence terms. Her calculation of total income (excluding subsistence crops) is based on the assumption that barter and subsistence income have the same relation to cash income as barter and subsistence property to cash property ... Her experiment brings out clearly the inter-relationship of the social sciences. Village social accounts must necessarily be based on anthropological information. We must know the social system and values of a society before we can attempt to measure their economic activities (ibid.: 171).

Deane's fundamental and pioneering work on the quantitative basis of development economics was a prerequisite for her solid and brilliant achievements in economic history represented by Deane and Cole (1962), Mitchell (with Deane) (1962), as well as Deane (1965, 1973).

Negley Harte denotes Deane's 1962 publications written with Cole and Mitchell, as

two triumphant products [emerging from her work at the Department of Applied Economics], books that mark an era in British economic history ... For a generation, these were the central works in a tradition going back to Gregory King and Adam Smith. 'Deane and Cole' and 'Mitchell and Deane' mark the beginning of the high point of British economic history as a discipline (Harte 2012: 2).

British Economic Growth, 1688–1959, begins with the Glorious Revolution of 1688, a

starting-point...imposed by the nature of the data available. To this period belong the first regularly compiled statistics of English overseas trade, the first acceptable population estimate and the first systematic contemporary estimate

of national income and expenditure. Unfortunately it was not the beginning of a new era in economic statistics. More than a century passed before the administrative records yielded as good a basis for a quantitative analysis of the English economy as they had in the time of Gregory King. In their completeness and internal consistency his estimates remained unique until the twentieth century ... From King's manuscripts and working sheets and from the contemporary sources in which his data were analysed it is possible to construct and outline a set of social accounts for England and Wales [which is set out in the most fascinating table 1, page 2] (Deane and Cole 1962: 1–2).

Almost certainly, the experience gained through her work on colonial national accounts enabled Deane to fully and ingeniously put to use the data collected by King to set up a system of social accounts for England and Wales in 1688. This is typical of her research: later work grew out of former work, always with a view to broadening and deepening knowledge on socio-economic and political matters. What is materially significant about the findings contained in *British Economic Growth, 1688–1959* is the large share of exports in national income which reflects the intense commercial activity in the mercantilist era and the absence of a subsistence sector, so important in twentieth-century colonial economies, indicating that late seventeenth-century England and Wales were already monetary production economies to a large extent.

The solid conceptual and empirical basis provided by King associated with modern Keynesian insights on national accounting sets the stage for the writing of *British Economic Growth, 1688–1959*. The research programme was

to compile and interpret long-term national income statistics, or approximations thereto, for the whole of the period of the British industrial revolution and its aftermath ... By describing the broad background trends in population, value of money, wages and international trade, we seek to indicate some general directions for the economy over the very long period (ibid.: 4).

A central conclusion of the book is set out on page 309:

Throughout the past two and a half centuries international trade has been a strategic factor in British economic growth. Overseas markets gave an outlet to industries which would have operated less efficiently within the confines of domestic demand: imported raw materials provided bases for innovation and specialisation: foreign investment offered profitable employment to capital which found home prospects unattractive. It is only within the past century, however, that international trade has come to play a dominating role by virtue of its sheer weight in total economic activity.

This conclusion confirms that export-led growth is typical for monetary production economies in which economic activity is governed by effective demand. On a theoretical level, Nicholas Kaldor, a Cambridge colleague of Deane, has indeed argued that, in the era of increasing globalisation, long-period effective demand is governed by exports and that consumption *and* investment are derived demand (Kaldor 1989).

British Economic Growth, 1688–1959, and lecture notes on development economics to graduate students led to Deane's major work in economic history: *The First Industrial Revolution*. On the aim of the book, she says:

What I wanted to do most of all was to introduce the economist's approach into economic history—much more than had so far characterised existing texts. I wanted to use economists' methods of analysis and bring in enough economic theory to fit economic history into a first year course for students working towards a degree in economics (Deane quoted in Crafts and Humphries 1996: 7).

One cannot do economic history without economic theory! Marshall and Keynes would have subscribed without hesitation. Deane's emphasising this fundamental issue shows how deeply she is embedded in the Cambridge tradition of economics or, more appropriately, political economy.

The book is written very clearly, fully mastering an immensely complex subject, showing thus Deane's commanding knowledge of the matter she is dealing with. The clear and elegant literary style and the comprehensive and balanced vision might let the reader forget that a dramatic event of world historical importance is being pictured. Given this, Deane, quite naturally, takes an intermediate position as to the much debated question of *continuity* or *discontinuity* in history:

Those who, like [John] Nef, choose to emphasise the underlying continuity of history will trace the origins of the process of industrialization back to earlier centuries. Those who, like [Walt] Rostow, prefer to focus on the significant discontinuities of history will stress the revolutionary character of the changes taking place in relatively short periods of time and will look for the crucial watersheds, the irreversible upturns in the statistical series. These are in methods of historical analysis and interpretation rather than disputes about what actually happened in history. To understand the process of economic change one needs to take both approaches into account, and to recognize the significant discontinuities in the 'seamless' web of history (Deane 1965: 4).

This is a typical case of the Cambridge economist striving to take a balanced view of things, recalling Marshall's endeavour for reconciliation and synthesis.

In her *First Industrial Revolution*, Deane first considers four interrelated revolutions: the demographic, agricultural, commercial, and transport revolutions. The agricultural revolution is fundamental and leads to a rising agricultural surplus, which is required to feed a rising population, including the workers active in the industrial sector. With the industrial sector growing, a cumulative interaction between agriculture and industry sets in, resulting in an internally driven process of economic development, as is pictured in book three of *The Wealth of Nations*. Given this, ‘the revolution in industry and the revolution in agriculture were part of the same process’ (ibid.: 51).

However, the internal development process pictured by Smith must be set in motion through some kind of autonomous effective demand. Most important are earnings from exports, which, in a monetary production economy, initiate a cumulative process of demand, production, and growth (Kaldor 1989); this is what occurred within the framework of the Industrial Revolution. The crucial role of international trade is duly recognised by Deane. Indeed, the most common way, perhaps, leading an economy from a pre-industrial to an industrial state is to exploit opportunities provided by international trade. By selling surplus domestic production abroad in return for goods which are scarce at home, the range of goods and services coming on to the home market can be widened, while increasing the value of domestic output. In widening the potential market for domestic producers, foreign trade encourages them to specialise, to develop special skills and techniques of economic organisation, and to reap the economies of large-scale production. This broadening of their economic horizons constitutes an incentive to greater productive activity and helps to break up the economic inertia which so often inhibits economic development (see Deane 1965: 53).

Deane admits that Britain was in a favourable position with respect to international trade. Indeed, around 1750, woollen textiles still accounted for well over half the value of English domestic exports. But by this time, the Atlantic trade had been opened and English plantations in the West Indies had greatly extended the range of commodities that English merchants could sell in Europe. Like the spices and tea of the Far East, the West Indian products—sugar, tobacco, cotton, indigo, and dyewoods—were valuable products unobtainable in Europe and rapidly became necessities of life. In the first half of the eighteenth century, the volume of English re-exports had increased by 90%: in the next half-century, the re-export trade expanded more than twice as fast (see ibid.: 54).

In addition, domestic exports increased impressively from the 1740s to the 1770s, as is implied in Figure 1 set out on page 68 of the *First Industrial Revolution* (the immense work required to establish this figure cannot but

be admired). Hence, around 1770, effective demand originating from the high purchasing power even of the less prosperous parts of the British population, from the incomes created through the building of roads and canals, and, most importantly, from re-exports and domestic exports, exerted a demand pressure on the British economy essential to bring about the Industrial Revolution, because traditional methods of production (manufactures, hand-craft production) were no longer sufficient. However, there were still major bottlenecks preventing the Industrial Revolution from coming into being: 'The most crucial and general of the bottlenecks limiting the expansion of the British economy on the eve of the British industrial revolution... were a shortage of wood and the shortage of power' (ibid.: 136). These bottlenecks were overcome through the 'most important achievement of the industrial revolution [which] converted the British economy from a wood-and-water basis to a coal-and-iron basis' (ibid.: 137). This was made possible through two crucially important 'eighteenth-century inventions which set the stage for the industrial revolution by precipitating a continuous process of industrialization and technical change, and hence sustained economic growth [namely Watt's improved] steam engine on the one hand, and on the other, Cort's puddling process which made a cheap and acceptable British malleable iron' (ibid.). 'The adoption of a metal using technology employing decentralised sources of power, which [these] inventions permitted, lies at the heart of the first industrial revolution' (ibid.: 139). On pages 137–139, Deane pictures this fascinating process.

Now, in the last quarter of the eighteenth century, the Industrial Revolution was ready to unfold: effective demand was there, mainly on account of Great Britain dominating world markets, and technology, too. Subsequently, the factory and the machine went on to conquer Britain, then Western Europe. France and Germany and other countries had to industrialise, the alternative being to perish, as Karl Marx had forcefully stated. Around 1850 Britain dominated the world in a way unprecedented in history. The long period of peace from 1815 to 1914, interrupted only by the Crimean War of 1853 to 1856, was indeed *Pax Britannica*. No major war was possible on account of British economic, political, and military supremacy. Deane then goes on to consider the role of labour and of capital, and of the banks. The superiority of Great Britain was so crushing that free trade could be adopted. The government played an important role, mainly through the British Navy, which protected sea routes and harbours. Economic growth was, in fact, cyclical. Long cycles were superseded by medium- and short-term cycles. Standards of living rose, in part also for the working classes; however, Deane also considers social issues, that is, the phenomenon of widespread poverty of the working people, which was to become the Social Question (Chapter 15).

Considering the overall achievement, Deane concludes that, endowed as it was with abundant labour, a limited and exhaustible heritage of land and other natural resources, a modest propensity to save and invest and other natural resources, and a government which preferred to leave economic development to the free play of private enterprise, the British economy came through its Industrial Revolution with a relatively low growth potential by comparison with most of the countries which industrialised later. Several decades of easy success made its entrepreneurs believe that they could avoid rapid changes. Around 1850 British manufactures and British machinery were technologically superior, except in a few special cases, to those of any other country. But it was a matter of time only before faster-growing rivals with a clear trail to follow and stronger incentive to invest and to innovate would begin to outstrip her in their readiness to reduce costs and thus to threaten her virtual monopoly in world markets. When these rivals found governments which were ready actively to assist the industrialisation process, through tariff policy, for example, the end of British industrial supremacy was definitely in sight (see *ibid.*: 295).

Deane's *First Industrial Revolution* is a monument in the field of economic history, and it was an immense success: two editions and a great number of reprints, the second edition was reprinted ten times, and all editions are to be found in around 1,500 libraries worldwide. The book has all the hallmarks of a scientific bestseller. Written in a clear and elegant style, it represents a masterful presentation of a most complex subject. It is certainly one of the best, if not the best, work on the socio-economic and technical aspects of the *Great Transformation* as Karl Polanyi called it. As such, Deane's book is an excellent introduction to the subject for the general reader, as well as for students, teachers, and researchers, and this is very likely to remain so. Indeed, the *First Industrial Revolution* has become a classic.

3 History of Economic Ideas and Political Economy

When Deane's article on the Industrial Revolution in Great Britain was published in 1973, her mind had already turned towards the history of economic thought. *The Evolution of Economic Ideas* came out in 1978, followed in 1989 by *The State and the Economic System: An Introduction to the History of Political Economy*. Deane felt the need to follow up the thinking on economic phenomena—value and price, distribution, employment, growth, money, and international trade—that had accompanied the evolution of industrial economies. She has indeed insisted on theory being required to understand changing socio-economic states

of affairs and to set up economic policies, which, in turn, shape the evolution of economies and societies. Once again there is a remarkable continuity in her thinking: one thing evolved out of another, almost by necessity.

The Evolution of Economic Ideas puts 'some of the current theoretical controversies into long-term perspective by tracing their historical antecedents' (Deane 1978: back cover) and, as such, pictures how economics as a science has evolved. However, *The State and the Economic System* is conceived as an introduction to the history of political economy, that is,

the development of economic knowledge over the past three hundred years with particular reference to the ways in which the broader contexts of moral, scientific, and political ideas or events have influenced successive economists' vision of the operations of the changing economic system and their views of the scope for purposive State action to shape the process of change (Deane 1989: vi).

In *The Evolution of Economic Ideas*, Deane starts by observing that there are basically two approaches to a study of the development of economic ideas. The first concentrates on the sequence of change in the theories, concepts, and analytical techniques which constitute the substance of the discipline; the second traces the historical process of change in the way successive generations of scientists have adapted their explanatory techniques to attempt to solve the problems they regarded as important. This book shifts the weight of its emphasis in the direction of the second approach and seeks to interpret the history of economic thought as a process of change in the ideas of successive generations of economists (see Deane 1978: ix).

The book was very well received:

Current fashion among historians of economic thought dictates an 'internalist' approach to the evolution of economic ideas. Phyllis Deane is most unfashionable. She refuses to consider the development of economics as simply a process of analytical refinement, a succession of research programmes subject to an internal dialectic of scientific discovery. She refuses to deny that there is an important influence of events and policies on economic theory. And she even refuses to abandon the paradigmatic perspective on the structure of scientific revolutions. We should be thankful that Deane is an economic historian, writing outside her field of professional expertise. It is a refreshing change (Stevens 1979: 790).

Deane's originality emerges most clearly through the position she takes on the work of Adam Smith, whose fundamental theory of society is set out in *The Theory of Moral Sentiments*. Here, Smith argues that society is designed to

a Divine Plan, which operates so as to maximise human happiness by means of the interplay of certain moral sentiments which are part of human nature. Social and individualistic, selfish and altruistic motives all play their 'necessary part of the plan of the universe' for social harmony. In *The Wealth of Nations*, the basic framework of this theory of society is taken for granted, and Smith goes on to work out a special case of its application—the economic case (see Deane 1978: 8–9).

This is highly important because there has been an extended discussion on whether *The Theory of Moral Sentiments* and *The Wealth of Nations* form a larger entity or whether they are separate works. Deane tends to find the former position more plausible as do philosophically minded interpreters of Smith. This has far-reaching implications. Most importantly, natural prices and wages are socially appropriate or socially fair prices, making sure that producers and workers get fair incomes. This in turn implies that the principle of distributive justice is broadly realised. Natural prices and wages thus constitute social–ethical centres of gravitation around which market prices and wages fluctuate, and constitute as such variants of the just price of the Scholastic doctors.

This way of interpreting Smith, pointing in the direction of Ricardo and classical–Keynesian political economy, can hardly be found in modern volumes on the history of economic ideas. Indeed, in her chapter on Smith's theory of value (Deane 1978: Chapter 2), Deane points to the modern interpretation of Smith:

The concept of a natural price gives Smith's theory of value a pivotal position in his general theory of a harmonious economic order in which the self-interest of individuals operating in a freely competitive market economy tends naturally to produce an optimum allocation of resources and thus to maximise total output. At the same time it connects his theory of value with his theory of distribution by linking the commodity markets to the factor markets (ibid.: 24–25).

Here, Smith appears as a precursor of neoclassical economics.

Deane's treatment of Smith illustrates the profound knowledge she possessed of the complex works of the great economists. This enables her to bring out the essential features of these works and of the important paradigms and to sketch the process of transition from one paradigm to another. In doing so, she puts three great issues to the fore: value, growth, and money (see ibid.: xv). Deane's treatments of monetary theory and the functioning of monetary institutions are particularly interesting, especially given her intimate knowledge of British economic history.

Deane considers the classical, neoclassical, Marxian, and Keynesian paradigms. The core of classical—Ricardian—theory is given by the labour theory of value, from which the prices of production deviate on account of the varying durability of fixed capital, the surplus, and the marginal principle of distribution and profits, with the rate of profits and the rate of growth tending towards zero as population grows as less fertile lands have to be cultivated. There were also controversies in monetary theory going on, notably the Bullionists versus the Banking School. Here, Deane is at her best. However, she goes on to explain why classical theory has taken on this shape. Although Smithian political economy had achieved a certain measure of academic recognition, its fashionable appeal and, above all, its attraction to active thinkers depended not on its academic reputation but on its immediate relevance to contemporary policy issues. The problems attracting the attention of the early nineteenth-century economists were about such basic topics as economic growth (the population explosion), commercial policy (the Corn Laws), and inflation (the high cost of bullion). Political economy was expected to provide a simple and effective method of analysing these complex problems, and hence a reasoned route to useful policy prescriptions. This brought to McCulloch's lectures men such as the Chancellor of the Exchequer, the Lord Mayor of London, and a bevy of bank directors, and induced an acute mind such as Ricardo's to retire from the stock exchange to write a book on the principles of political economy (see *ibid.*: 72).

There was a great confidence in science to solve problems among the classical economists to which Ricardo greatly contributed not least through his very clear way of arguing.

In Chapters 7 and 8 of *The Evolution of Economic Ideas*, Deane sketches in a masterly way the transition from classical political economy to neoclassical economics. In the former, decisively shaped by Ricardo, the social process of production stands at the centre of analysis. The fundamental prices are cost based, whereby the prices of production deviate from labour values; the surplus principle of distribution determines wages and profits, which are saved and invested, implying that growth is of primary importance. Now, Ricardo had been concerned by two shortfalls in his theory of value. The first was that a labour theory of value did not seem able to explain changes in value through time: for as one admits the possibility of capital with different degrees of durability being involved in the production process, then a rise in the real wage rate could alter relative prices independently of relative labour inputs by raising the prices of labour-intensive products relative to capital-intensive products. The second was that he had no answer to the question of what determined absolute value, for there was no commodity whose value was not independent of distribution, that is, the real wage rate or the rate of profits (see *ibid.*: 115).

As a consequence, the founders of the marginal revolution concluded that Ricardo's 'labour-embodied theory of value had led English classical economics into a *cul de sac* from which there was no escape unless one were prepared to start from a new set of premises and with a new set of analytical tools' (ibid.).

The great marginalist systems created by Jevons, Menger, Walras, and Marshall came into being between 1870 and 1890. The marginalists shifted the whole emphasis of economics from the social process of production and cost-determined natural prices, sometimes called prices of production, to the market and market prices, determined by supply and demand (Marshall), with supply being governed by marginal costs and demand by marginal utility. Marshall became the new leader of the neoclassical school, although he 'did not publish his *Principles of Economics*, the bible of the English neo-classical school, until 1890; but by then he had been teaching economics for twenty years in Cambridge, Bristol and Oxford, and he had indoctrinated the inhabitants of half the economic chairs in England' (ibid.: 102). As Deane continues, the fact that the neoclassical economists were able to retain unimpaired the classical bias towards economic individualism and laissez-faire up to 1914, and the ideological overtones implied in the policy conclusions deducible from their analyses, may have been mainly due to the success of the neoclassical paradigm, not to its problem-solving qualities. The problems of value and distribution which had preoccupied the Ricardians were solved, or, more accurately, swept under the carpet, by a simple process of definition. The problems of growth were outside the effective range of marginal analysis and further consideration of them was consciously postponed. Moreover, the very jargon of pure economic theory, for example, the notions of 'rationality', or 'perfect' competition, of an 'optimum' allocation of resources, helped to accentuate its ideological overtones and to lend ostensible 'scientific' support to a political *status quo* which depended on accepting a philosophy of economic individualism and harmony (see ibid.: 101).

This important statement is still valid today, and, yet, in between, there is the Apocalyptic Age spanning 1914–1945 with two world wars and the Great Depression, plus the financial crisis which started in 2007–2008.

Hence, in spite of two powerful paradigms, the Marxian alternative (Chapter 9) and the Keynesian revolution (Chapter 12), of which Deane provides excellent presentations, the neoclassical paradigm is still there, even growing in strength after her *Evolution of Economic Ideas*. Unimpressed by this fact, she went on working on the subject and published in 1989 *The State and the Economic System: An Introduction to the History of Political Economy*. The book immediately received favourable reviews. Thus, Cohen states:

This slim volume ‘on the interface between what seventeenth-century writers... would have called the polity (the civil order of society) and the economy’ (p. v) sketches admirably ‘the ways in which in the broader context of moral, scientific, and political ideas or events have influenced successive economists’ vision of the operations of the changing economic system and their views of the scope for purposive State action to shape the process of change’ (p. vi). While others often explain the interface between economic history and theory simplistically in terms of the historical or ideological determination of theories, Deane provides an exceptionally balanced descriptive narrative which reveals the richness and the complexity of the interface (Cohen 1990: 240).

A good example is Chapter 5, which sets out the

ways in which changing historical conditions led to changing policy concerns ... A demographic surge is linked to Malthus’s work. The need for monetary policy to manage the financing of the Napoleonic Wars is seen as a stimulus for the monetary theories of Thornton and Ricardo. Accelerated industrialization in Britain and associated changes in income distribution are used to explain Ricardo’s concern for distribution and its impact upon growth (*ibid.*).

Significantly, in Chapter 6, Deane presents Mill, who managed to establish a great synthesis, reconciling contradictions, which ‘effectively absorbed the weight of the Romantic onslaught on Ricardian political economy and justified the hard core of economic liberalism in epistemological, philosophical and political terms’ (Deane 1989: 101). In the same chapter, there are also most insightful passages on Marx who forcefully opposed the growing ‘consensus that the fundamental theories and laws of political economy must be detached from moral principles and value judgements [and his] analysis of the way the modern industrial economy operated was simultaneously a moral indictment of its monumental injustices’ (*ibid.*: 108). The concluding chapters are on most relevant and challenging topics: ‘From Political Economy to Economic Science’, ‘Economic Science in an Unstable World Economy’, and ‘The Rise and Fall of Economic Management’.

One can hardly imagine a more appropriate book to introduce the general reader, the teacher, and researcher as well as the student to political economy. There are many surprising, sometimes puzzling and contradictory, statements that are nevertheless thought-provoking, confirming thus Keynes’s observation that the serious study of the history of economic thought leads to the emancipation of the mind.

4 Political Economy and Economic Science

Having dealt with this issue already in Chapter 7 of Deane (1989), Deane took it up again in a lecture delivered on 9 May 1990 at the occasion of the hundredth anniversary of the foundation of the University of Fribourg—Switzerland (1989–1990). Thus, this lecture, entitled *Political Economy and Economic Science*, was delivered a century after the publication of Marshall's *Principles of Economics*, at a time when

economics was an infant academic discipline, recently recovered from a painful crisis of confidence in its scientific identity ... The founders of political economy—e.g. the Physiocrats, Adam Smith, the English classical economists, and even their unorthodox follower Karl Marx—had started with the confident presumption that there was a natural economic order whose laws could be discovered by applying objective principles of analysis (analogous to those of the natural sciences) to real world phenomena ... They took it for granted that political economy was a science. But by the middle of the nineteenth century the process of industrialisation, and especially its uneven development between regions and over time, had demonstrated that the section of the real world to which economists sought to apply scientific methods of enquiry was subject to change on a scale the physical world was not (Deane 1991: 173).

The classical vision of a naturally self-equilibrating economy was starting to be questioned:

Even in Britain, certainly by the 1840s and 1850s, the widening gap between rich and poor generated by industrialisation in the leading industrial nation was putting the advocates of economic freedom on the defensive against the claims of economic justice. When, in the early 1870s, a prolonged boom in international trade was followed by world market depression, the need for a politically more relevant perspective for economic theory and analysis was felt by the acknowledged leaders of the discipline throughout the western world—and not merely by its critics (*ibid.*) ... [Around 1870] time was ripe for a new approach to economic analysis and the reconstruction of economic theory which took place in the last three decades of the nineteenth century represented a response to that challenge. The international dimension of the response was dramatically underlined by the virtually simultaneous appearance, in three geographically separate locations, of three innovative texts which launched the so-called marginal revolution in economic theory. They were Jevons' *Theory of Political Economy* and Menger's *Principles of Economics* (both published in 1871) and Walras's *Elements*

of Pure Economics [1874] ... Each of these theoretical innovators reinterpreted the theory of value as a theory of market exchange and shifted the central focus of economic analysis from a macroeconomic to a microeconomic perspective ... Their common characteristic was a clarification and purification of would-be scientific theory—achieved by detaching it from its political and ethical implications in order to facilitate the professional consensus that seemed prerequisite to a cumulative advance of economic knowledge (ibid.: 173–174).

Deane's insight is impressive. Broadly since the mid-1970s, the neoclassical economists have indeed achieved a professional consensus that is overwhelming. The neoclassical mainstream has succeeded in creating an internationally recognised community and set of methodological standards, based on mathematics and econometrics, to the almost total exclusion of alternative strands of thinking. These developments were in fact rendered possible as a result of events that occurred in England in the year 1890 which

was a landmark in the professionalisation of the discipline. For two books were completed in that year which together provided the textual basis for the system of thought destined to replace classical political economy in the English-speaking world. The first was Marshall's *Principles of Economics* which became the Bible of neoclassical economics. The second, by John Neville Keynes, was the treatise [published in 1891] that was designed to put an end to the tedious methodological controversies of the 1870s and 1880s [for example, the German *Methodenstreit* between historians and theoreticians]—disputes that had seemed to many, inside and outside the discipline, to be discrediting the scientific credentials of classical political economy ... Accordingly, Neville Keynes, with the advice and encouragement of his teacher Marshall, set out the taxonomic ground rules that should enable the academic student of economics to distinguish assumptions and findings about what is (constituting the positive core of economic science) from...statements of what ought to be (whose scientific justification belongs to the moral sciences) and, on the other hand, prescriptions for achieving desired ends (i.e. the art of political economy) ... The importance of this system of classification was that it allowed the theoretical economist to insulate his fundamental theorems from accusations of ideological bias, or immorality, or relativity, and at the same time from failures of economic policies (ibid.: 174).

As Schumpeter later put it, the Marshallian–Walrasian system constitutes the Magna Carta of economic theory. A sound alternative way of theorising is not possible. For Schumpeter, 'the only fundamental cleavage in modern economics is between good work and bad' (Schumpeter quoted in ibid.).

This somewhat arrogant attitude has been undermined by the Great Depression in the 1930s, the so-called Keynesian episode of 1945–1973, and the financial crisis which started in 2007–2008. Even serious criticism, like the capital theoretic critique of the 1960s, could not damage the neoclassical citadel, the main reason being that the critics were not able to set forth a convincing complete theoretical system based on a vision of man and society other than liberalism or socialism.

In fact, what

Marshall offered the rising generation of would-be economic scientists in the 1890s was his personal vision of a Darwinian model of the advancement of economic knowledge, to replace the Newtonian vision that had inspired Adam Smith. His *Principles* explained the history of what he insisted on calling ‘economic science’ as a continuous evolutionary advance in economic knowledge, building on theoretical foundations laid down by Smith, Ricardo and Mill, and embodying the insights of both the historical and mathematical schools (Deane 1991: 174).

The academic economists were now no longer ‘obliged to develop or defend timeless, universal, economic laws. Instead, having admitted that real-world problem situations reflect environmentally specific conditions, they could try to arrive at generalised historical explanations of how, why and where such situations were likely to arise’ (ibid.: 174). Each historical situation may now be explained, even the great crisis of the 1930s, which Samuelson claimed was a *monetary accident*. In modern terms, explanation is by supply and demand shocks. In terms of the Walrasian model, everything may be explained *ex post* through parameter changes within the general equilibrium model:

Marshall had already extricated political economy from the false position of being a repository of simple economic truths and infallible policy prescriptions in 1885, when he told the Cambridge students gathered to hear his inaugural lecture that: “*that part of economic doctrine which alone can claim universality has no dogmas. It is not a body of concrete truth but an engine for the discovery of concrete truth*” [italics in Deane] ... Marshall’s vision of an infinitely progressive cumulative research program in economic science, and his rejection of doctrinal dogma, had immense inspirational force for the professionalisation of economics in the twentieth century (ibid.: 174–175).

All this tells us is that Marshall’s influence is still overwhelming at present. His *Principles* is certainly by far the most important book on economics ever written. Indeed, all the textbooks we have at present derive somehow from Marshall. In fact, it is really Marshall, assisted by Neville

Keynes, who built up the extremely solid neoclassical citadel, which still resists the gunfire of the critics at the beginning of the twenty-first century. It is an important achievement of Deane to have brought out this fact very clearly and succinctly in her 1990 Fribourg lecture. This aside, she goes on to say that there

remained, however, an inescapable tension between political economy and economic science—tension that could not, in practice, be bypassed by detaching positive economics from moral and ethical considerations, or pure-theoretic from applied economics. The object of any economic scientist's research is ultimately (if not always proximately) the solution of certain practical policy problems ... Marshall himself envisaged the cumulative results of systematic economic research as being useful and accessible to practical men of business as well as legislators ... However, being more sensitive than most Marshallians to the tension between the pure science of economics (of which he was an acknowledged master) and the art of political economy (which he wished to serve), he made it his rule 'to avoid taking part in the discussion of a burning political question even if it contains a large economic element' (ibid.: 175).

Marshall's extreme reluctance to take a position in the 'free trade versus protection' issue is an example (see ibid.: 175–177).

It seems reasonable to argue that Chapter 7 of Deane (1989) and her 1990 Fribourg lecture were the starting points for the writing of her last book on *The Life and Times of J. Neville Keynes: A Beacon in the Tempest* (Deane 2001), 'one of the finest jewels in the crown of our profession' (Harcourt 2012). This becomes evident from the, perhaps, most important chapter, number VIII, 'Faculty Conflicts and Tripos Reform':

Neville Keynes was an unreconstructed product of the pre-1883 moral sciences tripos. He held that, at the undergraduate level, economics should be taught alongside logic, psychology and ethics; and that specialised study of it was best reserved for mature students ... He dreaded the establishment of a separate economics tripos because he rightly foresaw that it would lead to political economy being excluded from the moral sciences tripos altogether (Deane 2001: 243).

However, after a Senate vote on 6 June 1903, 'Marshall had finally achieved his ambition and Cambridge had an economics tripos' (ibid.: 245). *Political economy* considering the political society as an entity had become *economics* with the autonomous individual moving to the fore. This heralded the splitting up of the moral sciences, which constituted a broad unity

because society and the state were considered an entity with the economy taking on an ancillary role. For his part, however, ‘John Maynard Keynes had no inhibitions about mixing politics with economics ... Keynes’s principal contribution to the art of political economy was to bring into sharp focus what is still its basic problem, i.e. identifying the scope for government intervention’ (Deane 1991: 177). In fact, Keynes’s life work implies a clear movement away from economics in the direction of political economy, grounded on a social and political philosophy. Given this, Maynard Keynes forcefully initiated a return to the *social and political sciences*, the modern form of Neville Keynes’s *moral sciences* (on this, see Fitzgibbons 1988; Bortis 1997, 2013). In this context, Maynard Keynes always insisted on economics being a moral science.

Only a political economist like Deane, being *simultaneously* an excellent economic historian and an outstanding historian of economic ideas, could bring the issue of *political economy* and *economic science* into sharp focus. Based on this fundamentally important distinction, the way is now open to arguing that both approaches to dealing with economic phenomena involve two fundamentally different paradigms, that is, *classical–Keynesian political economy* and *neoclassical economic science* or *neoclassical economics* (Bortis 1997, 2010, 2013). This is likely to open new perspectives in the development of economic theory and policy. Indeed, neoclassical theory, founded on the timeless principle of supply and demand, grounded, in turn, on the marginal principle, does *not* represent the *only* ‘engine for the discovery of concrete truth’. Classical–Keynesian political economy, *in a Keynesian vein a moral and cultural science*, is based on three principles: the labour value principle, the surplus principle, and the principle of effective demand (Bortis 2013), which also provide a most powerful engine for the discovery of concrete truth. It is open to discussion which of the two theoretical systems is more plausible.

5 Conclusion

Phyllis Deane’s academic achievement starts from groundwork on colonial social accounting, goes through economic history—the *First Industrial Revolution* most importantly—on to the history of economic ideas, and, finally, down to fundamental issues regarding economic science and political economy, thus opening the way to new developments in economic theory.

In addition, Deane served the Faculty of Economics and Politics at Cambridge with distinction and in difficult times: ‘Phyllis was both respected and liked by everyone in Cambridge’s deeply divided Faculty. Her own views were explicit

and clear and her fair mindedness and balanced approach were a much needed Godsend. As a result she did far more than her share of committee work in the Faculty' (Harcourt 2012). Jane Humphries has also written a fine tribute to her:

Phyllis Deane was a pioneering scholar, inspirational teacher, caring mentor, and warm friend. It was wonderful to come up to Newnham and see Phyllis's work included in all the required reading lists. [And her] research represented a seismic shift in the tectonic plates of economic history. After her book *British Economic Growth 1688–1959* (1962), the discipline was on a different development path. Phyllis [also] was an excellent supervisor: critical but constructive, encouraging but with the highest standards (Humphries 2012).

An extract from the epigraph to her book on Neville Keynes (quoted from Goldsworthy Lowes Dickinson's memoir of Frank Ramsey by Maynard Keynes in *Essays in Biography*) appropriately characterises Deane's general attitude and her life work:

It does not become a Cambridge man [or woman] to claim too much for his university ... But there is, I think, a certain type, rare, like all good things, which seems to be associated in some peculiar way with my alma mater ... It is a type unworldly without being saintly, unambitious without being inactive, warm-hearted without being sentimental. Through good report and ill such men [and women] work on, following the light of truth as they see it; to be sceptical without being paralysed; content to know what is knowable and to reserve judgement on what is not.

In this sense, Phyllis Deane was a Cambridge economist par excellence in her own unique way.

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Robin Marris (1924–2012)

Adrian Wood

1 Introduction

Robin Marris, who taught in Cambridge's Faculty of Economics for a quarter of a century, was known to all economists of his time for his work on business corporations. The words 'managerial capitalism' in the title of his best-known book became a standard phrase in economics. He made significant academic contributions in other areas, too, and was an active contributor to policy debates, in which he was not afraid to espouse unpopular causes. What motivated him as an economist was a desire to improve the world, and he constantly used statistics, but all his work was underpinned by theory, which to him was what put economists ahead of ordinary mortals.¹

Marris was born on 31 March 1924 in London. He came up to Cambridge in 1941 to read economics at King's College. Supervised by Gerald Shove, he got a First in Part I of the Tripos in 1942, but then joined the Royal Air Force. Back in Cambridge in 1945, he was supervised by Richard Kahn and the retired Professor Pigou (who helped teach the ex-servicemen) and graduated with another First in 1947. His contemporaries included Harry

I am deeply grateful to many people who knew Marris for their help and information and to Jamie Galbraith, Mervyn King, William Peterson, and David Vines for comments on a draft of this chapter.

¹As I soon learned when he dismissed my first undergraduate tutorial essay as 'high-class journalism'.

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Johnson, Robert Neild, I.G. Patel, Eric Russell, and Aubrey Silberston. In an autobiographical sketch, Angus Maddison, a 1948 graduate, mentions Marris as one of the fellow students who most influenced him.

In 1951, after working in the Treasury and in Geneva as editor of the publications of the Research Division of the UN Economic Commission for Europe, Marris returned to King's as a Fellow and as an Assistant Lecturer in the Faculty of Economics (later an Assistant Director of Research 1955, Lecturer 1960, and Reader 1971). After writing for some years on other topics, he achieved worldwide academic recognition for his book on managerial capitalism (Marris 1964a), John Kenneth Galbraith's references to which in his 1966 Reith Lectures and *The New Industrial State* made Marris known also to a wider audience. He was invited to spend 1967–1968 at Harvard, where he was drawn into a lively public debate between Galbraith and Robert Solow.

During the 1950s and 1960s, Marris continued a connection with the Labour Party that had started in his student days. When Labour was elected in 1964, he took leave from Cambridge for two years to be Director of the World Economy Division in the Ministry of Overseas Development under Barbara Castle and Anthony Greenwood. Dismayed by the abuse of union power in the 1970s, Marris later became 'hostile to socialism', but 'never swung *spiritually* to the political right' (Marris 1996: 1; italics in original) and resumed his support for Labour in the 1990s, dedicating his last book (Marris 1999a) to Clare Short, then Secretary of State for International Development.

In 1959, Marris became King's College's Director of Studies in economics. A long series of students, including the present author, benefited from his creative mind and warm personality. Christopher Bliss remembers that 'he was an inspiring teacher, who always made me feel that I was not just a little student, still learning the trade, but rather a full member of the economics profession' (Bliss quoted in Wood 2013: 90). In the 1950s, Marris helped Piero Sraffa to direct the Faculty's research students, running a weekly seminar. Geoff Harcourt recalls that 'when I arrived, we all fronted up to RLM in the old Marshall library in Downing Street. He was extremely lively and helpful to us all. He tried to bring the Keynesians and the Robertsonians together, for example asking them to the same students' parties, but had little success' (Harcourt quoted in *ibid.*).

In 1976, after 25 years of research and teaching in Cambridge, Marris left forever. The reasons were partly personal—a new American wife—and partly professional—he saw no prospect of getting a chair. Professorships in Cambridge at that time were few and far between, and he was also a somewhat isolated figure in the deeply divided Faculty. As is illustrated by the comments of Bliss and Harcourt (across this divide), Marris avoided

taking sides, academically or personally, so no one was strongly on his side. Intellectually and emotionally, however, he never left Cambridge. His most ambitious later research (Marris 1991) was about Cambridge subjects and people.

From Cambridge, Marris went to a chair at the University of Maryland, where he was head of department for three years, and in 1981 to Birkbeck College of the University of London, where again he served for three years as head of department, and from which he retired in 1987. At Maryland, he succeeded in raising academic standards, but in both departments his headships were stormy – and at Birkbeck aggravated by his intolerance of Marxists.

Marris continued to work and write until late in his life about pressing economic and social issues. In 1996, he published a book on deprivation in the UK and followed it up with advisory work on education and crime, the latter for Charles Clarke, a former student who was then Home Secretary. Another book in 1999 addressed poverty at the global level. Marris wrote regularly for the press on macro policy and was strongly in favour of the UK joining the euro. He died on 25 September 2012.

A fuller account of Marris's life, with more personal details, is in Wood (2013). The following four sections discuss his research. The first considers his early writing, with particular attention to his book on shift work. The second and longest section is about his work on managerial capitalism, the third is about macroeconomics with imperfect competition, and the fourth is about his book on saving the underclass.

2 From the Treasury to Multiple-Shift Work

Marris's first publication (Robinson and Marris 1950),² defended UK government support for agriculture against the criticism that the resources would be better used for producing exports to buy food imports, on the grounds that the price elasticity of demand for UK exports was low. In Marris (1955) he argued on the same grounds that the UK government's entire policy of promoting exports was misguided.

In retrospect, and as his critics argued at the time, Marris was greatly underestimating the elasticity of demand, on the basis of work he had started with Kenneth Berrill in the Treasury in 1947. He inferred a price elasticity of about one-half

²This article, co-authored with Austin Robinson, probably played a part in Marris's return to a job in Cambridge soon thereafter. In a personal communication, Geoff Harcourt writes: 'We were told that Austin R brought Robin and Ken [Berrill] to Cambridge after the war as the two brightest grad. students in the country'.

from a long-standing inverse relationship between the UK's export volume and its terms of trade which in reality probably mainly reflected the effect of fluctuating commodity prices on the purchasing power of major UK trading partners.

His first solo article, in 1954, was a comparison between the UK and the Netherlands of the—still fairly novel—role of economics and economists in government. His evidence for the UK came from his time in the Treasury, though he had worked in the Central Economic Planning Staff (CEPS) (coordinating the policies of different ministries) rather than in the Economic Section with which his article is largely concerned and which as a matter of official policy refused to give any help with, or comments on, the article.³ By contrast, his evidence for the Netherlands came from extensive interviews with staff and publications of their larger and more open Centraal Planbureau.

The article, regularly cited over the next two decades, compares the UK's 'arithmetic' approach to macro forecasting with the 'algebraic' approach of the Netherlands, where the Planbureau, directed by Jan Tinbergen, used an econometric model (of which a five-person team could invert the matrix in less than three days!). Marris favoured the algebraic approach, although he was aware of the risks of 'its operators becoming wedded to belief in the stability of relationships (parameters) which are not in fact stable' (Marris 1954: 772). He admitted that neither system could as yet be shown to be superior to the other, but saw the way forward for the UK government as greater professionalisation of its economic work—which happened 10 years later with the formation of the Government Economic Service.

In a *Review of Economic Studies* (*RES*) article on measuring the share of the public sector (Marris 1954–1955), he neatly explains that the right measure depends on the question being asked and calculates an appropriate set of numbers, all lower than the often-claimed 40%. Having criticised in this article Ursula Hicks (among others), Marris in another *RES* article in 1957 criticises John Hicks for claiming that a Paasche index exceeding a Laspeyres index is evidence of a change in tastes. In this technically skilful article, which acknowledges help from David Champernowne, Harry Johnson, Richard Kahn, and Brian Reddaway, Marris also shows that Fisher's ideal index (the geometric mean of Laspeyres and Paasche) is 'more ideal than has sometimes been realised' (Marris 1957: 38), though he revised his opinion of Fisher downwards in a later article on the International Comparison Project (Marris 1984: 48–49).

Marris's first book, *Economic Arithmetic* (Marris 1958), is an applied statistics text, probably based on his Cambridge lectures, which approaches the

³ Evidence that he worked in the CEPS is in the first footnote of Marris (1955).

economy as a body to be dissected with a selection of instruments. As the word ‘arithmetic’ in its title suggests, following the distinction that Marris had drawn between the UK and the Netherlands, the focus of the book is not on statistical techniques, but on economic interpretation of data by fairly simple methods, informed by theory. The exposition is clear, engaging, and packed with practical advice. The most technical section, nearly half of the book, is on index numbers, reflecting Marris’s research interest in this topic.

In 1954, with funding from the Ministry of Labour, Marris began a research project on multiple-shift work, whose results were eventually published in *The Economics of Capital Utilisation* (Marris 1964b). Starting from the fact that capital equipment on average is used for less than a quarter of all the hours in the year, the questions were why this is so, whether it is desirable, and what if anything should be done about it. The first four chapters—almost half of the book, and the main cause of its delayed publication—set out a theory of the choice of the planned level of multiple-shift work by a profit-oriented business. These chapters creatively develop concepts and relationships that formalise a wide range of practical considerations, including coordination of different processes and demand-side restraints on the output of particular plants.

The next five chapters test the theory against a battery of evidence, including statistics from the 1951 Census of Production, enquiries involving employment exchanges, the factory inspectorate, bus companies (on transport difficulties for shift workers) and unions, and case studies of 11 industries based on questionnaires and interviews. The results confirmed that multiple-shift work was rare—involving only one-eighth of all employees—and concentrated on a few industries and large plants. Only in special circumstances is multiple-shift work profitable. The benefits of its more intensive use of equipment are usually more than offset by its higher wage costs (at that time partly because it was illegal for women to work at night).

Marris had initially believed that the low level of multiple-shift work was economically irrational—a result of ignorance and inertia—and that its widespread extension would be desirable. Several years of research changed his mind, so the policy implications of the book were more limited than he had anticipated. Nonetheless, the book, whose methods were greatly influenced by ‘local colleagues and traditions’ in Cambridge (*ibid.*: xvi), was widely recognised as a major pioneering contribution and is reported in Google Scholar to have been cited by almost 200 subsequent studies of various aspects of capital utilisation.

3 *The Economic Theory of 'Managerial' Capitalism*

What set Marris off on the line of research that was to make his name is unclear—it was not a topic on which others in Cambridge were working. The likely explanation is that an interest in the behaviour of firms acquired in his study of shift work led him to read and reflect on three important books about the firm that were published in the late 1950s. His thinking progressed during a 1960–1961 sabbatical at Austin, Texas, and Berkeley, including seminars at other US universities, where he forged enduring links with many leading American economists.

Marris's focus was on the separation of ownership from management in joint-stock companies (or 'corporations') and on the weakness of control by shareholders, which gave managers considerable discretion. This feature of corporations, which produced a large share of gross domestic product, had been highlighted by Berle and Means (1932) and again by Burnham (1941) and Gordon (1945), but with no impact on the theory of the firm in economics texts, which continued to assume that markets were usually supplied by a multiplicity of small owner-managed firms. Nor had economic theory come to terms with the challenge of Sraffa (1926), underlined by Kaldor (1934), to Marshall's view that decreasing returns to scale caused there to be an optimal size of the firm.

Three independent contributions to a new theory then arrived simultaneously. Baumol (1959), influenced by his experience as a consultant, produced a formal model of the firm in which the personal benefits to managers of running larger firms caused them to maximise sales revenue subject to a minimum profit constraint. Penrose (1959) brilliantly recharacterised the firm, not as the supplier of some specific good but as a team of businesspeople able to produce many goods and enter new markets by merger or diversification. Abandoning the concept of optimal size, her theory focused on the growth of the firm, and on limits to the speed of its growth—though only informally, as Marris (1961) noted in a complimentary review.⁴ In an analysis of competition as a dynamic process, Downie (1958: 63–67) also sketched a model of the firm in which managers want to maximise growth subject to various dynamic constraints.

Building on this earlier work, and taking returns to scale to be constant, Marris set out a formal model of the growth of a firm run by managers who want it to grow as fast as possible. He also explored the implications of his

⁴ Marris's review, probably written in late 1960 during his US sabbatical, stresses the need for a formal theory of the growth of the firm, but gives no hint that he was working on one.

model for competition, economy-wide growth, and welfare. Almost all his publications in this field were in the period 1963–1980, but in 1998, he produced *Managerial Capitalism in Retrospect*, a revised and abridged edition of his famous 1964 book, *The Economic Theory of 'Managerial' Capitalism*.⁵ The 1998 version, which also contains Marris's final assessment of work in this field, is the best point of entry into this aspect of his writings.

The first version of his model (Marris 1963) appeared as a lead article in the *Quarterly Journal of Economics* (*QJE*), with a star-studded list of acknowledgements to economists in Cambridge and elsewhere. Managers are argued not only to want to make their firms grow—to increase their salaries, status, and power—but also to be concerned about the risk of being dismissed after a successful takeover raid. Three dynamic constraints on the firm's growth are identified and modelled. One is the cost of diversification into new markets, which causes the firm's average profit margin on sales to decline with faster growth. A second is the internal organisational costs of growth (the 'Penrose effect'), which cause the firm's capital-output ratio to rise with its growth rate.

The third constraint is the need to generate finance for investment in new capacity. As borrowing is limited by risk to some ratio of capital and new issues of shares are rare (and so assumed to be zero), the growth rate of the firm is linked positively to its profit rate by the retention ratio—the share of profits not distributed as dividends—to which an upper limit is set by the fear of takeover. The three constraints, expressed as equations, are ingeniously stitched together in a diagram in which the growth rate of the firm's assets is traced as an inverse U-shaped function of its diversification rate, of which the managerial desire for growth causes the apex to be chosen.

In a series of publications over the next two decades, this model evolved in various ways. It is convenient, however, to jump to its final version in Chapter 6 of the 1998 book (which is almost identical to the form it had reached by 1980). In the diagram that sums up this version (Figure 40.1), the firm's growth rate is on the horizontal axis, while the vertical axis shows its valuation ratio—the stock market value of its shares divided by the book value of its capital stock. Faster growth at first increases but then decreases the valuation ratio. Shareholders would like to be at the peak of this curve, but managers would prefer a higher growth rate. How far down the slope they choose to go is constrained by their fear of takeover, which sets a lower limit (assumed in this figure to be unity) on the valuation ratio and thus fixes the growth rate.

It is of interest to consider what stayed the same between the first and last versions of the model, what changed, and why. One obvious enduring feature

⁵ Recognising that the 1964 book had been work in progress, the abridgement cut out about half of the original text and removed most of the original algebra. Its lively style, however, was maintained.

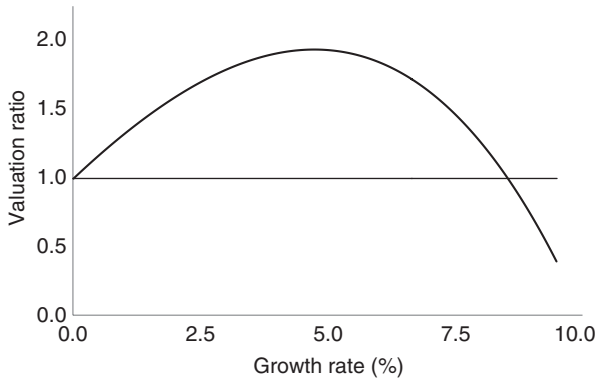


Figure 40.1 Determination of firm's growth rate

is the assumption, explained most fully in Chapter 2 of the 1964 book, that managers want to maximise the growth rates of their firms without running too much of a risk of dismissal as the result of a takeover. Another enduring feature is the upper limit on the gearing ratio—the ratio of debt to capital—which restricts growth by borrowing (although the utility of managers is assumed to depend on the scale of gross assets, not of equity capital).

All versions include a decreasing relationship between growth and profitability as a result of the rising cost of diversification, which is discussed and cleverly modelled in Chapter 4 of both editions of the book. But its rationale was broadened to include the costs of all the ways of expanding demand for the firm's products—research, advertising, acquisitions—while keeping its prices competitive. Moreover, the distinction between the effects of growth on the profit margin and on the capital-output ratio (originally suggested to Marris by Machlup) disappeared, and the 'demand' relationship became simply that between the growth rate and the profit rate on capital. Marris also came to believe that this relationship was decreasing only beyond a certain point, up to which the dynamics of growth increase profitability. In addition, he came to emphasise more clearly that his was a profit-driven model—growth-maximising firms were efficient in every sense other than growing faster than their shareholders would have wished.

What changed more radically was the 'finance' side of the model. An obvious new arrival, which made its entry in the 1964 book, is the valuation ratio (later reinvented by Tobin as his celebrated q). This variable, which required a formal treatment of the determination of share prices, was introduced to make the interests of shareholders—and their conflict of interest with managers—clearer than in the 1963 article. It also permitted a fuller analysis of the risks

of takeover, of which the 1964 book contained the first formal theory and laid the basis for much later empirical work.⁶

A notable disappearance was that of the financial variables—the retention ratio and the rate of issuance of new shares. Even before the *QJE* 1963 article, Modigliani and Miller had argued that shareholders should be indifferent to whether investments were financed by retaining profits or issuing new shares—all that mattered to them was the profitability of the investments. However, the assumptions required for this to be true, including complete certainty, seemed highly unrealistic to Marris, so in his 1964 book the valuation ratio depends both on profitability and on the financial variables.

Shortly after the book appeared, however, Marris's Cambridge and King's colleague, Richard Kahn, worked on the algebra of the valuation ratio and generated the same result as Modigliani and Miller, which caused Marris to capitulate (wrongly, in the present author's view).⁷ In later versions of his model, the retention ratio and the new issue rate play no substantive role.⁸ Kahn's formula, however, had the drawback that as the growth rate of a firm approached the market discount rate its valuation became infinite. Marris eventually solved this problem by making the firm's market discount rate increase with its growth rate, building on the analysis of share pricing under uncertainty in Lintner (1971).

In several publications, Marris explores the implications of his theory of the firm for competition and concentration in product markets—reconnecting with the origins of his theory in Downie's innovative analysis of the competitive process. He saw firms as competing at two levels (Marris 1998a: xix). At a low level, there is competition among the divisions of corporations (or quasi-firms) in mature markets—which governs the 'basic' profits of firms, before the absorption of part of these profits by spending on diversification and other demand-expanding activities. In Marris and Wood (1971: Chapter 9), he outlines a more general theory of oligopoly, based on the threat of entry, although most of his work on imperfect competition was done much later (Marris 1991).

At the higher level, to which Marris devotes far more attention, there is competition among whole firms, though of a sort which tends to generate

⁶In most versions of the model, the takeover risk constraint is expressed as some minimum value of the valuation ratio, but in a few versions the managerial utility function has a continuous trade-off between the growth rate and the valuation ratio.

⁷Kahn's analysis of the valuation ratio, published later in Kahn (1972), is based on assumptions just as unrealistic as those of Modigliani and Miller. Wood (1975) argues against these assumptions and puts forward a theory of profits (at both firm and economy-wide levels) in which the financial variables play a key role.

⁸Though it was sometimes convenient for the purposes of exposition to assume no new issues and use the retention ratio to link the growth rate to the profit rate (e.g. Marris and Mueller 1980: 47).

increasing concentration, a feature of capitalist economies that in his view is not explained by the neoclassical theory of the firm. In seeking to understand the concentration process, he repeatedly studied data on the persistence of firm profitability and growth and on the correlations among profit rates, growth rates, and firm size.⁹

Marris's fullest contribution in this field is his *de Vries Lectures* (1979), summarised and extended in Marris and Mueller (1980: 47–50), in which he combines his model of the firm with the stochastic theories of concentration pioneered by Gibrat and Steindl. More specifically, he shows that a stochastic version of his model generates a precise and predictable path of increasing concentration through internal growth and merger (simulated with a size-related takeover probability matrix). The trajectory of this path depends crucially on whether firm efficiency (in generating profits and using them for diversification) tends to increase or decrease with firm size.

The macroeconomic implications of his theory of the firm are also a recurrent theme of Marris's work.¹⁰ He believed strongly that the activities of managerial firms, which stimulated the expansion both of demand and of supply, had accelerated the growth of capitalist countries in the twentieth century. His ideas were skilfully elaborated by Odagiri (1981). One of their key policy implications is paradoxical: growth would be slowed by more competition, both in product markets (reducing the basic profits available to spend on diversification) and in the stock market (increasing the threat of takeover). Marris (1998a: xix, 164–165) emphasised the association of fast growth in post-war France, Germany, and Japan with the virtual absence of involuntary takeovers.

A side effect of this line of argument was to lead Marris into disagreement with his Cambridge colleagues about the relationship between the rate of growth and the rate of profit. The position of Kaldor, Pasinetti, and others, including the present author, was that the rate of profit is determined by the rate of growth, which could be taken as exogenous. The original Kaldor–Pasinetti linkage from growth to profits was through differential saving rates, but Kaldor (1966), without attribution to Marris, set out a new model using the retention ratio, share issues, and the valuation ratio.

Marris's views were entirely different. In Marris (1972), as in Marris (1964a: 309–310) and Marris (1991: 203–210), he maintains that the rate of profit is exogenously determined by 'the degree of oligopoly' and is a determinant of the growth rate rather than being determined by it. The behaviour of managers

⁹ For example, Marris (1964a: 277–288), Marris and Wood (1971: Appendix B), and Marris (1972: 330).

¹⁰ Marris (1964a: Chapter 8), Marris (1972: Appendices I and II), Marris and Mueller (1980: 45–46), and Marris (1998a: xix and Chapter 8).

also affects both growth and profits (since reported profits are net of diversification spending and thus understate the degree of market power: Marris and Mueller 1980: 56). Marris saw no basis for assuming differential saving rates and argued that Kaldor could not treat the retention ratio as exogenous, since it was set by managers to generate enough finance for their chosen growth rates. He also repeatedly criticised all the Cambridge macro theories of distribution for their failure to provide adequate microfoundations.¹¹

Although he believed that managerial capitalism resulted in faster growth, Marris was far more cautious about its broader normative implications. He was doubtful about the effects on economic welfare of diversification and other means of demand creation, which are central to his theory of the firm. Useful new products benefit consumers, but other ways of creating demand may be wasteful or harmful: ‘The possibility thus arises that the total cost of investments in nonprice competition exceed the benefits’ (ibid.: 57). Marris had even more doubts about the social and political merits of managerial capitalism, ending a chapter on the institutional needs of a better society with the words, ‘Increasingly, I think, the days of the stockholder corporation must be numbered’ (Marris 1974: 399).

In his 1998 book, looking back over 35 years, Marris recognises that economics had moved on. Building on the insights of Coase, of which he had initially been unaware, the existence of large firms can now be theorised in terms of their ability to reduce the high costs of transactions among small firms—though Marris found this explanation more plausible for vertically than for horizontally integrated firms (Marris 1998a: xv). The separation of ownership from control can now also be theorised as just one example of the difficulties that arise when principals have to act through agents.

Marris portrays his work as contributing to one of three strands—‘growth-orientated’—of managerial theories of the firm. The other strands are ‘discretionary’ theories, exemplified by Baumol and Oliver Williamson, in which managers use their freedom of manoeuvre to feather their own nests, and ‘bureaucratic’ theories, exemplified by Mosen and Downs, in which managerial control just makes large firms inefficient. Within the growth-orientated strand, started by Penrose, Marris stresses two contributions by Mueller, of which one was to develop the theory of takeovers from the perspective of the acquiring (rather than, as in Marris’s model, the acquired) firm.

¹¹ Agreeing with Marris about the lack of microfoundations, the present author tried to provide them in Wood (1975), but in the opinion of Marris (1991: 205) ‘misguidedly’. Marris’s criticism stemmed in part from disagreement about financial variables (see fn. 7 above), and in part from the absence from Wood (ibid.) of any formal model of price determination, to which the present author’s response is that his theory of profits sought to explain reported profits, not basic profits.

Mueller's other contribution was a life cycle interpretation of firm growth. In a firm that is 'young', in the sense of having found or created an unusually profitable set of new markets, there may be no trade-off between faster growth and a higher valuation ratio—over any relevant range, the two variables move together. Only when a firm 'ages', after saturating its initial markets, does a conflict arise between the desire of managers for faster growth and the desire of shareholders for higher valuation.

Marris recognised that there were prominent real examples of 'young' and 'dynamic neoclassical' firms, usually owner controlled, in which there is no trade-off between growth and valuation and sometimes stellar performance. He also acknowledged the relevance of the discretionary and bureaucratic theories—there are sleepy managerial firms as well as the dynamic growth-seeking ones on which he focused. He continued to believe, however, that dynamic managerial firms had been the most common type of business corporation during the twentieth century, and he argued that empirical studies strongly support managerial theories (Marris 1998a: 117–119; see also Hughes 1987).

In his 1998 book, Marris also recognises that the world had changed in three notable respects. One is that the share of managerial remuneration in the form of stock options and bonuses has increased, partly with the aim of aligning the interests of managers more closely with those of shareholders. Marris, however, was sceptical about stock options: as he put it, noting the greatly increased pay of top managers, 'once there is separation of ownership from management, the search for a perfect control contract between shareholders and managers is...liable to be perverted' (Marris 1998a: 40).

A change that he acknowledged may have made more of a difference was the greater ease of takeovers – more frequent mergers, more takeover raiders and the use of junk bonds as a financing vehicle – tending to push the choices of managers closer to the peaks of their valuation-growth frontiers. But he did not accept that this change had turned most corporations into valuation maximisers: the majority of raiders were other managerial firms, seeking to get bigger, a hypothesis strongly supported by evidence that, in most cases, takeovers make the shareholders of the acquiring firm worse off.

The third change was the information technology (IT) revolution. Marris noted that many IT firms were young or of the dynamic neoclassical type. Many IT firms also had little physical capital and incentivised their employees with allocations of shares rather than high salaries. In addition, Marris noted that IT itself had altered key determinants of the advantages and disadvantages of firms being large rather than small, although he was uncertain about the direction of the net effect. In so far as he believed that firms in the twenty-first century might behave differently from those in the twentieth century, it is IT that he saw as the most likely cause.

At the time of writing this chapter, it is just over half a century since Marris's landmark book appeared. His work precipitated a major academic debate that drew in Arrow, Solow, and other leading mainstream economists and became known to the public. The book also continues to be cited—about 75 citations per year since 2000.¹² Few of the recent citations, however, are in mainstream economics publications, tending to confirm Marris's own 1998 assessment of the long-term impact of his work.¹³ Though 'bloody but unbowed' (ibid.: xvi), he felt that his effort to divert economic theorising from the sizes of firms to their rates of growth had been 'without great influence' (ibid.: 113). It is to be hoped that his original and important work in this area will be rediscovered.

4 *Reconstructing Keynesian Economics with Imperfect Competition*

Marris's motives for writing his 1991 book stemmed ultimately from his Cambridge education in the 1940s, which made him a lifelong advocate of Keynesian theory as a basis for policies to improve the world. He had met Keynes, been taught by Kahn, and knew Joan Robinson. The subsequent attacks on Keynesian macroeconomics—as well as some of the defences—seemed to him misguided. In the early 1980s, he had started to write, with his then colleague Dennis Snower, 'a world-shaking new textbook of macroeconomics with explicit microfoundations' (Marris 1991: xvi). The collapse of that project led him to write his own book.

The central argument of the book is that Keynesian macro theory makes sense only when combined with the theory of imperfect competition. Only if firms set their own prices, rather than being price-takers in perfectly competitive markets, will the main effect of a fall in aggregate demand be to reduce total output and employment, which was Keynes's key assertion, rather than to lower the general price level. Yet imperfect competition is completely absent from *The General Theory*, which is puzzling, since Kahn had played key roles in the development of both this book and Robinson's book on imperfect competition. What kept the strands apart, Marris argues, were tensions in the triangle of personal relationships between Kahn, Keynes, and Robinson.

¹²These citation data are from Google Scholar and refer to the 1964 book rather than the 1998 revision, which has only 51 citations in total.

¹³In none of the volumes of the North-Holland *Handbook of Industrial Organisation* does 'managerial capitalism' appear in the subject index. In the 1989 volume I, Chapter 2 refers to 'the older literature on managerial theories of the firm', but without mentioning Marris.

Marris reviews the passages in *The General Theory* on microeconomic mechanisms, which he thinks were hastily inserted at a late stage and should not be taken seriously (ibid.: 180). These passages mainly imply that the immediate impact of a fall in aggregate demand is to lower prices, as in perfectly competitive markets, which leads firms to cut output. This mechanism is guaranteed to work only if nominal wages are fixed, which Keynes denied (Marris 1998b: 228), and if wages are flexible it works only on fragile assumptions (Marris 1999b: 25). Perfectly competitive markets are also rare in the sort of modern economy that Keynes had in mind.

The type of market structure described by Robinson and Chamberlin, which Marris eventually came to call ‘imperfect polipoly’, provides a simple alternative mechanism that is both more realistic and technically more robust. A fall in aggregate demand shifts each individual firm’s demand curve inwards, inducing it to cut its output. The firm will not alter its price, which is set to maximise profits as a mark-up on marginal cost, unless there is a change in the elasticity of demand for its product (which there is no obvious reason to expect) or its marginal costs change (which might happen later if nominal wages fell).¹⁴ Had the macro model of *The General Theory* been connected to this micro model of imperfect competition (a possible chapter structure is suggested in Marris 1997), Keynes’s book would have been far more internally consistent.

Marris explains in detail how this connection failed to be made. In a 1929 dissertation that he did not publish for 60 years, Kahn created the theory of imperfect competition before working closely with Robinson on her book (Marris 1992). Kahn was also the creator of the multiplier, and he worked closely with Keynes on *The General Theory*, providing extensive comments and possibly doing some drafting. But Kahn was not ‘prone to meta-theorising’ (Marris 1991: 183). Moreover, Keynes and Robinson did not get on, as Marris observed (ibid.: 187), and were competing for Kahn’s loyalty and affection.¹⁵ Keynes had told Ohlin that he could not ‘see how on earth imperfect competition comes in’ (Keynes quoted in ibid.: 185, fn. 1). The person who did see immediately how it came in was Kalecki, in a 1936 review of *The General Theory* in Polish, but he never followed up, possibly for ideological reasons (ibid.: 188–197). Marris’s book includes many other perceptive memories of Cambridge economists and their relationships.

¹⁴ This mechanism differs from ‘price stickiness’ (mentioned by Keynes at one point), which implies that firms want to reduce prices but for some reason cannot do so (Marris 1991: 229–230).

¹⁵ The book’s public disclosure of the personal relationship between Kahn and Robinson upset people in Cambridge, where it was common knowledge, partly because Austin Robinson, Joan’s husband, was still alive.

One chapter of Marris (1991) is a comprehensive and valuable review of alternative ways of modelling imperfect competition, divided into three parts. The first considers alternative approaches to ‘structure’—are there assumed to be distinct industries, and within industries are goods homogeneous or differentiated? The second part examines alternative formalisations of the competitive process based on conjectures about the reactions of rivals or on game theory (as a deeper rationalisation of conjectures). The first two parts are summarised in a decision tree for choice of model (*ibid.*: 152), based on observed circumstances. The third part compares assumptions about market entry for small groups and large groups of firms. A key conclusion is that other models of imperfect competition provide basically the same underpinning as imperfect polipoly for Keynesian economics, though they are more complicated.

The book’s exposition of its reconstruction of Keynesian economics is mainly in the form of a simulation model—the subtitle of the book is ‘a desk-top simulation’ and the picture on the front of the dust jacket is a screenshot of the results of a run of the model. The book contains many other screenshots, tracing the evolution of a key set of variables through time in response to specified shocks, much as in the output of many later dynamic stochastic general equilibrium (DSGE) models. Simulation modelling was not new, but this book was one of the first to come with a disk containing the model, for which Marris had spent two years writing the code (on an IBM AT), and with straightforward instructions to the reader about how to run the model on a personal computer. Variation in any or all of 18 key parameters permitted a wide range of experiments.

The economic content and workings of the model are explained in two chapters of the book, first in general terms and then in technical terms. Each firm in the economy is modelled separately and in detail. Firms take turns in making price and investment decisions on the basis of adaptive expectations of demand with a view to maximising long-term profits (modified, if the model user chooses, by managerial preferences for greater size). After each such decision, all firms revise their outputs in light of levels of unsold stocks and unfilled orders, and aggregate variables are recalculated.

In contrast to the treatment of firms, other aspects of the model are highly simplified, partly because of technical constraints on programming or computer memory. There is an aggregated household sector that earns income, buys goods, saves and supplies labour, and government spending. But there are no taxes and no markets for money or financial assets. The economy is closed and there is no technical progress. Subject to these limitations, the uses of the model are illustrated in the final chapter of the book with a series of experiments that explore the effects of fiscal policy, the relationship between the real wage and the demand for labour, and stagfla-

tion. As a bonus for the reader, there is also a fascinating chapter on the brain, computers, and economics.

Overall, this is an astonishing book, packed with ideas, insights, and information. Its two main arguments—that Keynesian macro theory makes sense only when combined with imperfect competition, and that this combination was omitted from *The General Theory* for personal reasons—are highly convincing. But although it was reviewed, in some respects enthusiastically, in the *Economic Journal* and the *Journal of Economic Literature*, the book had no discernible influence on mainstream macroeconomics. A telling illustration mentioned by Marris (1998b) is that the book is not cited in a 1994 survey of imperfect competition and macroeconomics (Dixon and Rankin 1994), even though one of the authors of the survey had reviewed it a couple of years earlier.

The reasons for its neglect are partly substantive. As Marris knew from his review of recent literature (Marris 1991: 213–233), the microfoundations of macroeconomics had become a crowded field—the survey that omitted him contained about 90 references. More basically, his main technical contribution—adding monopolistic competition—was already a key element of New Keynesian (NK) models.

Marris was aware of NK models but rejected them because of what he saw as their wrong assumptions about the labour market. In his model the labour market does not clear, as assumed by US NK economists, and nominal wages are either exogenous or respond to excess demand in the labour market. Nor in his model do real wages vary with employment on NK lines. However, European economists were modifying the NK model with a treatment of the labour market that was closer to Marris's thinking. The book's references include Layard et al. (1991), the leading contribution of this sort, but it had appeared too late to be discussed in the text, and Marris never identified himself as a European NK.¹⁶

Another important reason for the book's lack of impact is that its exposition was tied to a simulation model. As a result, some important variables and relationships had to be omitted for technical reasons. The model also rapidly became obsolete in terms of both software and hardware. Moreover, as Marris recognised in a review of the pros and cons of computer simulation (Marris 1991: 107–113), it is easier to communicate in the language of mathematics than in the language of simulation. To explain his model, he could not use either algebra or programming code and had to make up his own 'local notation' (ibid.: 234). All this Marris knew: that he nonetheless pursued

¹⁶In Marris (1998b), he attributes neglect of the book to errors in mainstream thinking. Marris (1997) reiterates its theoretical argument, although with no mention of the simulation model, and Marris (1999b) summarises it as part of an endorsement of the continuing relevance of Keynesian economics.

the simulation approach is probably mainly because he ‘fell in love with the microcomputer’ (ibid.: 2).

A further shortcoming of simulation models, which Marris does not recognise in his review, is that despite their seemingly objective technical nature, each model is just an extension of the brain of its author. The variables that are included and excluded and the causal links reflect what the author believes to be important or unimportant, which, apart from a limited range of parameter choices, the user must take or leave unless she wants to rewrite the code. As the reviewers pointed out, Marris’s model contained his own view of how the economy works. The qualitative results of his experiments, though not all predictable, arose largely by construction.

As a technical contribution to macroeconomics, Marris’s book alas sank without trace, and there is no possibility of it resurfacing. It remains of enduring relevance, though, as a contribution to the history of economic thought—flagging and explaining the puzzling omission of imperfect competition from *The General Theory*. Some of its thoughts on policy also remain relevant. Marris emphasised that macromanagement is difficult, especially in a democracy, and argued that its technical aspects should be ‘defended from the pollution of ideology’ (ibid.: 292), which in the UK soon came to pass with the creation of the Monetary Policy Committee and later the Office for Budget Responsibility. Although by then too ill to get involved, he must also have been pleased by the revival of Keynesian policies and thinking after the crash of 2008.

5 *How to Save the Underclass*

Marris’s last major book in 1996 combines his interests in macro policy, growth, and the brain with his enduring concern about human well-being. Like many other economists at the time, he worried about rising inequality in rich countries and especially about the increasingly dire plight of people at the bottom of the income distribution, particularly men, as a result of a drop in the demand for their labour. Like other economists, too, he attributes the different outcomes in the USA (low wages) and in Europe (unemployment) to different labour market and social security institutions. However, his assessment, both of the causes and of the appropriate policy responses to this problem, differs substantially from those of other economists.

Regarding causes, as signalled by his labelling of the afflicted group as an underclass, rather than the usual bland ‘unskilled workers’, a distinctive and controversial element of his argument is that, because access to education has become near universal, this fairly small group contains mainly those with low

intelligence quotients (IQs), largely inherited genetically (though worsened by early-life deprivation). Rich countries are now meritocracies, as forecast by the sociologist Michael Young, which Marris welcomed, so long as the meritocracy was 'moderate' (Marris 1996: 18). But meritocracy had been made 'severe' by the drop in demand for low-IQ labour, leaving this group unacceptably far behind.

His other distinctive argument about the causes is that the drop in demand for low-IQ labour was caused largely by slow aggregate growth as a result of misguided economic policies. As in his 1991 book, he traces the macro history back to shocks in the 1960s and 1970s—the Vietnam War, union activism, oil prices—to deal with whose inflationary results demand restrictions and more unemployment had been unavoidable. Slow growth was perpetuated after 1975, however, by irrational fears of inflation, excessive use of monetary policy (making interest rates high and unstable), and too many takeovers, stifling managerial desires for faster growth.

Marris rejects, on the basis of statistical analysis, the standard view that the slowdown in output growth was due to an exogenous slowdown in productivity growth. Part of the explanation was other countries catching up with the USA, but productivity growth then slowed in the USA, too. Marris argues that this later productivity slowdown was mainly caused by slower growth of total output—and was also partly a measurement error. In the USA especially, the fall in unskilled wages, combined with poor measures of output in services (Marris 1984: 54), created the illusion of slower productivity growth in services and thus in aggregate. In short, slower productivity growth did not help to solve the problem of an excess supply of labour, but merely concealed it.

He notes other causes of the worsened position of low-IQ men in the labour market. One is more trade with developing countries, aggravating deindustrialisation, though not in Marris's view by enough to explain the problem. The effects of IT he sees as being mainly at the other tail of the income distribution, where, as he grasped earlier than most economists, the causes of rising inequality are different. Marris also draws attention to the rise in the total labour supply caused by the increased participation of women. He explains why an overall deficiency of demand for labour, even without a systematic skill bias (as in the explanations of other economists), hurts especially those with low IQs. They are at the end of the hiring queue, so their initial lack of ability is compounded by lack of on-the-job training and experience, and in a malign cycle their deprivation in turn reduces the IQs of their children (Marris 1996: 13, 68).

Marris's views on appropriate policy responses to the problem of the underclass differ from those of other economists in ways that are corollaries of his different view of its causes. Unlike almost everyone else, he did not believe that the solution lies in better education. Early years programmes targeted on

children from deprived backgrounds can help (ibid.: 146). But the low educational qualifications of the underclass are mainly due to their low levels of inherited ability, not to a lack of access to education.

What Marris advocates instead is acceleration of growth (ibid.: 207–208). Maintaining a high rate of growth should be the principal objective of economic policy makers, both nationally and internationally. Inflation rates of up to 4% per year should be accepted. Interest rates should be kept low and stable, with more use of fiscal than of monetary policy. Steps should be taken to make takeovers more difficult.

If severe meritocracy persists, Marris argues, either because policies for faster growth are not implemented or because his diagnosis is wrong, the only technically available solution to the problem of the underclass is to make the welfare state more generous (than it already is in Europe, and vastly more so than in the USA), financed by higher taxes on those with higher IQs. To take this approach would be a moral obligation and would increase economic welfare, but would be second best, because ‘it is difficult to redistribute income without affecting incentives’ (ibid.: 127). Marris also rightly doubted that it would ‘command voting majorities’ (ibid.: 209).

Marris’s concern for the poor extended worldwide. *Ending Poverty* (Marris 1999a) was the final output of his long involvement with international development. It emphasises the importance of good macro policies in developed countries and good management of the international financial system. Echoing Marris (1970), it advocates increased aid to promote education, industrialisation, and effective government, but also argues for higher taxes on the rich in developing countries, and for more willingness to intervene directly in countries where poor citizens are not represented in government.

6 Conclusion

Marris published a remarkable series of books and papers—remarkable not for their number, but for their originality, diversity, and impact. The originality is clear across the board, from his early research on multiple-shift work to his late analysis of the causes of persistent poverty in rich countries. Though by no means always right, he was not afraid of being wrong. The diversity is also self-evident. Marris was stimulated by new ideas, though when he had latched on to an idea he had the tenacity to pursue it for years or even decades until he felt he had taken it as far as he could.

The impact of Marris’s writing, though, was concentrated on one area—managerial capitalism. His 1964 book and its follow-ups were a tremendous success, generating an amount of professional interest of which most econo-

mists can only dream. But his research on shift work had less impact than he had initially hoped, while his book on imperfect competition and Keynesian macroeconomics totally failed as a contribution to economic analysis and is neglected even as a contribution to the history of thought. The emphasis on inherited low IQ in his book on saving the underclass made it unacceptable to most other economists.

Even in the area where Marris was world-famous, his name has almost disappeared from mainstream economics. In research, however, influence is more enduring than fame. Individual bricks in the tower of knowledge are hard to see, and the names of their makers have worn away, but they are still there. Marris's lifelong engagement with policy, inside and outside government, had an influence, too, though it is even harder to assess the impact of advice than of research. Academics also have an enduring influence through their students, and this was certainly true of Marris. He inspired and supported young economists—Faculty as well as students—throughout his career.

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Frank Hahn (1925–2013)

Robert M. Solow

1 Introduction

Start with the bare biographical facts, even though the gap between them and the living, thinking person was wider in Hahn's case than for many other academic economists. Frank H. Hahn was born in Berlin on 26 April 1925. The family moved to Prague in 1931 and then to England in 1938. Most of his education and all of his academic training in economics were in England, but no one would ever have mistaken him for your basic Brit. He served as a navigator in the Royal Air Force (RAF) during the Second World War, in the course of which he met and married Dorothy Salter in a courtship that might be described as whirlwind if that were not a gross understatement. Dorothy Hahn was herself a considerable economist and Fellow of Newnham College. It was an extraordinarily close and devoted marriage. Frank's health deteriorated in the late 1990s. Dorothy took care of him tirelessly and supportively until his death on 29 January 2013. In his last years, illness and, even more, frustration turned him away from active economics. His last publication is dated 2003.

Hahn graduated from the London School of Economics (LSE) in 1945, and took his PhD there in 1951. He was Lecturer and Reader at the University of Birmingham from 1948 to 1960, and Lecturer in Cambridge from 1960 to 1966. From 1967 to 1972, he was Professor of Economics at LSE, reunited

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there with W.M. (Terence) Gorman, a lifelong friend and influence. He then returned to Cambridge as Professor of Economics, and remained until he reached retirement age in 1992. From 1960, he was a Fellow of Churchill College, one of the founding Fellows in fact. From 1992 to 2000, he was Richard Goodwin's successor as Professor of Economics at the University of Siena. To complete the picture, he was Visiting Professor several times at Massachusetts Institute of Technology (MIT) and also at Berkeley, and for 20 years a regular member of the famous Stanford Summer Workshop in mathematical economics. A goodly number of honorary degrees came his way along with Fellowship of the British Academy, election as Foreign Associate of the US National Academy of Sciences, and honorary memberships in various other foreign academies. It was a career of honor and dignity, especially for an economist whose personal style was usually critical and anything but dignified.

2 Basic Themes

Frank Hahn's significance for Cambridge economics extends in an important way beyond his published works. I will begin with a compact survey of his contributions to economic theory in general, and then turn to his central role in, as I see it, saving Cambridge economics from itself.

There are a few fundamental themes that run through Hahn's body of work although, like any talented economist, he would be visited by a stray idea or provoked by something in the literature, and produce an article off the main track. The largest of those central themes is the formal theory of general equilibrium in a system of many decentralized markets. The great monument to this theme is *General Competitive Analysis* (1971, though no doubt years in the making) written with Kenneth Arrow. I will return soon to a closer look at the contents of this important book. It is interesting, however, that Hahn's first stab at general equilibrium theory came in 1952–1953 with a short article in the *Review of Economic Studies* called 'The General Equilibrium Theory of Money: A Comment' (Hahn 1952–1953). Monetary economics was another long thread running through Hahn's work, extending at least until he joined Benjamin Friedman in editing the *Handbook of Monetary Economics* (Friedman and Hahn 1990), to which he contributed the article on 'Liquidity' (Hahn 1990).

A third enduring interest was the broad question of the stability of equilibrium, more or less any kind of interesting and well-defined equilibrium. The main example, of course, is the stability of competitive general equilibrium as prices and quantities vary. Any serious approach to the issue requires a specification of

the model economy's behavior out of equilibrium, and this tends to take the analysis away from the tried and true. Many economists duck the issue, but Hahn did not, and in fact made important contributions to be discussed later. He also wrote extensively and originally on the stability of growth models, but here the question is usually about the behavior and possible divergence of equilibrium paths. To take another example, he and I together once explored an analogous but unrelated matter, mentioned but not analyzed by Keynes: whether perfect flexibility of wages might lead to 'equilibrium' paths that could fairly be described as pathological. (The answer was Yes.)

As significant examples of Hahn's interests away from his main preoccupations, I will just mention two of his occasional topics. His 1951 PhD thesis at LSE, *The Share of Wages in the National Income*, was not published until 1972 (Hahn 1972a), though a 1951 article with the same title was among his first publications. The thesis was obviously much influenced by Nicholas Kaldor, his adviser; it makes use of the systematic difference between the marginal propensities to save from wage incomes and from profits (the remainder). But Hahn's use of the assumption is more subtle (and both more Keynesian and more general equilibrium in character) than Kaldor's. It is embedded in a more complete model of the economy, so that the level of output and the functional distribution of income are simultaneously determined. Two early papers, 'A Note on Profit and Uncertainty' (Hahn 1947) and 'Uncertainty and the Cobweb' (Hahn 1955), which he thought to be one of his more important efforts, signal Hahn's ongoing interest in questions related to uncertainty and expectations. He wrote others along the way, up to 'Notes on Sequence Economies, Transaction Costs and Uncertainty' (Arrow and Hahn 1999). But he never tried to develop a systematic treatment of uncertainty and expectations, and was highly critical of modern attempts to do so through the concept of 'rational expectations'. The examples in this paragraph do not come close to exhausting the range of Hahn's theoretical interests. What follows will be limited to the main themes of general equilibrium, money, and stability, which are closely enough related to be thought of as aspects of a single analytical project.

General Competitive Analysis is a difficult book, more difficult in its way than Gerard Debreu's loftier *Theory of Value* (Debreu 1959), to which it pays tribute. Arrow–Hahn's mathematics is much nitty-grittier than Debreu's more abstract treatment, but this is because Arrow and Hahn are far more concerned to relate the mathematical arguments, even if only sketchily, to economic intuition. In the Preface, they face the question why anyone should be interested in so abstract and complicated a construction. They start with the conventional answer: Adam Smith had famously claimed that a decentralized market

economy, motivated only by personal self-interest and guided by price signals, would lead not to chaos, as one might expect, but to a coherent outcome and, even more surprisingly, an outcome with many desirable properties. Smith's proposition has become an article of faith for many untrained people. 'The proposition having been put forward and very seriously entertained, it is important to know not only whether it *is* true, but also whether it *could be* true' (Arrow and Hahn 1971: vii; italics in original). In other words, they go on, one needs to know if Smithian optimism will be overturned if there is imperfect rather than perfect competition, or if there are external economies, or apparent irrationalities like judging quality by price, or if some markets are missing, or in a monetary economy. 'In attempting to answer the question "Could it be true?", we learn a good deal about why it might not be true' (ibid.). In later years, Hahn would often say that the main point of a book like that one is to show just how stringent the conditions are under which Smithian optimism has any claim to be believed.

Approximately the first third of *General Competitive Analysis* is given over to laying out in great detail the standard model of competitive equilibrium under favorable assumptions on consumer preferences and production possibilities, and then proving that this model has an equilibrium price vector (whose positive components correspond to goods with zero excess supply and goods with positive excess supply have prices equal to zero). In this treatment, commodities can be defined to have specific temporal and spatial dimensions. The treatment of uncertainty that more or less dissolves it by postulating the presence of a full set of state-contingent commodities is introduced but not explored in any detail. The next chapter (6) takes up some of the extensions or alternatives mentioned earlier: that prices might enter utility functions, that there are external economies or diseconomies in consumption or production, and that some markets are characterized by monopolistic competition. In each case, it is shown that, with appropriate modifications, an equilibrium price vector exists. In addition, there is an extended treatment of a two-period version of the sort of 'temporary' equilibrium introduced by John Hicks in *Value and Capital* (Hicks 1939): there are no futures markets, but there are certain storage possibilities. Once again, a short-period or temporary equilibrium is shown to exist. There is, however, no discussion of a monetary economy here; it is postponed to a later chapter on Keynesian economics.

The remainder of the book is taken up with special topics. There is a brief treatment of the case that some preferences are not convex (indifference surfaces have patches with the 'wrong' curvature). Chapter 8 on the core of a market economy is aimed at stating conditions under which, in a 'large'

economy, an allocation in the core (an unblocked allocation) is approximately a competitive equilibrium. A longer chapter on possible uniqueness of competitive equilibrium is a change of pace, the focus falling on cases where all commodities are gross substitutes or almost effectively so. Otherwise, uniqueness is not to be expected at this level of generality. A chapter on ‘comparing equilibria’ (what most economists, following Samuelson, call ‘comparative statics’) concludes that, with only the standard assumptions, the theory of general equilibrium does not provide enough information to calculate how equilibrium prices and quantities respond to parametric changes. Since discovering at least the sign and perhaps the magnitudes of those responses is more or less the bread and butter of economics, the implication is that choice of special assumptions is a critical matter.

There follow three chapters on the stability of general equilibrium. The first two are fairly conventional: they accept the admittedly silly hypothesis that there is no trading out of equilibrium and explore the role of Walras’s ‘auctioneer’, especially with universal gross substitutability. It was already known from Herbert Scarf’s famous 1960 example that global instability cannot be excluded. The last of the three chapters allows trading out of equilibrium in a pure exchange economy, making use of earlier work by Hahn himself and Takashi Negishi. I will pass over it because, stability having been one of his major interests, I will soon describe Hahn’s special contributions to this subject.

The last chapter of the text, on ‘The Keynesian Model’, is unusual for a book on general equilibrium theory. Perhaps its presence reflects the fact that Hahn was already associated with the ins and outs of Cambridge economics, but perhaps not: both Arrow and Hahn were well aware of what was going on in the broader field of economics, well beyond the confines of formal general equilibrium theory. Nevertheless, the content of the chapter is quite precisely concentrated on recognizably general equilibrium issues.

First, they identify *The General Theory* as being concerned with short-period or temporary equilibrium, a topic already discussed in the form Hicks had given it. There are, however, at least two important differences: Keynes’s agents, unlike those treated earlier, enter the current short period with debts and perhaps other obligations left over from earlier periods; and the context in which they act is an explicitly monetary economy. The main concern of the chapter is to investigate whether earlier theorems on the existence of a temporary equilibrium can survive these extensions. The two main problems are to make allowance for bankruptcy possibilities introduced by the presence of previously incurred obligations, and to find reasonable assumptions that will make the price of money positive. On the whole these problems can be solved, and a temporary equilibrium shown to exist.

Arrow and Hahn understand that this is not what ‘Keynesian economics’ is meant to be about; they think of this chapter as merely laying some groundwork. They mention, but do not seriously pursue, some more typical questions concerned with target demand and effective demand, with the formation and function of expectations, and with the uses of wage flexibility. I will come back to the last of these.

Two of the three chapters about the stability of equilibrium are more or less in the standard Walrasian tradition: all trading is suspended while an ‘auctioneer’ tries to find an equilibrium price vector. They follow the formalization of Samuelson: each price moves instantaneously, increasing or decreasing as excess demand in its market is positive or negative, each excess demand taken as a function of all prices. Everyone had always realized that this was not how reality could work; since the auctioneer was ‘fictitious’, so was the whole process, and since trading takes place all the time, there must be trading at nonequilibrium prices.

The third of the three chapters, allowing for trading out of equilibrium but limited to a pure exchange economy, reflects important work in the 1960s by Hahn (1961, 1962), Negishi (1961), and the two together (Hahn and Negishi 1962). This line of work, though not large in volume, was an important advance in the theory of stability of general equilibrium, and counts as one of Hahn’s lasting contributions. The basic idea of the Hahn Process—as it was dubbed by Negishi—is straightforward economics. Trade takes place out of equilibrium, but it is assumed that markets are sufficiently orderly so that at any time, in any market, there may be excess suppliers (those who would like to sell but cannot find buyers at current prices) or excess demanders (just the opposite), but not both. Markets may not clear, but they are good enough not to have at the same time unsatisfied buyers and unsatisfied sellers.

If prices behave in the manner described by Samuelson, excess suppliers will see the prices of the things they want to sell falling, and vice versa for excess demanders. With some further simplifications and extensions, this is enough to create a general tendency for traders’ utilities to be falling, and this aggregative fact can be used to provide a proof that prices converge to an equilibrium. The story can be extended to include firms and production. But it is still unsatisfactory, especially in its account of—or its failure to provide an account of—the formation of expectations, and their consequences. Any sort of general theory of stability that includes a useful treatment of speculation would be a remarkable achievement. It may be too much to hope for. Where the theory stands now owes substantially to Hahn’s work. His later views are summed up in the chapter on ‘Stability’ he contributed to the *Handbook of Mathematical Economics, Volume II* (Hahn 1982a).

Beginning immediately after the publication of *General Competitive Analysis* and continuing until 1999, Hahn was sporadically engaged in following up another fault line in the classical theory of general equilibrium: that connected with transaction costs, missing markets, and what he called sequence economies. This train of thought began with ‘Equilibrium with Transaction Costs’ (Hahn 1971), even as *General Competitive Analysis* was being published, and continued with ‘On Transaction Costs, Inessential Sequence Economies and Money’ (Hahn 1973a), and ending only with ‘Notes on Sequence Economies, Transaction Costs and Uncertainty’ (Arrow and Hahn 1999). Evidently the notion that Arrow and Hahn were bemused Walrasians is not serious.

The 1973 paper works by comparing two almost-identical abstract economies: in both of them, goods are distinguished both by their physical qualities and their delivery date (one of a finite number of dates into the future). In the Debreu-type economy, all those markets are open at the beginning, a full set of equilibrium prices is somehow found, and households and firms make their purchase and sale decisions over all goods and all times. Households are constrained only by one intertemporal budget-balance condition, but there are vaguely formulated transaction costs. No further transactions need occur after the initial moment; future deliveries have been specified and payment is feasible and confidently expected. In the sequence economy, transactions occur period by period (though plans extend further) and households balance their budgets only period by period. Prices for a specific good may differ according to the date of the transaction. (Why should things happen this way? Perhaps because the claim of intertemporal budget feasibility over long intervals may not be credible to counterparties.) These assumptions entail that there is a meaningful sequential structure to household behavior. Hahn’s mature take on these questions can be found in a 1993 overview entitled ‘Sequence Economies and Incomplete Markets’ (Hahn 1993). Much the best criticism of general equilibrium theory comes from the best general equilibrium theorists.

With so abstract a set up, only abstract questions can be discussed. The paper is given over mainly to issues of existence of equilibrium, and whether an equilibrium of the Debreu economy is also an equilibrium of the sequence economy. In that case the latter equilibrium is, of course, Pareto efficient. If there are equilibria of the sequence economy that are not Debreu-equilibria, they are not efficient. An inessential sequence economy is one in which this does not occur. The paper introduces money to the sequence economy in a fairly general way, and then shows that inefficient equilibria are still possible. It is pretty clear that Hahn thinks of all this apparatus as merely laying the groundwork for a truly monetary version of general equilibrium theory, not the thing itself.

Another example of Hahn's intention to get beyond simple Walras–Debreu assumptions without giving up rigor is 'On Non-Walrasian Equilibria' (Hahn 1978). It is interesting that he describes the particular motivation behind this paper as a wish to make precise sense of the notion that 'the division of labor depends on the extent of the market' (*ibid.*: 1). (The problem is that 'the extent of the market' is not well defined in the Walrasian setting.) But he also mentions the need to provide a general equilibrium setting for Keynesian economics.

In fact, the paper concentrates on the second of these objectives. It considers two ways of getting at the idea of Keynesian equilibria. The first is the approach (Dreze and Malinvaud–Younes) that holds prices fixed at non-market-clearing values in the short run and looks at possible outcomes when one side of the market is in some way rationed. Here, Hahn discusses mainly the existence of fixed-price equilibria that are orderly in the sense that only one side of any market has to be rationed. Such equilibria depend on the particular rationing scheme adopted for markets with either excess demand or excess supply. This rationing scheme is not a matter of individual choice.

Hahn prefers a different version of Keynesian underpinnings in which agents may, under some circumstances, have and understand that they have some market power: the constraint is not that prices are fixed, but that changing a price changes the amount that can be sold. Negishi (1960) had studied general equilibrium models with imperfect competition. Hahn's angle is different: he wants to show that an economy that has a Walrasian equilibrium can also have non-Walrasian equilibria characterized by sellers' conjectures about the demand curves they face. This is not really a satisfactory solution to the problem of finding a microeconomic basis for Keynesian economics because the model does not allow unconstrained agents (e.g. employers) to use to their own advantage the knowledge that counterparties (e.g. workers) are constrained. But it leads to an interesting discussion of the kinds of conjectures that are reasonable given the objective circumstances.

I will mention only two other research trails pursued by Frank Hahn, chosen because they intersect, one way or another, with my own. The first has to do with the theory of economic growth. The long survey article by Hahn and Matthews (1964) is everything that a survey article should be: comprehensive, clear, fair, and perceptive; it contains its own small but original contributions to the early stages of growth theory. Even earlier, with 'The Stability of Growth Equilibrium' (Hahn 1960), Hahn launched his own special project: he found that, under plausible assumptions, equilibrium paths in many good growth models need not converge to the standard sort of steady state. This work was continued in 'Equilibrium Dynamics with Heterogeneous Capital Goods' (Hahn 1966) and in 'On Some Equilibrium Paths' (Hahn 1973b).

The second is the book that he and I produced together: *A Critical Essay on Modern Macroeconomic Theory* (1995), written over a long period of years, one month per year, in Cambridge, Siena, and Helsinki. It was motivated by a shared distaste for the attempt of recent macroeconomic theory to lay ‘foundations’ by transferring the least credible assumptions of general equilibrium theory to a representative-agent context that only rendered them still less credible and even pernicious. We proceeded by following up a remark of Keynes: ‘[I]f competition between unemployed workers always led to a very great reduction of the money-wage, there would be a violent instability in the price-level’ (Keynes 1936 [1973]: 253) (and therefore in the real interest rate). In effect, we imposed perfect wage flexibility on a standard overlapping-generations model, so that the labor market always clears, and showed that many of the resulting trajectories could only be described as pathological. Unfortunately, we found no better way to demonstrate this conclusion than a prolonged application of brute force. It is no wonder that no one wanted to read this grim exercise. The fact that it was followed by some more palatable and, we thought, more acceptable macroeconomic modeling escaped notice. (For example, there is an attempt to explain wage rigidity as a response to piecemeal disturbance of traditional wage differentials, and there is an experiment with the incorporation of imperfect competition in a simple aggregative model.)

Quite a lot of Frank Hahn’s professional work stemmed from an acute aversion to nonsense. The nonsense in question could take several forms. One of them, certainly, was a lack of rigor in theory. This would account for his often-repeated remark that the point of *General Competitive Analysis* was to demonstrate, once and for all, how many very restrictive assumptions are required in order for classical general equilibrium theory to be true (including the First and Second Welfare Theorems). This may have been an afterthought: how could one know at the start of that massive enterprise where it would come out? The book may have begun with something more positive in mind. No matter: Hahn thought that the book, and several of his later articles as well, could stand as a warning against excessive claims that ‘the market’ could be relied upon to take care of this or that. The point is that he wanted such assertions to be rigorously refuted, not merely argued about.

Another form of nonsense consists of claims that have no real foundation in theory, but sound as if they did, especially with repetition. I think Hahn found this variety to be common in monetary and aggregative economics, where it might have the additional consequence of influencing policy. Something like that probably lay behind *Money and Inflation* (Hahn 1982b). Toward the end of his career, Hahn began to suspect that the sort of rigorous but realistically relevant theory that should be the economist’s goal might

simply be too hard to be done properly in the traditional way. He thought that perhaps computer simulation would now be the preferred way to accumulate analytical understanding. Those who knew him well will realize how paradoxical this tentative conclusion must have seemed, rather like a lifelong maker of fine wooden boats seeing the future in fiberglass.

3 Conclusion

The body of theoretical work that I have sketched, along with considerably more that has not been mentioned, is enough to make Frank Hahn the most significant Cambridge economist of the last third of the twentieth century. But his significance *for* Cambridge economics goes far beyond this. The situation I am about to describe is controversial, but that is exactly the point. I will pull no punches, but again that is the point: no punches were pulled.

In the 1960s and 1970s, Cambridge economics was rent by a deep factional dispute that, at the very least, bordered on the vicious. It was on its face an intellectual dispute, but it certainly had a substantial ideological coloring. The main protagonists were primarily senior members of the Faculty, of course, but among the stakes were such things as who gave the basic Part I lectures, appointments, and other matters of departmental control. The leaders on one side of the divide were Richard Kahn, Nicholas Kaldor, and Joan Robinson (in alphabetical order), but of course they were not alone. The 'other side' was more diffuse. Frank Hahn was certainly on the other side from the beginning, but he was not in Cambridge between 1967 and 1972; when he returned in that year, he was clearly the main opponent. James Meade was the other major figure but, as I shall point out in a moment, he was temperamentally unsuited to guerrilla warfare. My old teacher Richard Goodwin was in an embarrassing position: the nature of his work pretty clearly placed him with the Opposition, but he liked the 'revolutionary' air of the Kahn–Kaldor–Robinson faction. He tended to stay at home and paint. I was in Cambridge in 1963–1964; during that time (and before and after as well, mostly at a distance), I served as a convenient target. For better or worse, I did not mind.

All this was 50 years ago and (properly) seems faintly ridiculous now. The intensity has to be realized, however, if Hahn's role is to be understood. One example will suffice. There was a Monday 'secret seminar', apparently left over from Keynes's day. Its existence was not secret; it was 'secret' only in the sense that one had to be invited. Someone like Michael Farrell, an excellent Cambridge economist, who had no part in the ongoing controversy, was conspicuously not invited, presumably because he was politically conservative.

Meade was of course invited, but generally avoided the seminar: the verbal violence was more than his notion of reasonable argument would tolerate. During my year in Cambridge I did attend, and then so did Meade, because I was the preferred victim and I was not inclined to back off.

I have said nothing about the substance of controversy because this is not the place to rehearse obsolete history. The issue nominally at stake was the defense of the Keynesian tradition against, variously, ‘neoclassical’ or ‘bastard Keynesian’ subversion. Hahn thought that Keynesian economics was not really the issue at all (and in this his opinion coincided with mine). His view was that the other side simply did not understand what mainstream economics was about or what it claimed. In a review (Hahn 1972b) of Joan Robinson’s *Economic Heresies*, Hahn attributed to her a ‘lack of comprehension of what it is good modern theorists are saying’ (ibid.: 206).

On his return to Cambridge as Professor of Economics in 1972, Hahn came to the conclusion that the traditional ‘secret’ seminar was a waste of energy and, far worse, a discouragement to younger Cambridge economists. He organized an alternative economic theory seminar at Churchill College. This, and his leadership and encouragement, provided a rallying point for the younger generation who were otherwise disenfranchised. It is a splendid roster, including Anthony Atkinson, Christopher Bliss, Partha Dasgupta, Geoffrey Heal, James Mirrlees, and David Newbery, as well as a number of visitors from elsewhere.

The timing of Hahn’s return to Cambridge and the nature of the research program he stimulated are significant. It was immediately after the publication of Arrow–Hahn. His intention was to fill those gaps in general equilibrium theory that distanced it from macroeconomic reality. The most important of these, he thought, were the absence of a fundamental role for money, the lack of even a concept of unemployment, let alone an understanding of it, and, more generally, the need for a grounded theory of out-of-equilibrium behavior. It is plain, and was plain to him then, that this program could just as well be seen as trying to put Keynesian economics on a more complete and rigorous foundation. That way of looking at it was satisfactory to him, but he wanted both interpretations to be preserved.

To this end, he solicited a series of grants from the Social Science Research Council to support the research of a large number of graduate students and postdocs. In the nature of the case, what came out of this activity (and the ‘Quaker’ seminar that provided some glue) was not always exactly what its organizer had intended. But this enterprise produced some of the most interesting work by young Cambridge scholars during the 1970s and 1980s. Some of it appeared in a volume edited by Hahn: *The Economics of Missing Markets, Information, and Games* (Hahn 1989).

One never knows about alternative (counterfactual) histories. But I think it is a fair guess that if Frank Hahn had not returned from London when he did, with the moral stamina and the intellectual vigor to take a firm stand, Cambridge economics might have descended for years into parochial irrelevancy, altogether cut off from the evolution of economic thought around the world. That is not to say that what actually evolved, in Cambridge and elsewhere, is right and proper. Hahn certainly did not think so, ever.

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Wynne Godley (1926–2010)

Francis Cripps and Marc Lavoie

1 Introduction

The Hon. Wynne Alexander Hugh Godley was the grandson of Hugh Alexander Godley, Permanent Under-Secretary of State for India for 26 years (1883–1909) during which time India, with tariff protection, became one of the world's leading exporters (after the USA, UK, Germany, France, and the Netherlands).

Godley studied Philosophy, Politics and Economics (PPE) at Oxford (1947). He said he had two great teachers there, Isaiah Berlin and P.W.S. Andrews (Godley 2000). He first pursued a career as a professional oboe player, touring North America in the process, but stage fright induced him to revert to economics. He joined the Economic Section of the Treasury where, from 1956 to 1970, he developed methods for economic forecasting through continuous observation of monthly, quarterly, and annual statistics used to analyse the wide range of policy

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issues for which the Treasury was responsible.¹ As he explained much later in a 2008 interview recorded by Alan Macfarlane (2011), he learned how the economy worked without having much knowledge of economic theory. Indeed, because it was so antagonistic to what he learned about the British economy in the 1950s and 1960s, Godley could never make any sense of mainstream economics.

Given this background, it is hardly surprising that Godley saw government as the primary actor in macroeconomic systems. The anti-government tenor of mainstream economics did not impress him, and the dismal economic performance of the UK and many other countries in the 1980s was to him a clear demonstration of the futility of policies driven by simplistic theory rather than careful macroeconomic analysis. In his interview with Macfarlane, he repeated his belief that free markets alone will not provide solutions to problems confronting mankind and that the financial crisis then unfolding was a consequence of deregulation.

Godley brought his understanding and commitment to improve policy-making to Cambridge in 1970 at the suggestion of Nicholas Kaldor, becoming a Fellow of King's College and the Director of the Department of Applied Economics (DAE), a position that he held for nearly two decades. There he formed the Cambridge Economic Policy Group (CEPG) as a consortium of full-time researchers with differing areas of expertise. The Group started work analysing the alarming combination of 'stagflation' and 'deindustrialization' in the UK with unemployment passing one million by the end of 1970 while public and private sector wages and prices continued to rise rapidly. The DAE had around 30 full-time research staff and assistants, many of whom were members of either the CEPG or the parallel Cambridge Growth Project.

Professional knowledge of data sources and data processing techniques placed the department's staff in a different position from lecturers in the Faculty of Economics and Politics. Applied economics research as developed by the previous Directors, Richard Stone and Brian Reddaway, was a different discipline from the more abstract theoretical reasoning espoused by some academics. Nevertheless, the department's work attracted considerable interest from graduate students who came to Cambridge from all over the world.

As a civil servant, Godley had been frustrated by the deficiencies of modelling and the policy mistakes which followed. He believed empirical investigation of the workings of the national macroeconomy was an urgent need and a precondition for improved management of the economy. Although national

¹ Mata (2012) provides an account of the process by which Godley formed his basic understanding of 'how the economy works'.

accounts provide the framework for analysis and forecasting, prediction of the impact of government policies requires explicit assumptions about behavioural variables such as exports and imports, inflation, employment, consumer spending, and private investment. In the 1950s and 1960s, most of these variables were estimated by official committees, making the compilation of consistent forecasts slow and cumbersome, especially when iteration was required. Godley was proud of an import equation developed with J.R. Shepherd (Godley and Shepherd 1965) during his sabbatical at the National Institute of Economic and Social Research (NIESR) and believed it should be possible to use institutional and statistical evidence to pin down the size and dynamics of other important behavioural macroeconomic relationships in a similar fashion.

Godley's relationship with Kaldor started in the mid-1960s when Kaldor, acting as a senior adviser to the Chancellor, quickly discovered that Godley provided most of the Treasury's figuring on macroeconomic issues. Their partnership was particularly close in 1966–1967 when Godley worked out detailed plans and forecasts for the 1967 sterling devaluation.² A correlation between the balance of payments and the budget deficit used by Kaldor to provide a check on the appropriate budget balance following devaluation started a discussion that eventually led Godley to the key proposition about the private sector's financial balance being close to zero. After overcoming his initial surprise, Godley deduced that the Treasury's existing forecasting method might be defective as no attention had been paid to the financial balance implied by forecasts of corporate investment and profits.

The relationship between Godley and Kaldor continued through the 1970s as both were committed to provision of advice on policy issues formally or informally to the government and in Godley's case to parliamentary committees and the broader public. Although the advice that they gave sometimes differed,³ their analysis of predicaments confronting the UK was shared and both were shocked by the shift of politics and academia towards *laissez-faire* and liberalism.

Godley's interest in research on other aspects of government policy such as planning of public expenditure and finance of local government eventually gave way to an exclusive focus on macroeconomics (fiscal and monetary policy, incomes policy, exchange rate, and trade policy). Responding to the intensified academic criticism, even by fellow Keynesians, of what came to be dubbed by Richard Kahn and Michael Posner (1974a, b) as the *New*

² Godley had previously collaborated closely with Kaldor in preparing the implementation of the Selective Employment Tax and Regional Employment Premium.

³ See the account by Maloney (2012) of advice given by Kaldor and Godley to the Treasury in the 1970s.

Cambridge approach, Godley began to develop more formal and abstract presentations of his model, eventually concentrating on balance sheet and portfolio analysis but without ever giving up his interest in macroeconomic policy issues and prospects of the UK and, later, the USA.

Godley was dismayed by the termination of funding for the CEPG in 1983 and a subsequent enquiry into the DAE which eventually ended the department's independent status and merged it into the Faculty of Economics,⁴ but he was stubborn enough to argue back for the rest of his life. He deplored reduced-form models in which identities were dispensed with and accounting identities went unchecked; he went on writing columns or letters for newspapers on economic policy. In particular, Godley wrote a critique of the London School of Economics (LSE) price-setting labour demand model, showing that he could reproduce its neoclassical results (an inverse relationship between employment and real wages) by simulating an economy where prices were set as a markup over unit labour costs and where employment was unrelated to real wages by construction (Anyadike-Danes and Godley 1989); thus, as recalled by Felipe and McCombie (2013: 302), in his own way, Godley did participate in the Cambridge capital controversies. He also continued his work as a *conjoncturiste*. His insightful forecasting ability was finally recognized during his last year at Cambridge, in 1992–1993, when he was named one of the six wise men advising the Chancellor of the Exchequer.

During his 1991 sabbatical, Godley had been a visiting scholar at The Levy Economics Institute of Bard College, situated two hours north of New York City, the intellectual climate of which he found more congenial than that at Cambridge. On his retirement at Cambridge in 1993, when he was named Emeritus Professor in Applied Economics, Godley felt that he could still make many more contributions to economic policy and economic theory, and hence he happily took the Distinguished Scholar position that was being offered to him by Dimitri Papadimitriou, the President of the Levy Institute. Godley stayed in the USA until 2002. His main task there was to construct and update a stock–flow consistent (SFC) empirical model of the US economy and of the world economy (first devised by Cripps in 1979), as well as to produce strategic reports based on it. Thereafter, he returned to England as Visiting Senior Research Fellow at Cambridge, having been seduced by John Eatwell into joining the newly founded Cambridge Endowment for Research in Finance (CERF) at the Cambridge Judge Business School, where he was asked to replicate his

⁴ See Mata (2012: 24–25) and Godley (2000). Incidentally, James Tobin also saw his funding cut off in the USA in the very same year.

work on the US economy and adapt it to the UK. While in England, Godley continued to write strategic reports on the US economy for the Levy Institute. When his health and that of his wife, Kitty Epstein, started to deteriorate, Godley moved to live in Northern Ireland with his daughter Eve and his son-in-law.

While pursuing this empirical and applied work, Godley simultaneously chased another objective close to his heart, that of providing a method for constructing theoretical models that would fully integrate the real and the financial sides of the economy and that could be simulated. An earlier attempt had given rise to the book *Macroeconomics* (1983), written with his CEPG collaborator Francis Cripps. Drafts of the revised project were written in the 1990s, but it was only completed in 2006, five years after the start of a collaboration with Marc Lavoie which yielded *Monetary Economics* (Godley and Lavoie 2007a).

The next two sections of this chapter review Godley's career working in the Treasury and then at Cambridge as an applied economist concerned with live UK policy issues rather than macroeconomic theory. A description of the methodology that he followed and passed on to the CEPG is followed by an outline of the macroeconomic model that he developed with many collaborators, eventually summarized in a more abstract and theoretical form in the book on macroeconomics published in 1983. The last section describes some of his work on the US economy, but it mainly focuses on Godley's progression from macroeconomics to monetary economics and his search for what he most likely considered to be the Holy Grail of economic theory.

2 Methodology

Godley's explicit mission from the 1950s to the early 1980s was to inform UK government policy-making. As stated on the inside cover of the 1983 book on macroeconomics, the aim was 'to contribute a basis for the development of practical solutions to the problems of stagnation, unemployment and inflation' (Godley and Cripps 1983). After moving to the DAE, Godley decided to continue the approach he had followed at the Treasury, seeking to provide a clear understanding of the current situation and prospects of the UK economy and an assessment of the likely consequences of alternative courses of government action (Macfarlane 2011). Applied economics as practised by the CEPG in the 1970s was not a tool for testing economic theory.

The Group's work on macroeconomic policy was integrated by a computerized database and modelling system that provided systematic checks on whether past behaviour of the economy in each area was consistent with assumptions about 'how things work' as programmed into the computer and computed alternative scenarios for the future based on assumptions about special factors, ongoing trends, and policy responses. Godley checked all assumptions, results, implications, and interpretations with different members of the group. Each round of analysis was written up in an issue of the *Cambridge Economic Policy Review (CEPR)*, printed and published by the DAE, and later the Gower Press as a contribution to current UK policy debate.

The Group's approach may be familiar to applied economists of the older generation working in official institutions but is hardly documented in academic literature, although commentators have made reference to Godley poring over data and his ability to locate surprising figures.⁵ The main elements of the 1970s CEPG methodology were the following:⁶

- (i) study of available data, definitional (accounting) constraints on what it can show, how it has been put together, and likely sources of error;
- (ii) specification of behavioural norms based on institutional foundations and specialist knowledge in each field;
- (iii) examination of the available data as a test of behavioural assumptions and as a means of identification of special factors and longer-run trends;⁷
- (iv) identification of the main causes (trends, external factors, or policies) of current predicaments;
- (v) simulation of outcomes of different policy responses; and
- (vi) presentation of policy lessons learned from historical investigation and model simulations emphasizing the key trends and relationships involved.

This approach differed radically from the more abstract and scholastic methodologies espoused by academic economists who in most cases work alone or with one or two colleagues without the support of research teams. There is an inevitable clash between the two approaches as specific observation of

⁵ Mata (2012: 15–16). Cameron and Llewellyn (2010) wrote that Godley 'would take a vast spreadsheet of numbers, study them for sometimes hours at a time and then pronounce: "That figure is wrong," stabbing at it with an elegant oboist's finger. He was invariably found to be right. How did he know? The explanation, via his econometrician colleague Hashem Pesaran, was that he had what amounted to a full macroeconomic model in his head, which, by some sort of subconscious process, he computed'.

⁶ For a more complete exposition, see Cripps and Fetherston (1979).

⁷ As Kaldor often remarked, the evidence could contradict but not confirm. In this sense, the door was always open for improved hypotheses.

particular countries and historical moments often yields results that conflict with more general propositions advanced by theorists, and the evidence examined by applied economists is typically far more diverse in its nature, structure, and accuracy than assumed by academic econometricians.

The CEPG's work with national accounts and other official statistics was not a data-mining exercise in which regressions are run to determine 'correct' values of coefficients. Time series statistics, particularly at a highly aggregated level, could not provide a robust basis for specification of behavioural relationships, not only because the length of historical series available in the 1970s was often quite short but also because of features such as multicollinearity, measurement error, and limited effective degrees of variation which made estimated coefficients highly dependent on the precise way in which behavioural assumptions were specified.

Godley's view was that elasticities and other structural coefficients in the model's behavioural equations need to be broadly correct but cannot be precisely so and therefore that policy assessments need to be robust with respect to exact parameter values. Elasticities and time paths of behavioural responses were specified a priori on the basis of institutional considerations such as an understanding of the trade-offs involved and implications for longer-run stability, informed by more detailed, specialist empirical investigation. Structural coefficients could in some cases be inferred from stock–flow dynamics with the average lag of responses being determined by the stock–flow ratio (e.g. acquisition of financial assets, adjustment of inventories, and imports). Coefficients were also derived from historical studies. For example, investigation of wage bargaining and the movement of average earnings needed to consider evidence on many different aspects of labour cost, including wage rates, wage 'drift', hours worked (standard, short-time, and overtime), and so on, by industry and occupation. Surveys and censuses as well as time series provided the raw material from which behavioural conclusions could be drawn.

'Known' values of structural coefficients were used to adjust historical time series for cyclical effects and determine 'underlying trends'. This procedure was formalized in the computerized version of the CEPG model which updated estimates of trends and the size and pattern of departures from normal behaviour implied by given structural relationships, a procedure that differed from the 'scientific' method which requires models to be constructed on the basis of abstract assumptions rooted in an academic theoretical literature with coefficients and lag patterns estimated from time series data by econometric methods.⁸

⁸ Mata (2012). The relegation of time series econometrics to a subsidiary role caused the CEPG's model to be regarded with increasing mistrust by academic econometricians as well as a priori theorists.

Systematic re-estimation of structural coefficients at each forecasting round, practised by macro-modellers in the Klein tradition, was criticized by the CEPG as a less-than-transparent practice that blurs the distinction between model and data, potentially invalidating prior estimates and policy advice in an unclear manner and limiting lessons that might otherwise be learned from newly released data.

For Godley, each single observation was potentially valuable as a source of information. If the model and data did not agree, either may be wrong, and his first course of action was to solicit the advice of the statisticians who prepared the data. A good example was an abnormal level of imports of manufactures in the mid-1970s due to the arrival of rigs to drill North Sea oil. Kinks of this kind were usually familiar to government statisticians who had a good idea why they happened. Depending on the case, the explanation of an unexpected figure might be (i) identifiable and measurable 'special factors' known to the statisticians, (ii) estimation error (again known or surmised by the statisticians), or (iii) incomplete or faulty behavioural assumptions requiring a rethink of some aspects of the behavioural model. Ultimately, significant errors in the sense of discrepancies between the model's conditional (single-equation) prediction and actual values reported in official statistics had to be explained by specific historical circumstances. It was not good enough to classify errors as the outcome of unknown random shocks since they have important policy consequences. Only when there are plausible explanations for any large errors can we retain confidence in the model with which we interpret the past and examine current issues and policy choices.

Godley never trusted models as such and checked the output of various CEPG models very carefully, his standard challenge being: 'Are you prepared to bet either way?' It was not enough to be logically consistent. Researchers had to provide an assurance that, after considering all the evidence and as best as they could determine, their estimates were not biased in any particular direction.

Write-ups of a policy analysis could not be content with reporting simulation results. The main need was to explain why different outcomes may arise and why different policies will or will not have much impact on the outcome. Such explanations should be understandable in their own terms without reference to detailed model simulations. Thus, although model simulations are suggestive, helping analysts to rule out developments that may at first have seemed plausible to them and to assess the relative importance of different factors, their findings must be communicated in terms that not only are logically consistent but also make sense historically and institutionally. This requirement places a premium on simplicity of model structure. The more the behavioural detail, the more complex and in many cases less certain the inferences that can be drawn.

Godley's own preferred method for understanding the implications of a model was to focus on one or two key equations and simulate arithmetic examples with pencil and paper until he could internalize the implied dynamics and find a way to convey the main point to others.⁹ This approach and his emphasis on accounting constraints and the need to 'close' the model dated back to his time at the Treasury where national accounting identities provided a framework that he could use to check the consistency of forecasts of behavioural variables provided by official committees.

3 The Macroeconomic Policy Model

Godley brought a large part of what came to be the CEPG or 'New Cambridge' model of the UK economy in the 1970s to Cambridge from the Treasury. Key features of the model were set out formally in *Cambridge Economic Policy Review No. 1* (CEPG 1975: 85–91) and an article published in *Economica* (Cripps and Godley 1976). Extensions and refinements were written up in subsequent issues of the CEPR and the 1983 *Macroeconomics* book. Further development of financial aspects by Godley and others gave rise to the 2007 *Monetary Economics* book (Godley and Lavoie 2007a).

Price Setting, Inflation, and Real Income

A big issue of the 1960s and 1970s was the persistence of inflation while unemployment rose. Godley paid a lot of attention to the mechanics of price increases, in particular the degree to which they were demand-determined or cost-determined and the manner and timing of the passing on of costs into prices. He learned something about price formation from Andrews as a student at Oxford and made systematic investigations while at the NIESR in 1964. Further empirical research on pricing behaviour with Ken Coutts at Cambridge was eventually written up in a book (Coutts et al. 1978). The book was a theoretical and statistical development of Andrews's normal cost pricing hypothesis. The key idea was that firms set prices to maintain given margins over 'normal' unit costs, calculated on actual wages and input prices but assuming normal productivity. This hypothesis implies that the profit share will vary on account of cyclical fluctuations in productivity as employment adjusts less than fully to changes in output. It also implies that real wages are more or less given by normal productivity and profit markups irrespective of

⁹ Later, he used computer software (MODLER) in the same manner.

the rate of wage or price inflation. As in the case of trade, there were some important qualifications to do with timing (stock appreciation), world prices, and exchange rates (cost of imports and profitability of exports). In particular, in the analysis between historic unit costs and prices, lags were derived from inventory-sales ratios without using econometrics at all.

The CEPG developed an analysis of relationships between inflation and the level and distribution of real income in response to events in the early 1970s when the UK government guaranteed real earnings through an indexation scheme at a level that the economy could not deliver—the outcome being rapid escalation of inflation to reach a peak rate of more than 25% per annum in 1974. Godley understood that inflation neutrality could provide a valuable benchmark for understanding how inventories and work in progress should be financed and other components of national income such as interest and taxes should be adjusted in order to minimize impacts on real income and spending. The CEPG's model provided a relatively complete picture of income, expenditure, and financial balances measured in terms of a purchasing power standard. Relative price changes were an important part of the real economy story, but inflation in the sense of general changes in the price level was a distinct issue.

Although a lot of attention was paid to price adjustment and inflation accounting, Godley never had a strong theory of inflation as such. Inflation neutrality provided a behavioural benchmark but not a powerful predictor. Workers and employers may agree on adjustments intended to achieve or maintain target levels of real earnings, but there is no self-evident way of determining how fast target earnings should be expected to rise. Interest rates may (or may not) be adjusted to maintain normal real rates. In general, nominal adjustments cannot be fully effective in achieving real targets unless the level of real income determined by gross domestic product (GDP) (with adjustments for external income flows and the terms of trade) matches or exceeds the grand total of target real incomes. Inflation in the UK was widely regarded, not only by the CEPG, as resulting from the pursuit of improved levels of real earnings with money rates and earnings being increased regularly in a wage–price spiral. Incomes policies which sought to generate acceptable levels and relativities were not able to keep inflation at a low level.

Aggregate Demand, Productive Potential, and Full Employment

Godley's first lesson to the CEPG concerned the effects of fluctuations in aggregate demand relative to productive potential, for which he referred to Godley and Shepherd (1964). Potential output could be measured and

predicted by observing the past trend with allowance for demographic and participation trends (labour supply) and the trend increase of output per person employed (productivity) which was assumed to reflect longer-term processes of structural change in different production sectors. Employment relative to population would vary less than in proportion to aggregate demand relative to productive potential and unemployment would vary less than employment as part of any shortfall in employment was reflected in reduced labour force participation. On the basis of past experience, it was assumed that a rate of unemployment in the UK of 2–2.5% represented a normal or reasonable level that would allow sufficient flexibility in the labour market. These rule-of-thumb observations were supposed to be broadly consistent with Okun's findings for the USA. To maintain a high level of employment and low unemployment, the government would have to manage aggregate demand in line with productive potential defined relative to this unemployment target.

The External Trade Constraint

The second important set of behavioural relationships determined imports relative to domestic demand and exports relative to world demand as measured by estimates of world trade. For example, with world trade growing at an average rate of 9% per year and the UK share falling by 4% per year, UK exports would grow by around 5% per annum, and with import penetration rising 3% per annum relative to domestic demand, the latter would have to grow by not more than 2% per year to maintain a zero trade balance; and if productive potential were rising by 3% per year, then the UK faced a major medium-term problem. Failure to find a solution implied ongoing deindustrialization and rising unemployment.

Behind trends of exports and import penetration, there were cyclical aspects such as the fluctuation of imports with stock building (inventories) and of the terms of trade for primary commodities with the volume of world trade. This relationship offered some compensation to the UK as an exporter of industrial products and importer of food, raw materials, and, later, oil. Kaldor was very aware of the interdependence of world markets and stressed the need to counteract the Prebisch effect (declining terms of trade for primary products) by increasing the elasticity of world commodity markets which would help to sustain growth of world trade when increases in supply of raw materials run ahead of world demand.

Two more significant and longer-term issues were (i) changes in the commodity composition of UK imports as imports of manufactures started to

dominate imports of food and raw materials, pushing up the aggregate rate of import penetration without significant compensation on the export side; and (ii) the effect on imports and exports of real (inflation-adjusted) exchange rate changes on which Godley's and CEPG's views were fairly conventional.

Devaluation and Alternative Trade Policies

In the context of stagflation in the 1970s, both Godley and Kaldor became pessimistic about use of the exchange rate to correct the trend loss of export market shares and growth of imports of manufactures that made it impossible for the UK to combine a sustainable trade balance with full employment. Exchange rate devaluations would intensify inflationary pressure through their impact on import prices and would have only a temporary 'real' effect. While they may have believed with the government that wage moderation could eventually bring down inflation without the need for permanently high unemployment, they did not think the distributional impacts of a major real devaluation could be absorbed in the same way. This led them to examine alternatives such as various types of import controls, export taxes, or multiple exchange rates as possible methods for alleviating the balance-of-payments constraint on domestic economic expansion (Godley and May 1977; Cripps and Godley 1978; Maloney 2012). Godley's view was that import controls in countries lacking international competitiveness would not impede economic development; rather, by allowing governments to pursue expansionary fiscal policies without fearing rising current account deficits, it would instead raise world economic activity and world trade, while keeping fiscal deficits under control.

New Cambridge Propositions

New Cambridge advanced two key propositions that remain important but debatable today:

- (i) the government budget balance determines the current account balance; and
- (ii) the international competitiveness of an economy determines GDP.

Both propositions describe medium-term outcomes and may be reversed in the short term before multiplier/accelerator and asset adjustments work through.

The first proposition was based on the combination of an accounting identity and, as pointed out in the introduction, a specific empirical observation. The national accounting identity based on flows of funds—the fundamental

identity—simply says that the sum of three financial balances—domestic private net lending, domestic public net lending, and foreign net lending (the funds lent by foreigners, also equal to the current account deficit)—has to be equal to zero. With standard notations, and with *CAB* standing for the current account balance, this meant that $(S - I) + (T - G) + CAB = 0$. The empirical observation in the UK in the 1960s and early 1970s was that domestic private net lending ($S - I$, what Godley at the time called the net accumulation of financial assets of the private sector, including both firms and households) hardly changed in relation to GDP (or rather, at the time, gross national product). This meant that changes in government budget deficits and current account deficits were roughly of the same magnitude. It was felt at the time that the budget balance was more exogenous than the external balance since government expenditures could be controlled while exports depended on foreign demand, thus leading to the claim that the government budget balance determines the *CAB*. It has to be pointed out that in the early 2000s, Godley had reversed the main direction of causality. He then argued that the sum of the private and external balances was more likely to influence the budget balance (Godley and Izurieta 2004).

The second proposition—a Kaldorian one—was based on the assumption that current account deficits could not go on forever and hence that an economy lacking international competitiveness would eventually be forced to slow down, through either monetary or fiscal austerity policies, or both. Thus, international competitiveness, or what Godley and Cripps (1983: 296) called the trade performance ratio, that is, the ratio of exports to the propensity to import goods from abroad, was the long-term determinant of GDP—a proposition that can be attributed to Roy Harrod (1933). This proposition has given rise to a large theoretical and empirical literature, closely related to the export-led growth model and mainly developed by writers in the Kaldorian tradition. The proposition, instead of being stated in stationary terms, has been mainly presented in growth terms under the name of the balance-of-payments growth constraint, surveyed in detail by John McCombie and Anthony Thirlwall (1994). The first proposition has only survived in the form of the fundamental identity based on the three financial balances. As we shall see in the next section, the financial balances approach has been endorsed by a broad group of Post-Keynesians as well as several financial analysts. On the other hand, the stability of domestic net private lending turned out to be a temporary phenomenon, which could not justify the twin-deficit proposition of *New Cambridge*, later picked up by mainstream economists and the International Monetary Fund (IMF) to justify the imposition of austerity policies to developing countries.

Macroeconomics: The 1983 Book

In the early 1980s, Godley got the support of Frank Kermode, like himself Fellow of King's College and a well-known literary critic, to write a definitive book that would prove his case. He believed that once the need for consistent accounting is recognized, the scope of disagreement about what is or is not conceivable in the real world is greatly diminished. What remained to be done—modelling of behaviour such as fluctuations of exports, imports, inventories, output, and employment—was a more pragmatic observational matter. The disciplines set out in the book would leave no scope for the naive and highly theoretical thinking espoused by Friedman, Lucas, and others who had subverted mainstream academia and exerted an extremely damaging influence on government policy.

The book was faithful to the CEPG's computer model of the UK economy but was formulated in abstract algebraic terms. Godley wrote the main textual expositions and checked the presentation of equations carefully. In some places, the equations became somewhat tortuous, as Lavoie later observed, due to a possibly perverse aspiration to set out the model in a form that could readily be quantified for any particular country and time period using standard national accounts data.

One interpretation of Godley's theoretical work is that it is a quest for the Holy Grail of Keynesianism. Keynesians of all stripes had for a long time mentioned the need to integrate the real and the monetary sides of economics. Integration was all the talk, but for a long time, little seemed to be achieved. Stinted by the rise of monetarism in the 1970s, with the monetarists claiming that the Keynesians had no clue about the role played by financial aggregates, Kaldor was forced to sharpen his views on endogenous money while Godley wanted to discover how money got into the economy. The main purpose of the Godley and Cripps's 1983 book is to amalgamate the real and the financial sides, providing a theory of real output in a monetary economy. This is clearly stated in the book's introduction, where Godley and Cripps (1983: 17) claim that 'our present synthesis may be broadly characterized by saying that we make a "monetarist" financial system (based on the behaviour of stocks of money, financial assets and debts) drive a "Keynesian" flow system based on the response of expenditure to income', and it is reiterated in the Epilogue, when they claim 'to have provided a framework for an orderly analysis of whole economic systems evolving through time' (ibid.: 305). Indeed, in the very first sentence of the book, they point out that 'macroeconomics is the study of how whole economic systems function' (ibid.: 13).

Godley believed that Keynesian orthodoxy ‘did not properly incorporate money and other financial variables’ (ibid.: 15). Godley and Cripps and their colleagues ‘found quite early on that there was indeed something deficient in most macroeconomic models of the time’, including their own, ‘in that they tended to ignore constraints which adjustments of money and other financial assets impose on the economic system as a whole’ (ibid.: 16). Interestingly, Godley was aware of the work being carried out at about the same time by Tobin and his Yale colleagues, as well as by others such as Buiter, Christ, Ott and Ott, Turnovsky, and Blinder and Solow, who emphasized, as Godley and Cripps (ibid.: 18) did, that ‘money stocks and flows must satisfy accounting identities in individual budgets and in an economy as a whole’. Still, Godley thought that the analysis of the authors in this tradition was overly complicated, in particular because they assumed some given stock or growth rate of money, ‘leaving an endogenous rate of interest to reconcile’ this stock of money with the fiscal stance (Godley 1983: 137). Godley and Cripps (ibid.: 15) were also annoyed by several of the behavioural hypotheses found in the work of these more orthodox Keynesians, as they ‘could only give vague and complicated answers to simple questions like how money is created and what functions it fulfils’. The Cambridge authors thus wanted to start from scratch, with their own way of integrating the real and the financial sides, thus avoiding these ‘tormented replies’ (ibid.).

A review by Lester Thurow (1983) cast doubt on the assumption that different schools of thought could be persuaded on the basis of a formal model. His prognosis was certainly correct. The book failed to find its way onto the macroeconomics curriculum in UK universities, first because it looked overly original relative to existing standard macroeconomic texts, and second because the economics profession was taken by storm by the Lucas micro-foundations revolution and the rational expectations hypothesis, although the book did gradually build up a following among macroeconomists who could not accept the neo-classical consensus. Ultimately, Godley’s desire to present a definitive treatise based on consistent macroeconomic accounting gave rise, nearly 25 years later, to the *Monetary Economics* book (Godley and Lavoie 2007a), a volume that was reviewed quite favourably by Lance Taylor (2008: 639) who looked at it as ‘a major update’ of the previous synthesis found in *Macroeconomics*.

4 The Stock–Flow Consistent (SFC) Approach

Stock–flow accounting was not something that the CEPG pursued empirically in the 1970s. There were no time series on wealth and its breakdown by asset type in the UK. The concept of a stable ratio of private net financial

assets to income was more a reflection on the observation of behaviour of the flows (income and net acquisition of financial assets) than anything else.

Initially, Godley emphasized the importance of assessing the cumulative effects of flow imbalances as a check on the plausibility of stock-flow projections rather than treating asset decisions as the driver for saving, investment, and trade flows. While Godley had met James Tobin in Cambridge, his interest in balance sheets and Tobin's portfolio analysis developed later as a response to monetarism and as a necessary extension of the flow model to deal with academic criticism from the USA and later some groups in the UK.

Godley's efforts to truly integrate the real economy with the financial system gave rise to what is now called the Stock-Flow Consistent (SFC) approach, following the definition proposed in 2002 by Claudio Dos Santos in his PhD dissertation at the New School for Social Research on Tobin's and Godley's approaches. The SFC approach is perhaps a misnomer, as several other theories relate stocks and flows in a consistent way. What is the peculiarity of Godley's SFC approach, or of what could be called the *Post-Keynesian SFC approach*, is that its models truly integrate the real and the monetary sides. To get a gist of what this SFC approach implied for Godley and his collaborators, we should recall some of the other names which were suggested at various stages of its development, such as the *real stock-flow monetary model*, the *financial stock-flow coherent approach*, or the *sectorial stock-flow coherent approach*. These names all purported to emphasize that stock-flow consistency was not just limited to the link between real investment and tangible capital, but involved most importantly the interrelated financial flows and stocks of assets and liabilities of the main economic sectors of the economy.

Whereas the principle of stock-flow consistency and the idea that there should never be any black hole in the accounting were already present in the 1983 *Macroeconomics* book, Godley considered that a 1985 paper (Coutts et al. 1985) was his first comprehensive attempt at writing down consistent accounting in a whole economic system, and thus constituted the first step towards a proper SFC approach. The paper was an extension of the chapters of the Godley and Cripps book that had dealt with inflation accounting, and it defined the conditions for inflation-neutral effects on income distribution and aggregate demand—which was of great concern following several years of double-digit inflation in the UK. But it offered an additional innovation, as it included a stock matrix where all the financial assets must have a liability as an exact counterparty and where only tangible assets appear in the net wealth of a (closed) economy.

Godley kept working on this topic in the 1980s, with the intention of writing, in collaboration with Coutts, a complete monograph that would integrate both the real and the financial sectors. The intention is confirmed

in a paper published in 1993, where Godley announces that he is preparing a substantial monograph (again in collaboration with Coutts) and that there exists ‘a simulation model in which banks’ operations are fully articulated with income, expenditure and transfer flows together with asset demand functions’ (Godley 1993: 72). But no such simulations are shown in that paper.

Godley’s View of Credit and Banking

Godley (1996) is the paper that truly launched the SFC approach. The paper appeared as a Levy Economics Institute working paper where Godley had been working for three years. Papadimitriou had been getting a bit worried because until then Godley had produced hardly anything for the Institute. The reason was that he was setting up the empirical model of the US economy while simultaneously trying to write an academic paper that would show, with the help of simulations, how to synthesize a theory of credit creation with that of income determination. The paper was certainly a revelation to all those interested in monetary economics that came to read it.

Godley was most preoccupied with understanding the functioning of the economy as a whole. Godley (*ibid.*: 14) had been groping ‘to show how the whole system fits together and cast banks in a realistic role’. Godley was putting together a monetary flow analysis, linking monetary income and expenditure with a flow demand for credit, and a portfolio analysis, that explained the various demand functions for financial assets, including the demand for a stock of money. Godley’s 1996 paper reconciled an approach based on flows of credit with the more traditional Keynesian portfolio approach. He integrated the two views, showing formally, with a fully integrated model that could be simulated, how flows and stocks would gradually change in line with each other through time. Godley’s models could simultaneously determine the stocks of money or securities held by households and the flows of credit, investment, and income, as well as the stocks of private and public debts. The same integration had already been achieved in Godley and Cripps (1983), but the integration was only partial, the difference being that most of that book assumed that all financial assets held by households were retained in the form of bank deposits. In addition, Godley (1996) formalized the appearance and the treatment of capital gains.

As Godley points out on a number of occasions, he himself owed his formalization of portfolio choice and of the fully consistent transactions-flow matrices to Tobin. Godley was most particularly influenced and stimulated by his reading of the paper by Backus et al. (1980), as he writes in Godley

(1996: 5) and as he told us verbally several times. The discovery of the Backus et al. paper, with its large flow-of-funds matrix, was an eye-opener which helped Godley to move forward, by providing both a role for the banking sector and getting into sectoral flow-of-funds analysis, allowing him to build what he called a transactions-flow matrix, where all rows and columns had to sum to zero.

However, as explained in Godley and Lavoie (2007a: 493), despite their important similarities, there is a crucial difference in the works of Tobin and Godley devoted to the integration of the real and monetary sides. In Tobin, the focus is on one-period models, or on the dynamics of adjustment from an arbitrary distribution of assets to the desired portfolio composition, for a given income level. By contrast, in Godley and Cripps and in further works, Godley is preoccupied in describing a fully explicit traverse that has all the main stock and flow variables as endogenous variables. As he himself says, '[T]he present paper claims to have made...a rigorous synthesis of the theory of credit and money creation with that of income determination in the (Cambridge) Keynesian tradition' (Godley 1997: 48). Tobin never quite succeeds in doing so, thus not truly introducing (historical) time in his analysis, in contrast to the objective of the Godley and Cripps book.

Another major difference between Tobin and Godley is their views about the role of banks. Again, this is discussed in detail in Godley and Lavoie (2007a: 497–499). In most of Tobin's writings, banks are veils that provide households with a greater variety of asset choices. By contrast, in Godley's view, banks play a distinct and essential role, since bank credit allows the expansion of economic activity. Godley's banks are Kaldorian, responding to the financial needs of their creditworthy clients. This is linked to Godley's view of the production process, which is similar to that of the French and Italian monetary circuitists, in particular those of his long-time friend Augusto Graziani. Within this framework, bank loans act as a necessary buffer for the fluctuations in inventories. The link between inventories and bank loans is preserved all the way from Godley and Cripps (1983) to Godley and Lavoie (2007a).

Open Economies

A revised and slightly simplified version of the 1996 paper got published in the *Cambridge Journal of Economics* three years later (Godley 1999a). It is this publication that induced Lavoie to contact Godley at the end of 1999. Godley had produced a rough draft of the monograph that he had long intended to write, but it was only after having recruited Lavoie as a co-author

in 2001 that further progress was made. Godley and Lavoie shared the writing, with Godley providing most of the ideas and Lavoie most of the literature background. Godley built and ran the models, while Lavoie did the simulations and corresponding figures. This collaboration, except for two weeks spent together at the Levy Institute and then at King's College, Cambridge, was done mostly through long-distance phone calls and e-mail exchanges. It turned out to be highly successful, because the two authors shared very similar views on microeconomics and macroeconomics, as well as an identical understanding of the role of credit and money.

In 1999, Godley also produced another SFC model, this one dealing with an open economy (Godley 1999b). This was the first of a series of papers and book chapters that purported to analyse 'open economy macroeconomics using models of closed systems', as the title of the paper said, with all flows and all stocks fully accounted for wherever they arise. Godley's open-economy models reproduced the compensation thesis that Lavoie had put forward previously on the basis of endogenous money theory, that is, the claim that a country on a fixed exchange rate with balance-of-payment surpluses would not see its monetary base increase, in contrast to the standard result of the Mundell–Fleming model. Whereas international trade theory is usually based on a two-country and two-good framework, this is not the case in mainstream international macroeconomics, where the economy under study is presumed to be small and open with an exogenous 'rest of the world'. Godley produced instead models where feedback effects from one country to the other were taken into account, thus providing a whole set of real and financial interactions between countries. In these models, a vast array of variables (GDP; trade, capital, and *CABs*; public and foreign debt; foreign reserves or exchange rate; the stock of money; domestic, export, and import prices) become fully endogenous in both countries.

As argued by Jacques Mazier, a Professor at the University of Paris 13 and a specialist of open-economy macroeconomics, Godley's SFC approach and models are much superior to mainstream models—the general equilibrium representations of the world economy or the simplified portfolio model of exchange rate and portfolio account balances based on net assets. In both sets of mainstream models, output and government deficits are assumed to be given, an assumption that proved to be highly unrealistic during the recent global financial crisis; hence the whole adjustment process is assumed to fall on relative prices. Furthermore, the supply of assets is also assumed to be given, so that these models are devoid of a consistent analysis of the stock–flow financial dynamics. Thus, the integration between real and financial variables appears rather limited. By contrast, in Godley's open-economy SFC models, production is determined by aggregate demand.

An example of the usefulness of the SFC approach is the paper based on a three-country model which was published in January 2007 in the *Cambridge Journal of Economics* (Godley and Lavoie 2007b). The model consisted of two Eurozone countries associated with a single central bank (the European Central Bank—ECB) linked through a flexible exchange rate with a third country—the USA. The model showed that if a country within the Eurozone (say Germany) improved its trade performance vis-à-vis the USA, this would lead to a twin government budget and current account deficit in the other Eurozone country (say Italy), thus by no fault of its own. The model also showed that this situation would become unsustainable, with ever-rising deficits and interest rates, unless the ECB accepted to purchase the securities issued by the deficit country. An alternative closure of the model assumed that the deficit country would pursue fiscal austerity policies. The model showed that these policies would succeed in eliminating the twin deficits, but at the cost of a large recession for the deficit country, while the surplus country would not benefit any more from its improved trade performance.

Unsustainable Processes

The three-country model thus depicted fairly well what happened in the Eurozone after the sub-prime crisis. It also provided a formalized justification for Godley's (1992) opposition to the Maastricht Treaty. His main objections were that the Treaty proposed an independent central bank that would neither advance funds to central or national governments nor purchase sovereign bonds, and that the Treaty had no place for a strong central government pursuing counter-cyclical policies. This, for Godley, was an unsustainable set-up and a recipe for future disasters.

Godley has been called a 'Cassandra of the Fens', for instance, forecasting a huge rise in the number of unemployed workers as a result of Margaret Thatcher's economic policies. Perhaps his best-known piece describing an impending economic disaster, at least in the USA, is the report that he published for the Levy Economics Institute in January 1999, appropriately titled *Seven Unsustainable Processes* (Godley 1999c). This piece is considered by many to be the announcement, justified by economic analysis, that the Clinton boom years of the late 1990s were about to come to an end. Furthermore, many of the unsustainable processes described in the 1999 paper were still present in the 2000s, and so some observers also consider this paper to be a warning that the growth associated with the US housing boom of 2001–2006 was to peter out, as it did with the sub-prime financial crisis. The seven unsustainable processes occurring in the USA at the end of the 1990s were the following:

(1) the fall in private saving into ever deeper negative territory, (2) the rise in the flow of net lending to the private sector, (3) the rise in the growth rate of the real money stock, (4) the rise in asset prices at a rate that far exceeds the growth of profits (or of GDP), (5) the rise in the budget surplus, (6) the rise in the current account deficit, (7) the increase in the United States's net foreign indebtedness relative to GDP (ibid.: 4).

Godley was much preoccupied with the rising indebtedness of the private sector caused by the household sector, tying it with the huge surpluses that the government sector was then accumulating, making use once again of the fundamental identity $(S - I) + (T - G) + CAB = 0$, as previously described. There was indeed a recession in the USA in 2001. By September 2005, Godley argued that the housing market was about to peak, as the institutional changes in the mortgage industry that had driven up housing prices had about exhausted their effects, thus predicting a catastrophic fall in net lending as a proportion of income, and hence another recession, unless the government took steps to raise the public deficit to no *less* than 8.5% of GDP.¹⁰

While Godley was silent on current affairs for many years after he arrived at the Levy Institute, the *Seven Unsustainable Processes* paper launched an uninterrupted series of frequent medium-term forecasts and analyses, underlining a variety of explosive ratios, either with Godley on his own, or with various co-authors such as Randall Wray, Dimitri Papadimitriou, and Bill Martin, but mainly with Alex Izurieta, Claudio Dos Santos, and Gennaro Zezza. The latter had visited Cambridge as a student in the mid-1980s and had been asked by Godley to work with him. In the 1990s, Zezza provided a great deal of help to Godley in setting up the US model, which he now runs at the Levy Institute, as well as the web platform devoted to the SFC approach.

The model Godley used for his strategic analyses was based on the analysis of private expenditure rather than its individual components (consumption, investment, and the change in the stock of inventories), and is therefore strongly connected to the *New Cambridge* approach. Private expenditure was modelled as depending essentially on past expenditures, current and past disposable income, financial wealth, and the change in consumer debt, mortgage debt, and business debt, as well as the change in stock market prices and housing prices. The relevance of changes in household and business debt for economic activity also reflects empirical observations that Godley made when being at the Treasury. Thus, the circle is completed.

¹⁰ For more comments on Godley's unsustainable processes, see Bibow (2012).

5 Conclusion

Wynne Godley had warm working relationships with many of his collaborators at the DAE of the University of Cambridge, including particularly Francis Cripps, Ken Coutts, and Roger Tarling, and one or two visitors—notably Bill Nordhaus. He greatly enjoyed King's College where he spent a good part of his time and had friends such as Adrian Wood, but he was less comfortable with the academic mainstream where he felt himself an outsider. He maintained many of his London contacts and approached intellectual and political issues in a more open way than the more combative academics. In his interview with Macfarlane (2011), he confided that leaving the Treasury and moving to Cambridge was a mistake from both a financial and personal standpoint. Nevertheless, he was, as noted, devastated by the refusal of the Social Science Research Council in the early 1980s to continue funding the CEPG and spent much of the rest of his life coming to terms with rejection by the academic community of what for him and his associates had been an exciting and creative period of research leading to conclusions which were highly relevant for contemporary policy and politics.

He was an influential figure in UK economic policy circles, first through his work at the Treasury and later through his much-publicized modelling and forecasts with the CEPG and analyses of current policy issues that appeared in the press and were communicated to friends in politics and government. The financial balances approach that he advocated to make sure that forecasts are consistent is now used by a large number of research bureaus in financial firms, including Goldman Sachs. On the academic front, he always saw his work as being part of the Cambridge school of Keynesian economics, although this was not always clear to others. Today, a large number of young and eclectic researchers have adopted the SFC methodology put forward by Godley. Indeed, even some researchers building agent-based models are now careful to provide for stock–flow consistency as defined by Godley. Thus in the academic world, Godley is now much better known and better recognized by Post-Keynesian and heterodox economists than when he worked at Cambridge.

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Robert Charles Oliver (Robin) Matthews (1927–2010)

G.C. Harcourt

1 Introduction

Robert Charles Oliver Matthews, who was always known as Robin, was born on 16 June 1927, in Scotland. He was the son of Oliver Harwood Matthews, WS, and Ida Matthews (née Finlay). His father, an Englishman, became an Edinburgh solicitor. Robin's daughter, Alison Matthews, told me (26 September 2014) that his mother had 'an impeccable bourgeois Scots pedigree'. His parents met during the First World War in Constantinople/Istanbul—Oliver was a Commander Paymaster in the Royal Navy, Ida was in the diplomatic service—and after the war they married and went to Edinburgh where he worked in her father's law firm. Tam Dalyell writes: 'In the 1930s, legal Edinburgh was a small place [where] families knew each other' (Dalyell 2010).

Ida was the major influence on Robin. She spoke fluent French, often to Robin at home, she was at the Paris Peace Conference in 1918 as a translator,

I have been hampered in writing this chapter by being unable to obtain a complete CV of Robin Matthews. As he was a prolific writer, I have had to concentrate on what I hope has been a representative sample of his many wide-ranging publications. I am most grateful to Tony Atkinson, Prue Kerr, Michael Lipton, Alison Matthews, Gay Meeks, the late Aubrey Silberston, Vela Velupillai, Clemens Gresser and the wonderful staff of the Alfred Marshall Library, Claire Butlin of the Clare College Archives, and Bridget Riley, College Secretary, All Souls College, for the great help they gave me while I was writing the chapter.

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and in later life wrote poetry. Robin himself ‘could read and speak Russian, as well as the standard Latin and Greek, French and Italian of his generation and class’ (Alison Matthews, 26 September 2014). Robin went to the Edinburgh Academy for which he had a lifelong affection. He told Dalyell ‘that he owed his career to the School, and in particular to two superb maths teachers’. He went up to Corpus Christi College, Oxford, in 1943 (when he was 16), where he first read Classics (Part I) and then Modern Greats, Politics, Philosophy, and Economics (PPE). He was a research student at Nuffield, 1947–1948, a Lecturer at Merton, 1948–1949, before going to Cambridge as a University Assistant Lecturer, 1949–1951. He was elected a Fellow of St John’s in 1950 and promoted to a University Lectureship in 1951. (His and my great friend, the late Aubrey Silberston, was also a Fellow of St John’s.) In 1965 he was elected to the Drummond Chair of Political Economy at Oxford (with which went a Fellowship at All Souls), succeeding John Hicks.¹

In 1975 Robin was elected to the Mastership of Clare, a position that he held until 1993. (I was told that his election was masterminded by our mutual friend, the late Charles Feinstein, who was then Senior Tutor of Clare.) When Brian Reddaway, another Clare person, retired from the Chair of Political Economy (‘Marshall’s Chair’) Robin replaced him in 1980, becoming Emeritus in 1991. So Robin held the two senior Oxbridge chairs. His many contributions to the discipline, the profession, and the community make it abundantly clear why.

While his career was overwhelming at Cambridge and Oxford, from 1972 to 1975, he served as the Chair of the Social Sciences Research Council (SSRC) where Robin’s astute manoeuvres and integrity—his ‘voice [of] moderation and courtesy, [to which was] added considerable personal charm’, as Dalyell writes—prevented its abolition by Margaret Thatcher, the then Secretary for Education in the Heath government. He held many other major posts: for example, President of the Royal Economic Society, 1984–1986, Visiting Professor, University of California at Berkeley, 1961–1962, a Managing Trustee of the Nuffield Foundation, 1975–1996, Organisation for Economic Co-operation and Development (OECD) Expert Group on non-inflationary growth, 1975–1977, and Chairman, Bank of England Panel of Economic Consultants, 1977–1993. He received a number of honorary degrees and was elected an Honorary Member of the American Economic Association in 1993 and the American Academy of Arts and Social Sciences in 1995. He was elected a Fellow of the British Academy in 1968 and was

¹ I was in Robin’s room in the Cambridge Faculty when Hicks rang to tell him of his election. Believe it or not, I discreetly withdrew as their conversation began.

made a Commander of the Most Excellent Order of the British Empire (CBE) in 1975.

In his first period at Cambridge he was an assistant editor of the *Economic Journal*, working with Austin Robinson and as review editor.² He married Joyce Hilda Lloyds in 1948; they had one daughter, Alison. Joyce died in 2006. Robin died ‘very peacefully’, Tim Smiley (19 September 2010) tells us, after a long illness on the morning of 19 June 2010.

As with the very best PPE graduates, Robin was a real all-rounder. Over the years, his research took in economic theory, economic history, methodology, and, most important of all, policy. From the beginning he had a comprehensive structure in his approaches to economics but not ones ever carved in stone. His teaching, research, and administrative duties over the years made him more and more aware of the inadequacies of ‘conventional economic models of “rational individualistic utility maximisation,” [so that increasingly his] interests moved toward the institutional and psychological underpinnings of economic behaviour’ (*The Telegraph* 2010). His wide-ranging interests and approaches are reflected in his major publications. Though he had no formal training in mathematics after he left secondary school, he was a capable mathematician. He understood theory set out in a mathematical manner and he used mathematics relevantly and with mastery, but also sparingly in his own writings.

Robin always kept up to date with developments in economic theory and history and made sure his teaching embodied them in a critical but fair manner. I vividly remember going to a fine set of lectures which he gave in 1980 to Cambridge undergraduates on macroeconomics and monetary theory and policy in light of the contributions of Milton Friedman and Robert Lucas. He lucidly explained the gist of them; he extracted the positive aspects while maintaining the Keynesian foundations on which he had been brought up and to which he still adhered.

2 The First Cambridge Years

Robin’s earliest work was on the historical and theoretical aspects of the trade cycle in the UK and advanced capitalism generally. His first book, *A Study in Trade Cycle History* (Matthews 1954a), was published by Cambridge

²Robin very kindly gave me Minhas’s important 1963 book to review when I was very much a novice economist. This was typical of his behaviour. Reviewing it had a significant impact on my subsequent intellectual development.

University Press in 1954. The Preface is dated November 1952 so perhaps the manuscript of the book played a role in his promotion to a University Lectureship in 1951. He tells us in the Preface that his chosen method of enquiry—‘quantitative-historical’—was only one of many by which empirical research on the trade cycle may be carried out. He subjected a

single brief period [running from the late 1820s to the early 1840s] to close study so as to be able to do as full justice as the evidence permits to the complexity of the fluctuations experienced while avoiding the dangers of oversimplification through imposing too uniform a pattern on the history of fluctuations in different periods (ibid.: xiii).

His procedure did not allow an assessment of the place of fluctuations in the longer-run evolution of the national economy nor to discover changes in the cyclical process itself over time.

One of his major findings (ibid.: 224) was that the imperfect synchronisation of fluctuations as between industries and between home and export markets, in conjunction with the high proportion of the population dependent upon an industry, where outputs and prices were governed more by weather than by the level of demand, meant that changes in the distribution of income between social classes and between industry groups were a feature of short-period fluctuations scarcely less important than changes in the aggregate of incomes.

As an astute scholar of Keynes, Robin had a keen sense of the importance of finance in economic processes. He noted in his study that ‘even entirely agricultural regions were affected by the boom in joint stock bank formation ... [L]ocal traders were often induced to expand their operations by availability of finance during the boom’ (ibid.). These and other passages document that Robin was already bringing to his analysis a rich mixture and understanding of institutions, evolutionary changes, incentives, and the need to understand in detail what was happening in different sectors, in order to see how they interrelated and also to obtain an understanding of the impact of their linked behaviour on systemic processes.

Robin’s next book was his Cambridge Economic Handbook, *The Trade Cycle* (Matthews 1959). It is still one of the best introductions to the theories of this inherent characteristic of capitalism. As with all the best research, it was closely connected to his teaching. Dalyell writes: ‘As a young lecturer specialising in trade cycle theory, Robin was a beacon of clarity, [explaining] in understandable language the intricacies of the trade cycle theories of such heavyweights of the 1950s as Sir Roy Harrod, Nicholas Kaldor, Michał Kalecki and Franco Modigliani’ (Dalyell 2010).

In the Preface, Robin writes that he was much indebted to Richard Goodwin, Kaldor, Joan Robinson, and James Duesenberry when Duesenberry was on leave in Cambridge. He thanks Aubrey Silberston, Joan Robinson, Kenneth Berrill, and the General Editors of the series, Claude Guillebaud and Milton Friedman (!), for reading the manuscript. Would we now accept the claim by the General Editors (in Matthews 1959: vi) that ‘there is again a large measure of agreement among economists on the fundamental theoretical aspects of their subject’, adding the proviso that there is also ‘a wide divergence of views on policy’? Would we also accept what Maynard Keynes wrote in his Introduction to the series soon after the end of the First World War: ‘[T]he theory of economics does not furnish a body of settled conclusions immediately applicable to policy. It is a method rather than a doctrine, an apparatus of the mind, a technique of thinking which helps its possessor to draw correct conclusions’ (ibid.: vii)?

The titles of the 14 chapters in the table of contents are witness to the extraordinarily comprehensive coverage of the subject. The author saw his task as ‘mainly theoretical analysis’. The aim was to ‘provide a framework within which any particular phase of historical experience of fluctuations...can be usefully studied’. The subject matter was ‘advanced or relatively advanced countries’ (ibid.: 2). Robin thought then ‘that much of the theory of the cycle [could] be stated more or less independently of the exact nature of the underlying growth process’ (ibid.) so that a more systematic discussion of the relation between growth and fluctuations was left to near the end of the book. This separation is not a view that Dick Goodwin would have accepted even then and which, like Kalecki, he discussed comprehensively in his later writings (see, for example, Goodwin 1967; Kalecki 1968).

Robin (Matthews ibid.: 3) noted the general agreement that changes in the level of demand cause fluctuations in national income, that the rise and fall of demand is the essence. A typically wise insight, so characteristic of all his writings, is that output and prices move together whereas if fluctuations arose chiefly from supply, they would move in opposite directions (ibid.: 4). He singles out fluctuations in investment, the most volatile component of aggregate expenditure, as the principal focus, so that the main topic in trade cycle analysis is an explanation of investment fluctuations, themselves associated with the mutual interaction between consumption and investment.

The most difficult formal part of the book is in Chapter 2, ‘the aim of which is to provide a rigorous background to more realistic discussion later on where the chapters are less abstract and mathematical’ (ibid.: 7, fn. 1). Robin started with analysis of the accelerator principle as a means to his end of setting out the capital stock adjustment principle as a looser formulation

of a more satisfying and illuminating general model of the cycle.³ By having investment decisions varying directly with the level of income and indirectly with the stock of capital existence, most of the difficulties with the strict form of the accelerator principle are overcome. It is stressed that ‘inertia in expectations... serves to protect the system from the more extreme versions of cyclical instability’ (ibid.: 48).

The next chapters cover crucial phases of the cycle—upper and lower turning points, inventory cycles and so on—as well as the role of consumer behaviour. In Chapter 13, the author returns to the trend and the cycle. He starts by reminding us that some economists regard them as intimately related phenomena and that it is impossible to treat them in isolation from one another. Others, though, hold that they are independent; the cycle represents a movement of demand relative to supply while the trend is concerned with growth in demand and supply *pari passu* (ibid.: 227).

For Robin a large part of the problem is to evolve an analysis of the demand side of the growth process that is consistent with the analysis of fluctuations around it: ‘We cannot assume that effective demand, which gives so much trouble in the short period, simply looks after itself in the long period’ (ibid.: 229)—that wise insight has fallen on deaf ears in modern times. Technical progress and population growth are singled out, Robin tells us, for imparting a long-run upward trend to the consumption function and/or the inducement to invest (ibid.: 253).

With prescience, Robin argues (ibid.: 148) that monetary and financial factors may strengthen, not weaken, tendencies to real fluctuations, that they are sometimes responsible for magnifying the effects of relatively minor real disturbances into major movements in income and activity—Matthews’s Minsky moment? While Robin thinks it unrealistic to expect to be able to achieve complete stability in investment expenditure, he does not think that a policy of maintaining stable full employment will have an adverse effect on growth and productivity. Such a policy creates a more stable environment within which entrepreneurs are able to plan ahead more effectively (ibid.: 256).

He also argues that with the then existing wage- and price-fixing institutions, only really high levels of unemployment would ensure a stable price level, a policy which has little to commend it. He prefers policies which try to persuade those responsible for setting wages and prices to show moderation. Robin was sceptical of the effectiveness of an application of monetary policy in the slump (ibid.: 262). On page 263, ‘faith by magic’, an early

³Vela Velupillai tells me (7 May 2015) that Dick Goodwin probably did not approve of this ‘re-christening’ of the flexible accelerator.

anticipation of rational expectations, is discussed—if businessmen believe the government is committed to full employment, the government may not need to do anything as businessmen moderate changes in short-run expectations and become confident about long-term prospects. Robin was implicitly criticising much of the conventional wisdom of the time, not least that associated with Dennis Robertson's criticisms of Keynesianism and its policy stances.

The Trade Cycle was published four years after one of Robin's most influential articles, 'The Saving Function and the Problem of Trend and Cycle', was published in *R.E. Studs* in 1954–1955. Already he was considering the nature of the interaction between trend and cycle, starting from Roy Harrod's 1939 classic article and taking in Goodwin's emerging, indeed emerged, writings on their entwining (see Harcourt 2015), Duesenberry's 1949 classic on the consumption function, Modigliani's theory of lifetime saving behaviour (Modigliani 1949; Modigliani and Brumberg 1954; Modigliani and Ando 1957), and John Hicks's 'contribution' to the theory of the cycle (1950).

Goodwin had concentrated on the investment function as the means by which trend and cycle could be merged in an entwined relationship. Robin reached similar conclusions to Goodwin but concentrated instead on the saving function. The starting point, he argued, is the neglect in Duesenberry's book of ongoing productivity growth when he dated the onsets of the ratchet effect as the previous highest levels of income attained, rather than the previous lowest levels of unemployment. This is the sort of insight that the best economists provide and then spell out the implications, in Robin's case, for the relationship between trend and cycle. The latter has become the base on which our understanding of the processes at work in capitalism is built.

Subject to the limitations of 'a relatively high level of abstraction' (Matthews 1959: 95), Robin concludes:

The hypothesis that the proportion of income saved depends on the amount of unemployment leads to the inclusion in the saving function of a negative term that grows *pari passu* with population and productivity [causing] growth to take place in the type of system that would in its absence fluctuate about a stationary level. The growth that takes place will be at the natural rate ... The shape of the saving function may, therefore, be regarded...as the explanation of why the increase in saving does not prevent income from growing at the natural rate. The average level of activity over the cycle...will lead to an average ratio of saving to income equal to the average ratio of investment to income required by the natural rate ... The natural rate will...depend in part on the average level of activity, so that anything that...raises [the latter] will also raise the growth rate.

Robin thus provided a more convincing explanation of an endogenous link between the natural rate and the investment to income ratio than Kaldor's imposition of the required ratio onto his macroeconomic 'Keynesian' theory of distribution and its assumption of full employment being necessary to achieve long-period growth (see Kaldor 1955–1956; Harcourt 1963).

Here, Robin also departs from the usual interpretation then of Harrod's theory that g_w , g_a , g_e , and g_n do not interrelate whereas in fact technical progress is embodied in the capital stock by new accumulation, as Salter (1960, 1965) showed and the late John Cornwall devoted his life's work to developing (see Harcourt and Monadjemi 1999).

In 1961 Robin published in *Economica* an article entitled 'Liquidity Preference and the Multiplier'. Using Keynes's liquidity preference theory of the rate of interest, Robin wrote one of the earliest and best accounts of what is happening in the banking and financial sectors of the economy when the Kahn–Meade–Keynes multiplier is working itself out in the real sector. He also showed how self-finance and 'other imperfections in the capital market' (ibid.: 37) could be incorporated into the analysis. All the assumptions are set out clearly and concisely and the implications and limitations of the analysis are similarly treated. The article illustrates Robin's ability to integrate into his approach appropriate analysis from often seemingly opposing stances. Thus he incorporates ideas from both Robertson's loanable funds theory of the rate of interest and his lagged form of the consumption function with its accompanying use of process analysis. Keynes's theory is set out in terms of a utility function analysis incorporating indifference curves between bonds and money (but Robin points out that the theory takes in choices at the margin between all possible financial assets, expressing Keynes's essential insight that the levels of rates of interest are the means by which an uneasy truce is brought about between bulls and bears in financial markets). Robertson's theory is argued to be partial because it is solely concerned with flows. As it ignores stocks, it overlooks that what is being analysed are markets where stocks dominate flows and expectations about future yields dominate what Robertson regarded as the fundamental determinants—productivity and thrift.

Robin's formal analysis of the approach to a new short-period equilibrium following an autonomous change in either the saving function or the investment function allows him to discuss what happens within each period on the way where temporary hoards or dishoards of money may occur and neither desired nor planned saving and investment, or their equality, are achieved until the new equilibrium has been reached. He combines the simple multiplier analysis with other factors affecting holdings of cash and examines

their impact on both the size of the multiplier and that of the multiplicand. The discussion at the close of the article of imperfections and retained profits argues that the latter may bring an element of Say's Law reasoning back into an essentially Keynesian analysis as the retention of profits serves to match a planned increase in investment expenditure.

Altogether, the article is an example of highly original and relevant thinking set within major approaches by others to the issues concerned at the time. The tone of the article reveals an author who is on top of his material but who is also matter of fact, modest, and unassuming, so typical of Robin then and later.

In 1964, with Frank Hahn, Robin published in the December 1964 issue of the *Economic Journal* the famous survey of the theory of economic growth, the survey article that has been the role model for survey articles ever since. Though all three sections of the survey achieved high levels of exposition and scholarship, the second section on technical progress, which was primarily due to Robin, is its jewel in the crown. Its lucidity and clarity bring out the deep economic intuition it contains.

The first section, 'Growth without Technical Progress', is a joint product. It starts with a methodological discussion concerning steady states, existence, different types of stability, and different classifications of dynamic analysis. It takes the Harrod–Domar model as its starting point and reference point from then on. Coupling Harrod and Domar together is not a correct reading of their respective approaches and contributions, even though their writings contain the same expressions for their growth rate concepts. Harrod was concerned with growth rates at a point in time and their relationships, the dynamic counterparts of levels at a point in time in static analysis. He wanted to know the conditions that allowed overall investment plans to be realised and so maintained because expectations had been fulfilled—hence his concept of the warranted rate of growth, g_w , which he distinguished from the natural rate of growth, g_n , a supply concept which set out the maximum potential growth from workforce growth and technical progress. He examined the stability, or otherwise, of g_w and how g_w and g_n may be equalised. Domar was concerned with the maintenance of full employment of labour and the capital stock over time when both the employment-creating and capacity-creating effects of investment are taken into account. Hahn and Matthews ran these different objectives together, which defined the classification of their discussions from then on.

In Section 1, the authors discuss how Harrod's two puzzles, but especially the stability of g_w , have been tackled by various authors. Having established from the Keynesian saving–investment relationship that $g = s/v$, they examine the contributions of various authors—Kaldor, Joan Robinson, Solow, and Swan—who

tackled initial inequality by looking at market signals which affect the value of v and those which affect the value of s through changes in the distribution of income when $s_{II} > s_{II}$. They examine first one-sector, one-commodity models, and then two-sector models. They provide helpful diagrams, and sometimes simple algebra, to illustrate their excellent economic intuitions.

All this sets the stage for Section 2 where the effects of technical progress on the attainment or not of steady-state growth are exhaustively analysed. An organising principle is whether technical progress is Harrod-neutral or Hicks-neutral. The discussion sets out both the formal requirements for various results and the likelihood or not of actual economies providing the conditions for them to be achieved. The discussion includes the introduction of vintage models and the stance that rejects the use of a production function whereby the effects of deepening are distinguished from those of embodiment of technical advances through accumulation. Much of the discussion takes in the writings of Kaldor on these themes, the development of his growth models from 1957 to their culmination in Kaldor–Mirrlees in 1962.

The survey was written while both Ken Arrow and Bob Solow were on leave in Cambridge for a year. The first draft was discussed in early 1964 by Solow at the secret seminar—the seminar run by Joan Robinson, Kahn, and Kaldor in order to discuss on-the-frontier topics (but which excluded those members of the Faculty who were associated with Robertson). Arrow was present on this momentous occasion.⁴

3 A Highlight from the Oxford Years, 1965–1975

Robin's most admired and cited article is his 1968 paper in the *Economic Journal* on why the UK had had full employment since the War. In it, his great understanding of historical and political processes linked to his command of sound economic theory (not only that of Keynes) combined to produce a convincing set of arguments for why full employment was attained and maintained, a set of arguments so much more sophisticated and measured than the simple appeal of the then conventional wisdom to the application of Keynesian policies by governments in the post-war period.

Robin starts by contrasting the employment and unemployment experiences of the nineteenth century (up to 1914), the interwar years and the post-war years.

⁴One of the greatest intellectual regrets of my life was that I was not present because I caught mumps the week before from a member of the Department of Applied Economics (DAE). He came to work when he was most infectious and I foolishly took it upon myself to tell him to go home as some of us had small children.

Making adjustments the best he and other economic historians could, the unemployment record of the nineteenth century sits in between those of the other two periods. Robin systematically attempts to put orders of magnitude and qualitative judgements on the likely causes at work—Marxian unemployment associated with insufficient capital to allow full employment of labour, dual labour markets whereby one portion of the workforce is guaranteed employment for long periods, while the other portion is at the mercy of daily hiring and firing. The proportions of the first to the second changed over the three periods concerned. Keynesian unemployment due to deficient effective demand became more important between the nineteenth century and the interwar years but was not that important a part of the story because of full employment in the post-war years. But was this primarily due to direct government measures as opposed to creating an environment in which animal spirits were more buoyant so that required expected profits were not as high as in the preceding periods? Robin highlights the much higher amount of private sector accumulation in the post-war period than in the two preceding periods. He dubs this a result of a ‘safety net theory of the role of government’: ‘although the acrobat had not fallen, his knowledge of the presence of the safety net had given him confidence and improved his performance’ (ibid.: 562).

Robin’s aim is to test two propositions:

- (i) As compared with the inter-war period there has...been an increase in effective demand...relative to supply; but it is non-proven that this is due to government policy...in any simple sense.
- (ii) The decline in unemployment as compared with before 1914 is not...a Keynesian phenomenon at all (ibid.: 556).

After a wide-ranging and exhaustive account of the three periods, Robin sums up:

Part of the reason for low unemployment in the post-war period has been the trend increase in the scarcity of labour relative to capital. This is non-Keynesian...will persist and...has little...to do with government policy ... [I]t provided...protection to labour from the effects of fluctuations in demand. [Also] there has been a major change compared with the abnormally depressed inter-war period in the level of effective demand relative to supply [but] it is unclear how much of this has been due to government policy (ibid.: 568).

Robin concludes with the proviso that he ‘does not mean to deny that a different policy...could have prevented there being full employment ... That this was not [so] is something for which economists and the Keynesian revolution can have some credit’ (ibid.: 568–569).

4 Return to Cambridge in 1975: Master of Clare and then, as well, Professor of Political Economy in 1980

As I noted above (see page 957), Robin's teaching, research, and administrative duties over the years made him more and more sceptical of the conventional mainstream model of human behaviour, so that his interests moved increasingly towards its institutional and psychological underpinnings. This change may be discerned in his review in the *Economic Journal* of Fred Hirsch's 1977 classic, *Social Limits to Growth*, and, perhaps, even more so, in 'Animal Spirits', his Keynes Lecture to the British Academy in June 1984, and in 'The Economics of Institutions and the Sources of Growth', his Presidential Address to the Royal Economic Society in April 1986.

He wrote of Hirsch's book on a most important issue, the fruits of growth and why they seem increasingly disappointing, that it was 'exceptionally interesting, original and well written' (Matthews 1977: 574). It addressed 'uncomfortable topics that economists prefer...to sweep under the carpet'. Hirsch, though, had 'the intellectual stamina to develop his...wide-ranging treatment of them into a coherent whole' (ibid.).

Hirsch posed three main problems:

- (i) *The paradox of affluence*—why has economic advance remained a compelling goal, even though its fruits are disappointing?
- (ii) *Distributional compulsion*—why is there so much emphasis on the division of the pie when only a larger pie can raise living standards for most of us?
- (iii) *Reluctant collectivism*—why is the predominant trend towards collective provision and state regulation when individual freedom of action is extolled, even given unprecedented rein in non-economic areas, for example, aesthetic and sexual standards? (Hirsch 1977: 1).

Robin goes carefully and clearly through each. A major classification by Hirsch is the division of goods into 'material' and 'positional'. Access to the first is a function of real income so growth brings greater quantities of them to citizens and society alike. With positional goods, however, access is a function of relative income and general growth does not usually increase their availability, not least because many are in fixed supply. Robin points out that Hirsch's 'impressive scholarship' (Matthews 1977: 574) takes in philosophy, politics,

and sociology and has shown that what is often non-positional for economists is positional for sociologists.

Robin's first criticism relates to the importance of distinguishing between waste that arises from the positional nature of goods and those due to market malfunctioning. He lauds Hirsch for pointing this out but criticises him for not always pushing it far enough. Moreover, some of Hirsch's examples, for instance, excessive pursuit of educational qualifications as a screening device for scarce top jobs, result from faults in the market mechanism, not from the positional nature of the esteemed jobs, so that 'it is not self-evident that economic growth aggravates it' (ibid.: 575).

A fundamental point that made Robin 'uneasy' is the argument 'that economic growth has increased the importance of positional goods... because it has...come nearer to sating biological needs', which is what, Hirsch argues, non-positional goods do. Robin finds the concept of biological needs 'dubious' so while it could be true that positional goods are 'more important than they used to be...the biological argument does not prove it' (ibid.: 576).

Robin agrees with Hirsch's emphasis on the competitive system producing 'an excessive and increasing *bias towards commercialisation*' (ibid.; italics in original). This can involve loss of utility because utility depends on how, as well as what, goods are supplied. Hirsch, writes Robin, 'disposes very effectively of the shallow psychology of some "market economists"', that 'the model of man is and ought to be that of the gangster' (ibid.). He agrees with Hirsch that it has been a mistake of economists to shy away from morality but thinks he underrates the practical difficulties of Hirsch's 'as-if altruism' higher form of self-interest (ibid.: 577). People pay taxes because, while it is in their interest not to, it is not in their interest to have a system where no one does.

Robin sums up:

The troubles [Hirsch] diagnosed are real enough ... They do make a major dent in the conventional case for capitalism ... There is no presumption that further growth could cure them. [Robin is] not so convinced that the troubles are *due* to growth, or at least the increased satisfaction of material needs (ibid.: 578; italics in original).

Finally, Robin points out that it is only after the Second World War that steady improvements in living standards were expected, so that increasingly satisfaction from them is discounted: '[H]ence increased concern with positional and distributional elements' (ibid.). Sadly, I suspect both Fred Hirsch

and Robin would have considerably revised their views in the present much more harsh and non-collective environment of modern societies.

A major treatise, *British Economic Growth, 1856–1973*, co-authored by Robin with Charles Feinstein and John Odling-Smee, was published by Stanford University Press in 1982. It was one of a series of parallel studies of growth in industrial countries overseen by Moses Amramovitz and Simon Kuznets. Research for it started while Robin was still at Oxford. The subject matter is the causes of growth in the UK from 1856 to 1973, especially in the post-war period to 1973, ‘years of unparalleled growth in Britain, as in other countries’ (ibid.: 3). Robin thought he spent too much time on the project. Be that as it may, it is easy to concur with the judgement of the other authors that they wished ‘to put on record that it was...Robin...who made by far the most significant contributions to the design of the investigation, the analysis of the material and the writing of the final text’ (ibid.: v).

In a review in the *Business History Review* in 1983, the then young Barry Eichengreen places the volume within the rich tradition of quantification in the study of British economic growth, starting in the eighteenth century with Gregory King and William Petty and continued in the early 1960s landmark studies through Brian Mitchell and Phyllis Deane’s (1962) *Abstract of British Historical Statistics*, and Deane and W.A. Cole’s (1967) companion volume, *British Economic Growth, 1688–1959*. He assesses the volume as ‘a most worthy successor’ and identifies its statistical companion as Feinstein’s *National Income, Expenditure and Output in the United Kingdom, 1855–1964* (Eichengreen 1983: 311).

There are five parts to the book: Part 1 concerns concepts, formulae, and methodology; Part 2 examines growth at its most aggregate; Part 3 looks at these aggregates at sector level; Part 4 is a ‘special topics’ section—aggregate demand, investment, and international transactions; and Part 5 summarises the book’s findings on the proximate sources of growth (ibid.).

The focus is on long-term trends in the rate of economic growth from cyclical peak to peak. The cycle is only relevant in so far as it has implications for the secular rate of growth; Goodwin’s influence was not yet overwhelming in Robin’s approach. Britain’s economy is shown as nearly always in decline relative to other industrial countries from the mid-nineteenth century on. Many factors are examined to explain this, providing, Eichengreen argues, a mostly familiar story ‘with exceptional skill and insight’ (ibid.: 312).

Another reviewer, Donald McCloskey (1983) is also full of praise but, as would be expected of him, with many negative caveats. He writes that the volume ‘is a double triumph...in historical economics, and in the national income school’ (ibid.: 14). He graphically sums up the main story as ‘a great U’

in the movement of accumulation, growth, and productivity—declines from 1856, bottoming around the Second World War and ascending up to 1973 when the volume ends. Much of the detailed narrative ties in with the evidence for the themes of Robin's 1968 *Economic Journal* article. I suspect that towards the end of writing the volume Robin was chafing under the constraints imposed by Ambramovitz and Kuznets in order to have comparable studies with other economies, because he was getting more sceptical of the merits of the framework itself, what McCloskey dubs 'nervous neoclassicism and uneasy Keynesianism' (ibid.).

As I noted above (see page 966), Robin gave two important lectures in the mid-1980s, the British Academy Keynes Lecture and the Royal Economic Society (RES) Presidential Lecture. Both are vintage Matthews: his wide-ranging knowledge and understanding together with his reflective deep thinking are unobtrusively present. For the Keynes Lecture he thanks an impressive list of friends: Ambramovitz, Arrow, Margaret Bray, Stephen Dunnett, Hahn, Kahn, Gay Meeks, Don Moggridge, Tad Rybczynski, Tibor Scitovsky, Amartya Sen, and Silberston.

The starting point is three well-known passages from Chapter 12 of *The General Theory*. The first relates to the average outcome of most investments that disappoint 'the hopes that prompted them', so that investment is as much induced by the 'temptation to take a chance' as by 'cold calculation' that by itself would not lead to much investment (Keynes 1936 [1973]: 150). Keynes then defined 'animal spirits' as 'a spontaneous urge to action rather than inaction', to which he contrasted calculations remarkably like his own definition of the *marginal efficiency of capital* in Chapter 11: Because 'decisions affecting the future...cannot depend on strict mathematical expectation...it is our innate urge to activity which makes the wheels go round' (ibid.: 162–163).

Robin points out that uncertainty is the main theme of Chapter 12, that animal spirits take second place to Keynes's discussion of the volatility of investment expenditure, and that they are not mentioned in Keynes's 1937 *Quarterly Journal of Economics* article where he develops his concept of uncertainty which had originated in *A Treatise on Probability* (Keynes 1921 [1973]).

Robin further points out that Keynes moves rather confusingly between the effects of uncertainty on real investment, on the one hand, and on stock exchange fluctuations, on the other. Both give rise to instability, but purely speculative activity on the stock exchange arises when participants attempting to outguess everyone else dominates. Keynes does map out connections between stock exchange fluctuations and actual investment. Robin sums up what he takes from Keynes's discussion: 'The hypothesis...is that people...are predisposed towards actions rather than inaction and hence are predisposed

to ignore some of the downside risks of action ... This serves as a stimulus to investment...that is positive on average but...does not override unambiguous prospects of gain or loss' (Matthews 1991: 105).⁵

The aim of the lecture is to relate this type of idea 'to recent thinking about the psychology of human behaviour, and then...appraise its significance for investment and...other aspects of economic life' (ibid.). Robin points out that Keynes built on a well-established Cambridge tradition in these areas—Marshall, Pigou, Lavington, Robertson, and Hawtrey, 'ideas with some radical and far reaching implications ... Activity itself—travelling hopefully—assumes as much importance...as the consequence of action—arriving' (ibid.: 107).⁶ He distinguishes two aspects of the current thinking: motivational, which corresponds to animal spirits, and cognitive, the response to uncertainty (ibid.: 108). He starts with Tibor Scitovsky's *The Joyless Economy* (1977). Scitovsky mainly discussed consumption, but his ideas may also be applied to investment behaviour. He distinguished between comfort and pleasure and related both to arousal. Comfort is a function of the level, pleasure, and the rate of change of arousal, which itself depends on stimulus. Robin links doing and being and illustrates their interaction if formalised in Scitovsky's manner as giving rise to cycles. Marx's trade cycle model is cited as an example. The discussion leads on to the role of Simon's concept of satisficing and the prospect theory of Kahneman and Tversky (1979).

In concluding, Robin suggests the need for the application of modern psychology and animal spirits to takeovers and human capital as well as physical investment. The approach may also be helpful in explanations of technical progress and productivity growth where economic analysis has been much less successful. Because all but the most routine decisions involve long drawn out effects, uncertainty of necessity is involved. So Robin brings together insights from his work in 1964 on technical progress and his more recent excursions into modern psychology to raise challenging hypotheses and avenues for future research: exactly what a public lecture should do.

Robin's RES Presidential Address was given in Cambridge on 9 April 1986, at the *RES/AUTE* Conference, one of the last times the two were combined. The lecture starts with a percipient quote from Marshall's 1885 Inaugural Lecture: 'The chief fault in English economists at the beginning of the [nineteenth] century was...that they did not see how liable to change are the habits and institutions of industry' (Marshall quoted in Matthews 1986: 903). Robin conjectures that this was an olive branch from Marshall to Archdeacon Cunningham, the High Tory economic historian who was an anti-theory

⁵ All page references are to the revised version of the Keynes Lecture.

⁶ Robin especially singles out Dennis Robertson who quotes 'from Heraclitus "everything is in flux": uncertainty and Walt Whitman: "urge and urge and urge, always the procreant urge of the world": animal spirits' (ibid.: 107).

institutionalist famous as a polemicist, and ‘the only other serious contender for the Chair [of Professor of Political Economy]’. Robin quips: ‘Perhaps *he* should be regarded as the spiritual father of the Cambridge school’ (ibid.; italics in original).

Economic theorists of the first half of the twentieth century concentrated on ‘the theory of the interaction of unchanging utility-maximising individuals in a *given* institutional structure’ (ibid.: italics in original), that which Marshall wrongly thought well established. American institutionalists, including Thorstein Veblen, had little lasting impact because their doctrines were too ill-defined, their only clear message was that there was something seriously wrong with neoclassical economics, a judgement that rings bells at the present time!

Things have greatly changed; the economics of institutions is now all the rage and it has brought economists in touch with other social sciences: ‘A body of thinking has evolved based on two propositions: (i) institutions do matter, (ii) [their] determinants are susceptible to analysis by the tools of economic theory’ (ibid.). The aims of the RES Lecture are to flesh out ‘the central concepts... of this thinking and... consider its application... the contributions of institutional change to economic growth’ (ibid.: 903, 908).

Thinking in this area comes from a number of different angles but there is overall convergence as well as different emphases and different definitions. Robin names four different angles: ‘The first identifies economic institutions with alternative systems of *property rights* laid down by the law’ (ibid.: 904; italics in original). He refers to Coase’s 1960 argument that any system of property rights can lead to Pareto efficiency, provided it is a complete system but that transaction costs stop this occurring so that some incomplete systems are more conducive to Pareto efficiency than others. This approach connects to the huge literature on law and economics.

The second sees institutions as *conventions* or norms of economic behaviour, an ‘approach that has affinities with moral philosophy’ (ibid.). The third concerns *types of contracts* in use, for example, whether insurance is available and the lengths of time for which labour is employed. The fourth is a subhead of the third—contracts and authority, ‘about who decides what’ (ibid.).

Robin cites Coase (1937) as its modern origin, the impact of transaction costs on whether the market or the command system within the firm prevails. A common feature of the four is ‘the concept of institutions... as sets of rights and obligations affecting people in their economic lives’ (Matthews 1986: 905). So Robin sees a system of institutions as defining what markets (voluntary exchanges) exist and how economic relations are regulated in areas without markets. For Robin, transaction costs consist of the costs of arranging a contract *ex ante* and monitoring and enforcing it *ex post*—production costs are the costs of executing the contract.

Robin identified a static and a dynamic aspect of institutions. When considering economic change, he argues that institutional change may be due to new ideas or an external change in costs. The object therefore is not to minimise transaction costs but the sum of transactions and production costs. He quotes Douglas North's claim (1984) that an enormous increase in transaction costs that has been associated with the increase in the division of labour implies that skills in dealing with things improve at a rate way ahead of those dealing with other people.

The second part of the Address discusses the contributions of institutional change to economic growth (income per head). As sources of growth, institutional change takes two different forms. First, institutions need continual adaptation because of the changing environment of techniques and tastes, a necessary part of growth but not an alternative source. The other view is that the attainment of Pareto-superior institutions is not achieved immediately but is a very long-run process (Matthews 1986: 908).

Robin discusses the views associated with these differences in the writings of Posner on the roles of law and the changing influence of government. He concludes that in this area theory has run far ahead of empirical work and urges that more time and effort be spent on the latter, a conclusion to be expected of an economist whose own empirical work, as we saw, was economic history analysed in the style of an economist.

Robin was Master of Clare for 18 years, from 1975 to 1993, a period as Head of an Oxbridge House way beyond what is now considered to be the maximum either bearable or desirable (in my view, 7–10 years). When he retired, the consensus was that he had been an outstanding Master. He brought scholarly ideals, liberal approaches, especially amongst Fellows—to everyone he was Robin—a wicked good humour, integrity, and enthusiastic championing of undergraduates. He expertly guided the College from Lord Ashby's inheritance of 'a medium-size college which was in good shape, though not particularly wealthy' (Clare College 2010: 6) to a College with a Fellowship of over 80, nearly 600 undergraduates and graduate students combined, near equality of men and women student numbers, and a considerable increase in women Fellows, though not anywhere near enough in numbers and senior university posts as Robin himself would have wished.

He was a wise and sometimes passionate chairperson, who defended his ideals of a university and a college and who did not shirk confrontation, though he was deeply sensitive to criticism. In his tribute to Robin at the Memorial Service on 20 November 2010, Terry Moore noted that, '[O]ne shrewd Fellow observed that [Robin] will not be remembered as an impartial Chair..., but as one who pondered the responsibilities of a College, felt [they] entailed high-principled behaviour, and fought his corner with all the expedients he had learned during his stint with the SSRC and...long term as Master' (ibid.: 10).

Robin presided at High Table without pomposity or formal dignity. He led conversation in the Combination Room on all manner of topics. He drew on his appreciation of his Scottish education, his passion for chess—he had an international reputation for his writing on solutions to chess problems—he told amusing tales of his experiences on public bodies, in a bitterly divisive Faculty in Cambridge and in that unique Oxford institutional fossil, All Souls College. Young Research Fellows greatly enjoyed his frequent visits to the Combination Room on non-formal evenings where his wit and wisdom were unobtrusively on display. Hence the following judgement: his ‘most notable achievement is to have nurtured a body of Fellows, the majority of whom were elected during his [Mastership] and who, in the College, form a congenial and cohesive society’ (ibid.). It also ought to be added that he led a major fundraising campaign which resulted in a new library, common room, and musical facilities.

5 Robin and Chess

A consuming passion of Robin’s was the solution of chess problems. At the Memorial Service for Robin, Michael Lipton, also a chess tragic who collaborated with Robin on one book, said: ‘For all his human qualities, Robin’s career was in large part a life of the mind, lived mainly through economics and chess problems’ (Lipton 2010: 20). ‘[He] was unique in his joint distinction in both areas’ (ibid.: 23). He concluded:

Robin’s chess composition, like much of his economics, is an implicit commentary on the scope, meaning and limits of economisation as a guiding principle. His best chess problems will preserve his name forever ... [M]uch of his economics will long be read and analysed for its integrity, balance and new, often deep, never trendy insights ... [T]he uniqueness of his intellectual life lies in bridging...at a high level, these two very different worlds (ibid.: 24).

6 Further Activities

Though I used to tease Robin on the basis of his limited success in ‘reforming’ the Cambridge Faculty⁷ so that his comparative advantage lay much more in scholarship than changing administrative regulations, the late Aubrey Silberston in his affectionate and joint tribute to his great friend in the January

⁷In retrospect, he came to think that the attempted reforms were misguided, to some extent anyway.

2011 *RES Newsletter* pointed out that ‘Robin had considerable success in administration in other spheres’, especially the SSRC and Clare. He writes:

Robin faced opposition to the SSRC from Margaret Thatcher...but did not have the major problems that Michael Posner encountered a few years later. [Nevertheless,] Robin did an excellent job as Chairman...,with his outstanding mind, his strong sense of fairness and his ability to steer in an acceptable way, in a clear and rational manner, on important funding issues affecting many different institutions and individuals (Silberston 2011: 12).

Though Robin was never an economic policy advisor to any government (he was to the Social Democratic Party (SDP)), he nevertheless played an important role in public discussions of appropriate policies for the UK government. Silberston tells us that soon after Robin came to Clare, he and Alec Caincross ‘decided there was room for an informal group of economists who could tackle conflicting views of the problems of the British economy’ (ibid.). The CLARE Group was born in 1976 and ran to 2002; it included economists from ‘some dozen different institutions—Reddaway, Feinstein, Posner (Cambridge), David Henderson, Tony Atkinson, Silberston (London), Caincross, John Fleming, Peter Oppenheimer (Oxford), Dick Sargent, Marcus Miller (Warwick). Other members were John Kay, Mervyn King, Alan Prest and Brian Tew. The Group produced over 50 articles. Robin and Sargent edited a volume of the earlier papers in *Contemporary Problems of Economic Policy* in 1983’ (ibid.).

7 Conclusion

When Robin retired from his Chair and then the Mastership in the mid-1980s, he and Joyce moved to Stradbroke and then Diss in Suffolk (Alison Matthews, 26 May 2015). After Joyce died in 2006, Robin came back to Cambridge. He lived in a unit off Grange Road, well within walking distance of Clare, where he would have lunch most days, and of his close friends, Dorothy and Frank Hahn, who lived in Adams Road. (The house, 16 Adams Road, had been the Matthews’s house before they went to Oxford. Frank said he had long coveted *chez* Matthews, not least because he would never be able to afford his ultimate ambition, a Queen Anne Mansion.)

I encouraged Robin to come to the Austin Robinson Building in Sidgwick Avenue for the traditional 11 o’clock coffee break for coffee in the Richard Stone Room, so that he could meet new members of the Faculty and chat

to old friends and former colleagues. Of those times, I especially remember his incisive dissection of each sentence of the Lord's Prayer, pulling out inner meanings and advancing cogent reasons why he personally could not accept what often was, he showed, illogical and/or incoherent.

His sight, never good, deteriorated in his last years, so that he had to read through a magnifying glass with books set up on a dais. He bore with fortitude the discomfort of his final illness, earning the affection and respect of his carers.

As many have said, Robin's legacy to the Cambridge Faculty and to the profession through his wide-ranging scholarship will be a lasting one. Let me close, though, on a personal note. Robin had an austere presence but he was basically a friendly and kind person, extremely supportive of the young, both students and colleagues. He was excellent company, with a dry wit. He enjoyed tossing about ideas and old-fashioned gossip. As with many others, I regard it as a great privilege to have been his colleague and friend, in my case, for over 50 years.

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Luigi L. Pasinetti (1930–)

Mauro Baranzini and Pier Luigi Porta

1 Introduction

Luigi Pasinetti was born on 12 September 1930 in the village of Zanica (south of Bergamo, Northern Italy). He was the eldest son of a modest family. His father, Giovanni Angelo, was employed as a *disegnatore edile*, or architect, with a local construction firm, which was forced to close down during the Second World War. Luigi's father being thus unemployed, the family had to rely on the income provided by his mother, Romilda Arzuffi, who was the midwife of the small village and surrounding area. A few months after losing his job, Giovanni Angelo decided to set up a small construction firm. The business was far from thriving, and the family barely managed to survive. Life was hard, compounded by food rationing. Unfortunately, Luigi's mother died suddenly in April 1949, aged 49. At that stage, Luigi was 18 and was just about to complete high school and earn the *Diploma di Ragioniere*, a sort of A-level focusing on accountancy and business studies.

In spite of a successful career in school, it was not easy to find a job. Only towards the end of 1949 was Luigi offered employment as a clerk in the book

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keeping division of a manufacturing firm based in Milan. He was happy to cease to be a financial burden on his family: not much more than that, though, as his wage was barely enough to cover his food and lodging. After a few months of hard work, the headquarters of his firm moved close to the seat of the Catholic University, where the Economics Faculty gave evening classes for working students. During one of his first visits there, Pasinetti dared to enter one of the large amphitheatre-shaped classrooms at the end of the second quadrangle where Arnaldo Masotti, a mathematician of the Polytechnic of Milan, was explaining the concept of a limit in front of a large class. Luigi was impressed by this lecture as well as by the topic, and decided that *that* was the sort of thing he wanted to do. The degree at the Catholic University could be obtained either by day classes or by evening classes: he chose the latter in order to be able to continue his work to support himself through university. In 1953–1954, he completed his dissertation on ‘Econometric Models and their Application to the Trade Cycle Analysis’, under the supervision of Francesco Vito and Siro Lombardini. At that stage he quit his job and got down to working full-time on a research scholarship. In those years he got in touch with Beniamino Andreatta, Pasquale Saraceno, Romano Prodi, and many others. He was strongly encouraged to go abroad for research. His first destination, in 1956, was Cambridge, UK, where he was a research student and completed his PhD in 1962, with one year at Harvard (1957–1958) and two years at Nuffield College, Oxford (1959–1961). At present, Luigi Pasinetti is rightly called the ‘senior’ representative of the Cambridge Post-Keynesian School of Economics, both because of his acknowledged achievements and for his early involvement with the pupils of John Maynard Keynes. He today shares this title with Geoffrey Harcourt, who is one year his junior.

While he was a Research Fellow at Nuffield, Pasinetti was appointed, thanks to the influence of Richard Kahn and Nicholas Kaldor, from 1 October 1961, at Cambridge as an Assistant Lecturer in the Faculty of Economics and Politics. Soon after, he was made a University Lecturer and from 1973 a Reader in the same Faculty, being the second scholar of Italian origin, after Piero Sraffa, to reach that rank. In the summer of 1976, he resigned his position in Cambridge and returned, mainly for family reasons, to Italy. At the Catholic University of Milan, his alma mater, where he is now Emeritus, he took up the Chair of Economic Analysis and was elected Dean of the Faculty of Economics. Through the years, Pasinetti has taught in many universities around the world and in the 1990s he successfully supported the foundation of the University of Lugano, the first university to be established in the Italian-speaking part of the Swiss Federation. He was elected President of the Italian Economic Society for 1986–1989. Pasinetti was also the first President of the European Society for the History of Economic Thought in 1996. He has been active in the International Economic Association,

of which he is honorary President. Pasinetti is a Fellow of the Lincei Academy in Rome and of the Lombard Institute in Milan.

Pasinetti's soaring international fame set in after moving to Cambridge in 1956. At the same time he also gained access to the top economic journals of that period. His 1960 paper on Ricardian economics remains a classic for its penetrating insights. The author exhibits from the start his typically clear and perceptive style. Pasinetti's own research is, first and foremost, an inquiry into the theoretical tools for a *dynamic* theory of economic growth and income distribution. His Cambridge PhD thesis on 'A Multi-Sector Model of Economic Growth' soon turned into a well-known piece of analysis (Pasinetti 1965), which he would develop later on in his 1981 and 1993 volumes for Cambridge University Press. His whole work, in a distinctive Cambridge fashion, is explicitly rooted in classical economic analysis; in other words, he has always felt a deep attachment to the Cambridge tradition. For about three decades he was closely associated with Richard Kahn, Nicholas Kaldor, Joan Robinson, Richard Goodwin, Richard Stone, and Piero Sraffa.

During the 1960s Pasinetti's theoretical hits were outstanding and surprising. His 1962 theorem on the Cambridge equation—a long-run equilibrium relationship between the rate of profits and the rate of growth, usually written as $P/K = n/s_c$, where P is the total profit, K is the capital stock of the system, n is the constant rate of growth, and s_c is the exogenously given propensity to save of the 'pure' capitalists—would soon become famous and give rise to a vast literature. Pasinetti's creative insight provided a correction and an intriguing generalization of the Kaldor model of growth and distribution. Neoclassical economists, such as Paul Samuelson and Franco Modigliani, chose to label his theorem the Pasinetti Paradox, since it struck at the heart of their own theory. Thereafter it has become customary to speak of the Kaldor–Pasinetti model of growth and distribution. We shall discuss in detail below the various strands of analysis that this theorem has originated.

A few years later, in 1965, one of Samuelson's pupils, David Levhari, published a paper claiming to show that some of the paradoxes in capital theory—that had emerged in the literature and had been brought into full light by Sraffa's famous 1960 book, *Production of Commodities by Means of Commodities*—only had limited validity. It was left to Pasinetti to prove conclusively that there were no grounds for such an attack on Sraffa and on the Cambridge view. Pasinetti's paper was first presented in 1965 in Rome at the First World Congress of the Econometric Society. It would then open a special issue of the *Quarterly Journal of Economics* (see Pasinetti 1966), where Levhari's theorem was acknowledged to be false by the author and by Samuelson himself. It was an impressive show-down which honours the economics profession and has since given rise to a full range of insights in the theory of capital (see Pasinetti 1969).

Pasinetti's scientific contributions focus on two main issues: (1) the critique of the neoclassical system; and (2) the completion of the Keynesian system and its extension to distribution, growth, and accumulation. Pasinetti's most significant contribution on both accounts is probably to be found in his 1981 volume on *Structural Change and Economic Growth*. It is one of the outstanding achievements in economic dynamics at the turn of the millennium. Along with the dissatisfaction with aggregate dynamic models and with the deceiving character of a number of disaggregated models (e.g. von Neumann's model), it is the historical fact of the widespread unevenness of development processes—Pasinetti himself argues—that provides the drift to structural dynamics. In the present state of economic growth theory, Pasinetti's contribution has a special place. His system provides the only theory focusing on the conditions for an economic system to reach and maintain full employment and full-capacity utilization in the long run when the system is subject to structural change. Here again, classical economic analysis provides the foundation to Pasinetti's framework. His scholarly approach to classical economics of course encompasses Marx, although Pasinetti has no interest in establishing any canon or dogma on value and distribution. Rather, he aims at building on the open and innovative side of the classical tradition. His scheme of structural economic dynamics focuses on technical progress and human learning and develops a groundbreaking contribution to growth theory.

Pasinetti's research programme spans more than half a century (1959–2015). It has followed a coherent pattern, first outlining the weaknesses of the marginalist or neoclassical model, and then laying step by step the foundations of the reconstruction, on mixed classical-‘pure’ Keynesian bases, of a ‘more general theory’ in order to identify, explain, and analytically recompose the mechanisms and dynamics of modern economic systems. This has been carried out by the use of powerful tools of analysis, in particular the method of vertical integration (Pasinetti 1973) which has opened the field to the understanding of a number of very complicated phenomena, such as the unequal distribution and pace of technical progress, non-linear variations in the composition of demand, the presence of a great variety of types of asymmetric behaviour, the complex role of institutions, and the relevance of the distribution of income and wealth among factors of production and different socio-economic classes.

2 Luigi Pasinetti: A System-Builder

Pasinetti was one of the major contributors to the controversies on capital theory, income distribution, and the theory of value that were fought out between Cambridge, England, and Cambridge, Massachusetts, during the ‘raging’ 1950s,

1960s, and 1970s, with ‘upshots’ in the 1980s and 1990s and even into the twenty-first century. As a less known episode, he also started in the late 1950s a controversy with Robert Solow on the concepts and measures of changes in productivity. All of these controversies were secured by Cambridge, England. It is sometimes argued that—probably as a compensatory move—the losing American side was awarded, over the years, a number of Nobel Prizes for Economics, while the Cambridge, England, Nobel Prize winners (Sir Richard Stone, Amartya Sen, James Meade, and Sir James Mirrlees) were mainly at the margins of the controversies. Despite the strength and the high analytical rigour of those on the English side, as time passed, the ‘Keynesians’ and later the ‘Neo-Keynesian’ schools have progressively lost ground (at least until the financial crash of 2007 when all embraced some form of New Keynesianism). At the same time, the marginalist, or neoclassical, school has taken over: numerically, at least, they have become the dominant school in most of the Western world. In a certain sense the first ‘Keynesian revolution’, led by John Maynard Keynes himself and his pupils of the so-called Circus, had been able to ‘create a break with the past’ and successfully blaze a trail for most of the leading British and American economists and politicians of the 1930s, 1940s, and 1950s. However, when the time was ripe to extend the ‘Keynesian Programme’ to cover issues of the long run, the majority of economists did not follow. The Keynesian revolution seemed to have come to a halt, and its leading members found themselves besieged mainly in Cambridge, England, as well as elsewhere. The so-called neoclassical synthesis had, in the meantime, swept the field (see Pasinetti 2007).

Pasinetti has a distinct clarity of mind and of vision. He has been able to carry out a remarkably unified research programme encompassing a great number of strands. He is one of the best examples of straight arguing in a typically Ricardian style. A friend once wrote of Ricardo at the time that with him ‘the understanding is improved without the temper being ever tried in the discussion’ (Maria Edgeworth quoted in Ricardo 1955: 169). The same can be said of Pasinetti. Much as it happened to Ricardo, however, straightness of mind may sound provocative, especially to vested interests. Unsympathetic critics have frequently taken Pasinetti to task. Among these were eminent economists such as Paul Samuelson, Franco Modigliani, Robert Solow, James Tobin, James Meade, Joseph Stiglitz, Nicholas Stern, and Christopher Bliss.

At the time of writing (April 2015), Pasinetti’s scientific output consisted of 130-odd papers mainly published in international journals; to these must be added four groundbreaking volumes published by Cambridge University Press (see references) and eight other volumes and seven edited volumes with the best presses and usually translated into several languages. His scientific programme may be subdivided into seven separate but strongly interrelated

lines of research (see also Porta 2007). They represent major contributions in the fields of the theory of structural change, growth, value, income distribution, capital accumulation, and technological change. More precisely, as pointed out in Baranzini and Harcourt (1993):

1. On the measurement of productivity changes, that is, the first ‘two Cambridges controversy’.
2. On Ricardo and classical political economy, in particular the Ricardian model of growth and distribution.
3. The second ‘two Cambridges controversy’: on profit determination, income and wealth distribution, and capital accumulation.
4. The third ‘two Cambridges controversy’, on capital theory.
5. On structural dynamics and vertical integration, that is, on the formulation of the concept of vertically integrated sectors and their relation to Sraffa’s subsystems.
6. On the pure labour theory of value.
7. On the roots of the present financial crisis, that is, the Modigliani–Miller Theorem.

We shall now consider these issues within the context of the Cambridge School of Economics as a whole.

2.1 Pasinetti on the Measurement of Productivity Changes

During his stay at Harvard in 1957–1958, Pasinetti wrote, under the supervision of Wassily Leontief, an essay ‘On Concepts and Measures of Changes in Productivity’, later to be published in the *Review of Economics and Statistics* in 1959. It was the first controversy that he stirred, since it led Robert Solow, at Massachusetts Institute of Technology (MIT), to write a long ‘Comment’ trying to rebuke Pasinetti’s analysis (Solow 1959: 282–285). Pasinetti expressed a deep dissatisfaction with the way in which the marginalist school treated technical progress in general, and Solow in particular in his famous paper ‘Technical Change and the Aggregate Production Function’ (Solow 1957), which had appeared in the *Review of Economics and Statistics* in 1957. Pasinetti, who in this context asserts the prominence of technical progress over capital accumulation, notes that: ‘[T]he whole neo-classical movement and the increasing modern application of mathematics, which have contributed so much to improving the tools of economic analysis and to conferring rigor and definiteness on economic thought, have preferred to leave technical progress aside’ (Pasinetti 1959: 270).

We may recall that it was in the 1950s and 1960s that the rate of growth and the role of technical progress turned out to be so important in most industrialized countries. It is not surprising that Pasinetti goes on adding that:

The trouble is that technical changes are hard variables to deal with in analytical terms. The statistical data which have appeared in the last few decades have come as quite a surprise to economists, because their theories, from the Ricardian to the neo-classical, did not lead them to expect such results. I refer in particular to some statistical evidence for the United States and for other ‘capitalistic’ countries which has shown that, on the average and in the long run, shares of labor and capital in the national product have not changed very much, that wages have not remained at the subsistence level but have risen in proportion to national income, that capital per man has indeed increased but output per man has also increased in proportion, that the rate of remuneration of capital has remained almost constant (*ibid.*).

He adds:

There have been some attempts by economists to complete these evaluations and to introduce capital into the picture, by making use of theoretical notions like the production function, but these attempts—in the writer’s opinion—have neglected an important characteristic of capital—that it is reproducible and that its process of production is also subject to technical change (*ibid.*).

The question of technical progress and composition of demand has always been the thread along which Pasinetti has developed his research programme. In 1974, he wrote:

[In] my own discussion with Solow...I have pointed out that, according to Solow’s own findings, the aggregate capital-output ratio in the U.S. economy was lower in 1949 than in 1909. It could therefore be argued that, during that period, the overall capital intensity of the U.S. production processes, very far from increasing (as Solow’s ‘moving along the production function’ would suggest), has in fact decreased (Pasinetti 1974: 92, fn. 15).

2.2 Pasinetti on Ricardo

Pasinetti’s (1960) early work on Ricardo shows his mastery of the inter-connections between value, distribution, and growth. The paper is an outstanding classic in the history of economic analysis. Its principal object

was to show how an analytical model could capture the ingredients of Ricardo's system and (re)produce his results. The model contained the essence of Ricardo's theory of value—that it was principally embodied labour which in practice determined the natural exchange ratios of reproducible commodities. It also highlighted Ricardo's own stress on persistent and permanent or dominant factors at work in the economy which expressed themselves in the forces which determined natural prices. The short-term factors associated with supply and demand and the determination of market prices were relegated to a secondary position. No more so was this the case than in Ricardo's theory of the natural wage and changes in population cum labour force, with which was associated his theory of accumulation. In Pasinetti's model of Ricardo's system, the Malthusian principle of population, which Ricardo adopted, is treated as though it works instantaneously so that the wage is *always* at its natural level even though accumulation is occurring. (As with Ricardo, Pasinetti does *not* suppose the natural wage to be a physiologically determined subsistence wage; habit and history also influence its size.) This simplification allows a much more clear-cut picture of the accumulation process and of the approach to the stationary state.

We may recall that a number of authors (John Hicks, Samuel Hollander, and Carlo Casarosa) had in the late 1970s and early 1980s suggested a model of the Ricardian theory of distribution and economic growth which, contrary to the Sraffa–Pasinetti interpretation, emphasized the forces of supply and demand for the determination of the wage rate. This version has been challenged by Pasinetti, for whom the interpretation of the Ricardian theory of distribution and growth followed by the above-mentioned scholars simply belongs to a stream of economic thought going back to Marshall's interpretation of Ricardo. Such an interpretation tries to incorporate the basic propositions of Ricardo into the analytical framework based on the working of supply and demand. The pre-Sraffa economic literature included several attempts (Marshall's being one of the better known) written with the purpose of reconciling Ricardo with marginal economic theory.

2.3 Pasinetti on Income Distribution and Growth

Pasinetti's economics has a constant focus on *economic dynamics*, or rather (more precisely) on growth and income distribution. His works in the field have generated an extensive literature in scholarly journals, plus numerous

books and a ‘must’ reference in a large number of textbooks on economic analysis and growth (see Baranzini and Mirante 2013; Baranzini 1991). His 1962 seminal paper, which set the tone, provided a constructive critique of Kaldor’s growth model, published in 1956, where there exist two different and positive saving rates, one for the workers and one for the capitalists or entrepreneurs. Pasinetti (1962) shows that the equilibrium rate of profits is totally independent of the saving behaviour of the working class; it is determined only by the saving rate of the pure capitalists (s_c) and by the rate of growth of the system (n). His work thus established the so-called Cambridge equation, spelling out the conditions whereby the rate of growth is coincident with the profit rate. In Pasinetti’s model, the rate of profits is independent of the production function and of the capital-output ratio. This was a smart, elegant, impressive result: ‘[I]magine now the general surprise’, Pasinetti (2012: 1,438–1,439) recalled, ‘when in preparing his PhD [Cambridge] dissertation, the present author discovered that the “Cambridge equation”... remains valid...in general! ... This result had a devastating effect [and it] created much discomfort ... Paul Samuelson and Franco Modigliani [at MIT] took up the challenge and reacted’: they produced a ‘dual’ theorem, ‘which they called the Anti-Pasinetti theorem’ ... ‘Its sole function was that of protecting mainstream economics from a debacle, by giving the impression that it could provide an alternative to the Cambridge equation’.

In this way, on the basis of the Pasinetti Theorem (i.e. the Cambridge equation), the Cambridge (or Post-Keynesian) School was in a position to:

1. Provide a solution to the famous Harrod–Domar dilemma by specifying an aggregate saving ratio determined by the exogenously given rate of growth of population, capital-output ratio, and the entrepreneurs’ propensity to save;
2. Determine the long-period equilibrium value of the rate of profits, the distribution of income between profits and wages, and the distribution of disposable income between classes;
3. Allow for the existence of an income residual (very much in line with classical and neo-Ricardian models), namely the wages consistent with the assumption of a relationship between the savings of that class of individuals (the capitalists or entrepreneurs) who determine the process of production and the patterns of capital accumulation;
4. Provide new insights into the process of accumulation of capital by specifying the equilibrium shares of capital for the two or more socio-economic classes.

On Kaldor’s and Pasinetti’s original contributions via their Cambridge equation, Pasinetti (*ibid.*: 1,440) wrote:

The importance of this outcome goes well beyond Kaldor's attempt at applying it to the working of a capitalist economy. The principle of income distribution that emerges from this theory really becomes much more important than Kaldor himself thought, because it belongs to that level of investigation that I have called 'natural' (i.e. normative). At such a basic level of investigation, the Cambridge Equation is independent of the institutional set-up of the society that is considered.

The above range of results was obtained by Pasinetti (1962, 1974: Chapter 6) with the use of a fairly simple framework and on the basis of relatively few assumptions, much less 'hybrid, opposite and extreme' (Pasinetti 1974: 125) than those of the neoclassical model. In fact, the Kaldor-Pasinetti model gave rise to whole stream of literature including a flood of subsequent contributions branching out in various directions and covering many aspects of the wider research programme relevant for the general topic of income distribution, profit determination, and capital accumulation, both from a theoretical and applied point of view. Among the research lines grafted onto the model, we may distinguish at least the following six:

1. *The introduction of a differentiated rate of return on savings for the two or more classes.* This hypothesis has generated a huge literature since the late 1960s with contributions by Neil Fullerton Laing, Richard Kahn, and Geoffrey Harcourt. In general, it has confirmed the validity of the Cambridge equation. (On this point, see Baranzini and Mirante 2013: 292–295.)
2. *The introduction of the monetary sector and of portfolio choice.* The introduction of the monetary sector and of portfolio choice (where groups of individuals or classes may choose among different assets with different rates of return and different variances) has the merit of bringing together the 'objectivist' and 'subjectivist' Keynesian research programme. Additionally, stochastic models including portfolio choice, under fairly general conditions, confirm the polarization of different socio-economic classes, with a very different rate of wealth accumulation (see, for instance, *ibid.*: 295–298).
3. *Stability analysis and the long-term properties of the model.* The Kaldor-Pasinetti model in general displays good local and global stability. Numerous scholars have considered this issue (for a review see, for instance, Baranzini 1991: 60). As was to be anticipated, the adjustment time required to arrive at the steady-state solution (or to return in the case of external shocks or disturbances) is, in general, quite long. However, we should not forget that the scope of comparative static analysis is not primarily to consider the conditions under which the system may converge towards, or may be deviated from, its long-term steady-state growth path, but instead to inquire into the

mechanisms that, under general conditions, are bound to influence and determine the distribution of income and wealth (on this see *ibid.*: 61).

4. *The introduction of a public sector.* It may be shown that the equilibrium rate of profits is determined by the natural rate of growth divided by the capitalists' propensities to save (as from the 'Cambridge equation'), here 'modified' by the effects of taxation of profits and of government deficit (or surplus) spending.
5. *The introduction of micro-foundations into the model.* The propensities to save of the various socio-economic classes are not simply exogenously given but are a function of economic, demographic, and institutional parameters of the system. In this way, the historical aspects of savings and capital accumulation are brought to the forefront.
6. *The distribution of wealth and the income shares of the economic classes.* Various studies have tried to determine the share of income and wealth (both intergenerational and life cycle) accruing to each class of the system. This aspect is important since the seminal works focused on the distribution accruing to the factors of production on the basis of the value of the equilibrium rate of profits. At least ten scholars (for a list see *ibid.*: 65) have formulated models where the savings of the classes are determined by the kind of income earned and not by the class status. This of course tends to weaken the assumption of two constant propensities to save related to the existence of homogeneous socio-economic classes and introduces the possibility of a more flexible approach to the treatment of savings. The extreme step in this direction is represented by the case in which the saving ratio becomes endogenously given and is conditioned by the value of a number of parameters and variables. Then the analytical solutions obtained will depend on the framework of analysis chosen, that is, on the assumption or rejection of a neoclassical production function and on the constancy or flexibility of the capital-output ratio. (On the relative weights of the life cycle and intergenerational capital stock of the socio-economic classes see, for instance, *ibid.*: Chapters 5, 6, 7, and 8.)

2.4 Pasinetti on Capital Theory

It was in the 1950s that economists first came to doubt that it could no longer be taken for granted that there is a unique, unambiguous profitability ranking of production techniques in terms of physical capital intensity along the scale variation of the rate of profits (Pasinetti and Scazzieri 1987). According to Pasinetti:

Up to the 1966 QJE, many skirmishes had indeed taken place, especially between Joan Robinson and Bob Solow—rather harsh in tones, but hardly convincing in substance. The real point of departure of the controversy was provided by the last chapter—XII: ‘Switch in methods of production’—of Piero Sraffa’s (1960) book, where it was shown that the same method of production can become the most profitable one of many different levels of the wage-rate/rate-of-profit distribution curve, thus contradicting the neoclassical basic contention of a monotonic inverse relationship between capital and its ‘price’ (the rate of profits) (Pasinetti 2003: 227).

In the early 1960s, Paul Samuelson with a number of fellow economists started looking for the conditions that would ensure a strictly monotonic relation between the rate of profits and capital intensity (capital-labour ratio) even in the presence of a non-linear relation between the wage rate and the rate of profits. As already noted, this was claimed to have been found by a pupil of Paul Samuelson, David Levhari, in an article published in 1965 in the *Quarterly Journal of Economics*. Levhari claimed to have shown that while reswitching and capital reversing could occur in an industry, they could not occur in the economy as a whole. It was left to Pasinetti (1966, 1969) to prove otherwise.

Pasinetti’s 1969 paper was attacked, especially by Robert Solow, Christopher Dougherty, John Craven, Christopher Bliss, and Avinash Dixit. One main point remains unchallenged to the present day and that is the disjunction between the ‘profit-wage rate’ relationship and the ‘technique of production-profit rate’ relationship. More specifically, slight changes in the rate of profits may lead to sharp variations in the choice of techniques of production in terms of capital intensity (per man). This is a property of capitalist economic systems, first pointed out by Pasinetti, which has never been challenged. The implications are far reaching: for the price system variations may be quite small, but for the choice of techniques and the distribution of income such variations will be much greater. This is what Pasinetti adds to his 2003 paper on the ‘Cambridge Capital Controversies’:

A vast literature followed. Attempts were made to minimize the shattering effects of reswitching and capital reversing on neoclassical models, by focusing on an intertemporal general equilibrium scheme, where reswitching and capital reversing can be hidden or confused with many other difficulties and causes of instability of the solutions. Yet, even among the general equilibrium theorists, one fact remains undisputed as a result of the 1966 QJE symposium, namely that the relationship between capital—whatever the way in which it is measured or aggregated (Fisher 1971)—and its ‘factor price’ is in general a non-monotonic relation.

This characteristic is contrary to the assumptions underlying neoclassical capital theory, including the recent models on endogenous growth (Pasinetti 2003: 228).

This is what he concludes:

Samuelson closed the 1966 *Symposium* with a few wise words (p. 583): ‘If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life.’ Current neoclassical economists do not seem to like Samuelson’s ‘facts of life’. True, we have been told by Thomas Kuhn... that ‘normal science’ must sometimes put up with black spots like inconsistencies and adverse evidence, ignoring them temporarily and waiting for further investigations. But the nonmonotonic adverse relation between capital intensity and rate of profit seems to be more than a mere black spot. Indeed, Sraffa thought that it could swallow up the whole neoclassical theory of capital—a ‘black hole’, we might say, rather than a black spot (ibid.).

2.5 Pasinetti on Structural Dynamics and Vertical Integration

Pasinetti’s research programme on the ‘long-term evolution of industrial economic systems’ (Pasinetti 1981: xi) was originated, as he wrote, by ‘[a] combination of three factors—one factual and two theoretical’. In his own words:

The factual element was provided by the extremely uneven development—from sector to sector, from region to region—of the environment in which I lived (post-war Europe) at the time I began my training in economics. The two theoretical factors are represented by the two types of theories—specifically the macro-dynamic models of economic growth and input–output analysis... Both the macro-dynamic growth models and input–output analysis impressed me at the time; but they left me profoundly dissatisfied when I tried to use them in order to understand what was going on in economic systems with a very high degree of dynamism, i.e. of technical progress. And I began to think that an attempt might be made to develop a theoretical scheme which, while retaining the analytical character of input–output analysis, could also deal with uneven increases in productivity, in the way the macro-dynamic models had begun to do, but only for the very simplified case of a one-commodity world (ibid.).

Pasinetti’s emphasis on economic dynamics also implies a special constant focus on technical progress. Again in his 2007 book on the Cambridge

Keynesians, he stresses the '[n]ecessity of finding an appropriate analytical framework for dealing with technical change and economic growth' (Pasinetti 2007: 232). The trio of factors just mentioned lie at the core of Pasinetti's magnum opus on structural dynamics (Pasinetti 1981). That is a work that provides the *omega point*, as it were, of his whole research programme and gives the flavour and the gist of the fundamental Smithian inspiration of his own brand of enriched classicism in economics. The book has as a notable subtitle: 'A Theoretical Essay on the Dynamics of the Wealth of Nations'. The vertical integration approach provides a way of representing the dynamics of the economic system that has been crucial for the development of economic analysis since its early application in Smith's *Wealth of Nations*:

[T]he concept of vertical economic structure, in its dynamic applications, has been mainly associated with the analysis of technological requirements for economic expansion (Smith), or with the technological interrelatedness in its implications for the diffusion of particular impulses (Pigou). On the other hand, vertical integration has been linked with the autonomous role of effective demand on the dynamic path (Malthus and Keynes). The analytical formulation introduced by Pasinetti provides a logical framework in which both technological and demand conditions may be integrated in order to give a comprehensive interpretation of the dynamics of the 'wealth of nations', concerning both its absolute level and possible changes in its composition (structural economic dynamics) (Baranzini and Scazzieri 1990: 299).

Step by step, Pasinetti's work became a 'theoretical essay on the dynamics of industrial systems' (Pasinetti 1981: xi). It was only with his 1973 article in *Metroeconomica* on vertical integration that Pasinetti was able to establish the link with Sraffa's analysis and to work out on its basis, without any loss of generality, all analytical inter-industry connections, while at the same time bringing out the correct hierarchy within the Classical School between a Smithian vertical view and a Ricardian horizontal approach:

Pasinetti introduces a vertical integration of production processes that allows a reclassification of the inter-industry commodity flows so that the intermediate input requirements for each produced commodity are reduced to the corresponding quantity of labour input and to a certain residual quantity of physical means of production. This procedure can be performed more than once so that eventually one is left with a negligible residual of physical means of production and with the quantity of labour input directly and indirectly required to produce the commodity under consideration. In the particular case in which the starting point is provided by a production technology, in which commodities are produced by means of labour and other produced commodities, one is left with an economic system of the 'pure labour type' (Baranzini and Scazzieri 1990: 241).

In this case, the stock of each particular intermediate commodity (performing the role of capital goods) is also split into distinct components depending on the final use with which they may be associated. As a result the overall economic system ends up being split into as many subsystems as there are final uses. According to this framework of analysis, the production process is represented in its purest form: men produce commodities by means of their activity alone, and the produced commodities are used only for their personal consumption; no intermediate goods or services need to be taken into account. That is why Pasinetti is led to produce a further outline of his approach in his 1993 book, where the ‘economic consequences of human learning are explored’ in the framework of structural dynamics: here the *pure labour economy* becomes dominant. ‘In the end, I became convinced that indeed the “pure labour economy” takes in classical and Keynesian economics the place that the “pure exchange economy” has taken in Walrasian (or...in neo-classical) economic theory’ (Pasinetti 1993: xiv).

No doubt, however, the same ‘one way’ picture of the overall technology may be maintained if the procedure of vertical integration, which has been outlined above, is applied to a production technology in which commodities are produced by means of labour and other commodities (labour and intermediate inputs). For, in this case, the produced commodities that may be used as inputs in each other’s production are reclassified depending on the type of *final* consumption good in whose production they are indirectly used (e.g. the corn used as an input for the production of machine tools to be used in the textile sector will now appear as a quantity of corn directly used in the textile sector). This ingenious device put forward by Pasinetti allows economic analysis to ‘break’ the horizontal structure of the ‘circular flow’, and brings forth the unidirectionality underlying the production process. Once this is achieved, only one other logical step is required to obtain an increasingly smaller residual of produced inputs by iteration of the vertical integration procedure outlined above.

Pasinetti’s (1973) contribution to the study of vertical integration outlines in a rigorous way the logic of unidirectionality in the specification of economic structure. We may say that the exclusion, in Pasinetti’s analysis, of intermediate commodities provides a sort of ‘shortcut’ for a direct link between the technological conditions determining the ‘productive power’ of the economic system (as influenced by technical progress) and the level and composition of effective demand. Additionally, vertical integration allows for the handling of technical progress without unsettling the relationship between changes in the productive potentiality of the economic system and the level of disposable income. The latter in fact determines the level and composition of effective demand which is so crucial in Keynesian macroeconomic models.

2.6 Pasinetti on the Pure Labour Theory of Value

Pasinetti (1986) argues that the exchange-production duality may be traced back to a deeper dichotomy in the theories of value. According to him, many important insights are to be gained from the pre-theoretical stages of the discussions on value, where diverging principles become acceptable being considered within a normative framework. Pasinetti identifies in Smith and William Stanley Jevons, respectively, the reference points for discussing two alternative paradigms: (1) the *pure-preference model* (or pure-exchange model), which looks at all economic problems as if they were problems of the optimum allocation of resources, and where prices are determined by individual's preferences and resources endowment; (2) a *pure labour economy model*, where production and exchange are inherently linked by labour specialization and where prices emerge from the necessity of an extensive division of labour. In particular, Pasinetti stresses the importance for each paradigm of developing its basic structure by concentrating on one single principle. Pasinetti maintains that alternative visions of the economic system may be associated with different aims of analysis, each of them with a large degree of autonomy.

Starting from this analytical scheme, Pasinetti puts forward a 'pure' labour theory of value and distribution. He first introduced a set of 'newly defined sub-systems', much more comprehensive than those considered in Pasinetti (1973, 1981: Chapter 7) since 'they include not only the labour and the means of production for the reproduction of each subsystem, but *also* the labour and the means of production necessary to its *expansion* at its particular rate of growth ($g+r$)' (Pasinetti 1988: 126–127; italics in original). By additionally assuming that (1) the rate of growth of these 'newly defined sub-systems' may be different (due to a different rate of growth of technical progress and changes in the level and/or composition of demand), (2) there will be a particular natural rate of profits for each hyper subsystem (where a natural rate of profits is defined as a rate of profits which is equal to the rate of growth of demand for the corresponding consumption good), and (3) defining by $l^{(i)}$ the vector of the vertically hyper-integrated labour coefficient for commodity i , Pasinetti (ibid.: 129) obtains the specific set of natural prices $p^{(i)} = l^{(i)} w$, where w is the wage rate. This result is remarkable since, as Pasinetti himself points out, it is a complete generalization of the pure labour theory of value; in fact, each physical quantity of any consumption good is shown to be unambiguously related to a physical quantity of labour. What is more important is that 'the two [i.e. physical quantities of consumption and of labour] have, in between them, a physically defined self-replacing, and expanding, circular process' (ibid.: 130).

Not surprisingly, Pasinetti's analysis reveals a strong intellectual sympathy with Smith (see Porta 1998), first for the representation of the productive system as a set of vertically integrated sectors and second for the associated concept (common to both Pasinetti and Smith) that *labour* may be considered as the ultimate source of wealth. So it happens that Pasinetti's 1993 volume (Pasinetti 1993), as mentioned above, offers a theoretical investigation of the development through time, as a consequence of human learning, of a 'pure labour economy', that is to say, of an economy in which production activity is carried out by labour alone—'labour unassisted by any intermediate commodity' (ibid.: xiii) (as the author himself defines it). Economists have known for a long time the two basic phenomena at the root of the long-term movements of our industrial societies: capital accumulation and technical progress. But, according to Pasinetti, the privileged position has always been given to capital accumulation. In Pasinetti's 1993 volume, this approach is reversed, and technical progress is assigned the central role. Within a multisector framework, he first describes (against a background of 'natural relations') the structural dynamics of prices, of production, and of employment (implied by differentiated rates of productivity growth and of expansion of demand); he then discusses a whole series of problems that arise at the institutional level. According to Pasinetti, personal and social learning, know-how, and diffusion of information emerge as the fundamental factors accounting for the features and fate of industrial societies: the source of their wealth, but also the source of their troubles. The 'pure labour theory of value' allows Pasinetti to shift the theory of long-term economic development from a traditional framework based on capital accumulation to new foundations based on learning, technical progress, and diffusion of knowledge.

2.7 Pasinetti on the Roots of the Present Financial Crisis, that is, the Modigliani–Miller Theorem

Recently, Pasinetti (2012: 1,440) has drawn from his own framework an interesting interpretation of the financial crisis which started in 2007–2008. He states: 'It is a crude fact that, from then [i.e. the appearance of the Modigliani-Miller theorem in 1958] onwards, the dominant theory on savings, investment and growth has sharply changed track', given by the so-called Modigliani–Miller theorem. For Pasinetti, the Modigliani–Miller Theorem contributed 'to generating a dramatic shift in economic, monetary and financial policies over the past few decades (ibid.)'. He continues:

One may say that the Modigliani-Miller theorem demonstrates, under certain, specific, ideal conditions...that the market value of a corporation does not depend on its financial structure: in short, it does not depend on its debts to assets ratio. The theorem has in this way led theorists (and financial operators!) to believe that increasing indebtedness has no counter-indications, without considering that an absolutely necessary assumption for reaching such a conclusion is that corporations must always remain in the same category of risk, which of course is impossible if the corporation's indebtedness increases. In even simpler, immediate and practical words, the theorem has led to the belief that there is no difference between the two traditionally considered alternatives regarding the allocation of each single firm's profits, i.e.: (i) that of using them internally by adding them to the existing capital *stock*, or (ii) that of immediately distributing the dividends to the shareholders (ibid.: 1,442; italics in original).

Pasinetti's conclusion is straightforward. For the Classical and Post-Keynesian schools, the traditional role of the 'capitalists' or 'entrepreneurs' is to reinvest profits, not to distribute them. In the Post-Keynesian model, new investment must ensure full employment of the system, the absorption of net new labour, and the generation of 'that process of *capital accumulation* that characterizes our capitalist economies' (ibid.; italics added). That is why, for Pasinetti,

The Modigliani-Miller theorem came to introduce a sharp break with the whole Classical—and by implication Keynesian—approach. It channelled all economic arguments to a supposedly micro-economic process founded on the rational behaviour of the single economic agents, in the search of efficiency for the corporation's investments, internal or external, *in the belief that* efficiency was associated, not so much with the traditional maximization of profits, since this could generate complications in regimes of uncertainties, but with the maximization of the corporations' market value (i.e. of the value of the corporations' shares, as evaluated on the stock exchange market). All this of course within a cultural context of supposedly perfect knowledge, perfect communication of information, and an institutional framework of perfectly automatic self regulating free markets (ibid.: 1,442–1,443; italics added).

In this way, Pasinetti's analysis comes full circle. It started in the mid-1950s from the argument that the industrialized world was then, and still is, characterized by a highly unequal distribution of income and wealth, and fluctuating unemployment often in the presence of excessive production capacity. After nearly six decades as an economist, Pasinetti thus points out an additional failure of mainstream economics.

3 Conclusion

In September 2012, the Meiji University of Tokyo, at the initiative of Professor Takashi Yagi, convened a major international entitled *Conference on Structural Economic Dynamics*, where Luigi Pasinetti was the guest of honour. Immediately after, Roberto Scazzieri and Michelle Baddeley were in Cambridge, England, the convenors of a conference in honour of Pasinetti on *The Economics of Structural Change: Theory, Institutions and Policies*, which was sponsored by the Cambridge Political Economy Society. (We may recall that Pasinetti, with Amartya Sen, has been a patron of the *Cambridge Journal of Economics* since its foundation in the late 1970s, Richard Goodwin having died in 1996.) The latter conference took place at Luigi's alma mater, Gonville and Caius College. Twenty-eight scholars from all over the world contributed. The aim of the conference was 'to provide a critical assessment of current research in the economics of structural change from a theoretical, institutional and policy perspective' (Baddeley and Scazzieri 2012: 2). According to the two convenors of the conference:

The lifetime work of Luigi Pasinetti, who has co-founded the Cambridge analytical tradition through a unique combination of the Classical and Keynesian lines, has highlighted structural change as the most important feature of modern economies since the first industrial revolution and has addressed the relationship between macroeconomic aggregates and changes in sectoral proportions as the most distinctive feature of long-term dynamics. His [works] are path breaking constructive contributions to the theory of a multisectoral economy evolving through time under the influence of technical progress and changes in final demand composition (ibid.: 1–2).

It is important to emphasize that Pasinetti has drawn attention to the dichotomy between 'natural models', which describe the very basic relationships among variables, and 'institutional models', where different institutional set-ups come into play. This highlights his own drift to providing new frameworks for the explanation of the mechanisms at the basis of modern economic systems. Pasinetti's 2007 work, especially Book 3, 'Towards a Production Paradigm for an Expanding Economy', is of great relevance here (Pasinetti 2007; see also Porta and Scazzieri 2008; Arena and Porta 2012): it is largely based on Pasinetti's 'Separation Theorem', stating that 'we must make it possible to disengage those investigations that concern the foundational bases of economic relations—to be detected at a strictly essential

level of basic economic analysis—from those investigations that must be carried out at the level of the actual economic institutions’ (Pasinetti *ibid.*: 275). Investigation of the former type considers fundamental economic relationships detected independently of specific behavioural patterns and institutional set-ups. This level of investigation, which Pasinetti calls ‘natural’, allows the determination of economic magnitudes ‘at a level which is so fundamental as to allow us to investigate them independently of the rules of individual and social behaviour to be chosen in order to achieve them’. For him, this stage of analysis identifies the domain of pure economic theory and should be kept distinct from ‘a second stage of investigation, which concerns how the economic magnitudes are actually determined, within the bounds and constraints of the institutions characterising the economy at the time it is investigated’ (*ibid.*) (See also Scazzieri 1996 and Teixeira 1998.)

It is worth recalling that Pasinetti today blazes a highly enlightening trail concerning the reconstruction of Sraffa’s intellectual development, taking the vast array of Sraffa’s unpublished papers into account: this analysis is so deeply interlaced with Pasinetti’s analytical framework that it becomes one of its main pillars (see *ibid.*: 172–194; see also Porta 2012a, b). In addition, this brings out his straightforward critical stance against some of the ideological sins spoiling the heritage of the Cambridge School (see his interlude on ‘Unwise Behaviour’ in Pasinetti 2007: 199–204).

It is of some interest to note that, after many years and controversies, it is today no less an authority than Robert Solow who pays tribute to Pasinetti for exploring territories into which the theory of growth has surprisingly not expanded: ‘As Solow points out, “Luigi Pasinetti has written extensively on the sort of structural changes to be expected along a trajectory, arising from such inevitable factors as differing income elasticities of demand for different goods”. The solid realism of Solow’s timely remark is confirmed also by the fact that this is the *only place* where Pasinetti’s name—undoubtedly the greatest economist alive to have focused attention on structural dynamics today—finds mention in the entire two-volume *Handbook [of Economic Growth]* of some 1,800 pages’ (Arena and Porta 2012: 5; italics in original).

Pasinetti’s economics has the breadth of the long run, successfully combining history of facts, history of ideas, and new conceptual paradigms. Since the late 1950s, he has been one of the most influential economists of the Cambridge Post-Keynesian School of Economics, has maintained the classical British tradition of economic thought at the frontier of economic research, and has been outstanding as a ‘tool-maker’ rather than a ‘tool-user’, much in the same vein as Kaldor and Hicks. His Separation Theorem

shows that his views are firmly grounded in an interdisciplinary perspective, which shuns the current mood of endogenizing all aspects of human life under the umbrella of self-interested behaviour. He is a real and true pupil of Keynes in this, and he is among those who bring out the best of the Cambridge heritage in economics.

In the wake of the present moral and economic crisis of Western capitalism, economists are now giving signals of a return to being interested in income distribution and economic dynamics. Indeed, Pasinetti's experience, as the senior living heir of the Cambridge tradition, has much to teach young researchers, increasingly disenchanted with the platitudes of the mainstream. Luigi Pasinetti is one of those great minds whose contributions cannot be ignored.

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G.C. Harcourt (1931–)

K. Vela Velupillai

1 *The Thirteenth Tribe: Harcourt's Humane Visions*¹

We all know that Alfred Marshall said that the Mecca of economists was biology even though he was never able to solve his own dilemma of theorizing in terms of concepts derived from classical physics while having a ‘vision’ of capitalism as an evolving organic system (Harcourt 2011: 263).

Geoff Harcourt’s visions were, ultimately, grounded in the humane concerns that characterized Hume and Smith, Ricardo and Wicksell, and his near Oxbridge contemporaries, Hicks and Dobb. Harcourt² is a scholar

¹I have chosen the titles of Arthur Koestler’s various books as the headings of the first four sections of this ultra-brief essay on Geoff Harcourt’s extraordinarily deep, sustained, and prolific contributions to economics. The concluding section should have been titled ‘Arrival and Departure’—but is not, due to the needs to comply with the publisher’s convention. However, nothing in this essay implies any endorsement of Koestler’s changing and controversial views and philosophies, nor is there any implication that Harcourt approves either my choice of Koestler-based headings, or that he adheres to the changing ideological underpinnings of Koestler.

²I shall, in the rest of this chapter, refer to Harcourt as GCH, for simplicity, and Post-Keynesian Economics, for the same reason, as PKE. GCH writes faster than mortals like me can read (certainly in one human lifetime). In Harcourt (2014), GCH estimates ‘over 360 articles, etc., and 17 books’; the most

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of impeccable integrity, unrivalled dedication, and admirable courage, all tempered by kindness and generosity, even—perhaps, especially—to those who did not, or could not match him in these noble virtues. That I consider GCH the bearer of the traditions of Hume, Smith, Ricardo, Wicksell, Hicks, and Dobb, is my way of characterizing his wide and deep scholarship of *classical, Marxian, and neoclassical economics*³ and philosophy. There is no doubt whatsoever that he forged, out of the economics of Keynes and Kalecki, a wholly new path of enlightened theory and theoretically sound policy; that he was the scholar par excellence of the deep economic contributions of his Cambridge mentor—Joan Robinson; and that he was the pioneer—at least in my opinion—who fashioned a *Post Keynesian Economics* (PKE), out of the works of these three outstanding economists.⁴

Justice, fairness, equality, and sympathy for the less privileged characterized the economic policy proposals—always grounded in the enlightened economic theory of classical, neoclassical, and PKE—that made GCH an unquestioned inheritor of the noble traditions of the triple triptych. That he enhanced, generalized, and finessed these traditions, with his own *visions*, tempered and determined by the new challenges posed by an increasingly virulent orthodoxy, is a tribute to his intellectual, political, and personal courage, in the face of obdurate adversaries.

recent CV I had access to, sent me by GCH about six months ago (Harcourt 2015a) lists 260 articles, notes, chapters in books and review articles, and thirty-seven books (*not* counting second editions, translations into non-English languages, and reviews)—and this does *not* include his correspondence, reports as an examiner, and letters to newspapers (the latest of which was one published in the *Financial Times* (FT), just a few days ago!). Whether there is a meaningful discrepancy between the two numbers, ‘over 360’ and 297, and between seventeen and thirty-seven books, is, I think, a moot question, especially since a close friend, collaborator, and authority on GCH lists, ‘29 books and over 360 articles and book chapters, not counting numerous notes and non-academic publications’ (Blankenburg 2014: 1,295). Given, therefore, the word constraint, coupled to the more mortal ones of time and *ability*, cannot but imply that this paper can only be an outline of GCH’s amazingly wide, deep, and varied contribution—even *only to economics*.

³ I am well aware that the world of macroeconomics is proverbially many faceted—even without beginning to classify varieties of microeconomics. For the limited purposes of this paper, it is sufficient to work with these four enduring ‘schools’. If ‘push comes to shove’, it is easy to include, for example, New Keynesian or New Classical macroeconomics as subclasses of neoclassical economics. On the other hand, the other, microeconomic, divide, between partial and general equilibrium systems is of relevance in considering, even summarily, GCH’s contribution to economics. This is obviously because he was a dedicated scholar of Marshall and Pigou.

⁴ Had either Keynes or Kalecki displayed the mastery of Marxian or neoclassical economics, respectively, or if only Joan Robinson’s impressive scholarship was ‘tempered by kindness and generosity’, especially to adversaries, I would have had no hesitation to include them in the class of Hume, Smith, Ricardo, Wicksell, Dobb, and Hicks—and, hence, forming the triple triptych of the fountainhead of the core economic philosophy and methodology of GCH’s sympathetic and outstandingly original theoretical and policy framework. GCH himself was greatly influenced, both with regard to economic theory application and policy, and proper behaviour as a teacher, and as a university and Australian citizen, by Eric Russell, his greatest Australian friend and mentor at Adelaide (see Harcourt 1977).

The bare bones of his personal life⁵ (Harcourt 1995a) indicate he was born in Melbourne, Australia, on 27 June 1931, as one of twins,⁶ to Marjorie and Kenneth Harcourt, who were ‘right-wing, assimilationist, agnostic, Jews’, making up a household ‘in the stuffy, snobby, sectarian environment which Melbourne was then’. He, and his brother, experienced ‘anti-semitism⁷ at the private school to which [they] were sent in 1945’.

He, therefore, felt ‘liberated’ that his initial university education was at a Methodist institution, Queen’s College (a constituent college of the University of Melbourne). He married Joan Bartrop, who has graced GCH’s life as his wife, now, for over 60 years, on 30 July 1955. They were blessed with four children, Wendy, Robert, Timothy, and Rebecca, and now, as grandparents to Caterina, Emma Claire Sardoni, and Yunshi and Jhen Huie Harcourt. Wendy’s husband is the distinguished Italian economist, Claudio Sardoni, who carries on the Marx–Keynes–Kalecki⁸ (see Sardoni 2011) aspects of PKE I have come to associate with GCH.

I have to mention that GCH considered Rothschild’s *Price Theory and Oligopoly* (Rothschild 1947), as ‘the single most influential article of [his] undergraduate years’, but was also introduced to Stone (1945)⁹—all 107 pages of it—by ‘Don Cochrane (of Cochrane and Orcutt fame)’, in the fourth year as an undergraduate at Melbourne (Harcourt 2012a: 27). These, coupled with a serious reading of Hicks’s classic on *Value and Capital* and Swan’s—in my opinion—acid review article of Hicks’s *Trade Cycle* book (Swan 1950), gave GCH that grounding in Hicksian economics which made me include the great Oxford economist in the formation of the Cambridge stalwart GCH was to become.

⁵ Much of the material here is available in much greater detail, and placed in the context of the social, political, and religious environment in which GCH lived through his formative years, in Harcourt (1995a, 1999, 2012a).

⁶ He refers (ibid.: 225) to his twin brother John as ‘a distinguished academic dentist’ and one who ‘Keynes would have approved’. This reference to Keynes’s ‘approval’ is, of course, to the famous last lines of Keynes (1930b [1972] 332; italics added): ‘If economists could manage to get themselves thought of as humble competent people, *on a level with dentists*, that would be splendid’. There is no doubt at all, at least in my mind (and I share this view with GCH, I am sure), that Keynes the economist was supremely competent, but humble!

⁷ During a light-hearted conversation in Cambridge, in the early 1980s, GCH told me he may well be the only ‘Methodist-Jew, who was also a socialist’—I was not sure, then, whether the statement was to be taken with an appropriate ‘pinch of salt’!

⁸ MKK, as I refer to this trio, are as uneasy bedfellows as that other, perhaps more famous trio, referred to as MKS (Marx–Keynes–Schumpeter). I am not sure that the popularization of MKS due to the events of 1883—one death and two births—is a cogent reason for finding commonalities.

⁹ Surely, Harcourt and Pesaran (2000) and, partly, Harcourt and Kitson (1993) reflect these early background influences.

GCH was awarded a travelling scholarship to continue doctoral studies at King's College, Cambridge University, from September 1955, where he obtained his PhD, in 1960. His contemporaries, as graduate students in Cambridge in the late 1950s, read like a veritable 'who's who' of economics in the latter part of the twentieth and early twenty-first centuries: Amartya Sen, Luigi Pasinetti, Pierangelo Garegnani, John Whitaker, and a whole host of Australians, of whom I shall only mention Hugh Hudson.

The work and path towards the *applied* work that became his Cambridge doctoral thesis has left, in my opinion, an indelibly felicitous mark on almost all the outstanding *theoretical* works by GCH. It is, therefore, useful to quote him on how this work came about (Harcourt 2006a: 146):

[K]aldor went on leave for a year and I had Ron Henderson [as my supervisor] from then on ... He immediately sent me to London to work at the National Institute for Economic and Social Research (NIESR) on the soon-to-be-published combined accounts of all quoted public companies in the United Kingdom for the period 1949–53 ... The data I examined became the raw inputs into my dissertation, Harcourt (1960). It was, in effect, Mathews and Grant on inflation and company finance... with Joan Robinson's *Accumulation of Capital* (1956) thrown in.

Those 'in the know' will understand now, how and why GCH's two most cited papers, Harcourt (1965a, 1969a), are intimately related. GCH has always walked on two feet—sometimes with theory and applied work in harness; at other times with ethics and economics, or policy and poetry, just as much as he remains an *Australian Patriot and a Cambridge Economist* (Harcourt 1995a)—always well anchored on two feet.

GCH has been the recipient of numerous honorary degrees and even more awards and distinctions—ranging from the D.Litt. at Cambridge to the recent Veblen-Commons Award by the Association for Evolutionary Economics.¹⁰ He is currently a Visiting Professorial Fellow at the University of New South Wales—having been a Lecturer, Senior Lecturer, Reader, Professor, and Professor Emeritus at the University of Adelaide, between 1958 and 1988. At Cambridge he was a 'young' University Lecturer and Fellow and Director of Studies at Trinity Hall in 1964–1966, a Visiting Fellow at Clare Hall in 1972–1973 and, in his later 'incarnation', Fellow and College Lecturer at Jesus College (1982–1998), Emeritus Fellow (1998–) and University Lecturer (1982–1990), Reader (1990–1998) and Emeritus Reader (1998–) at Cambridge University, in 1964–1966 and 1988–1998. In between, he served in the distinguished capac-

¹⁰ See Harcourt (2015a) for a full list of degrees, appointments, and awards.

ity as the President of Jesus College, Cambridge, twice (in 1988–1989 and 1990–1992), equally many times as the Secretary of the Faculty of Economics and Politics, Cambridge University, and eight years on the University Council.

The rest of the chapter¹¹ is structured as follows. In the next section, there is a discussion of what I consider to be four of the great classics to emanate from the fertile pen of GCH. They provide a Golden Bridge between the empirically loose, yet important world of the accountant, and the mythical Kingdom of the Golden Age. In Section 3, a further six classics by GCH, based on five themes, are the subject matter of discussion. Section 4 tries to provide a basis for the type of macroeconomics GCH has made his own. The concluding Section 5 is an attempt—perhaps a lame one—to outline the way GCH has tried to propagate the ideas of the pioneers via his expository and review articles. It tries, also, to pay *homage* to the philosophy of GCH.

The interested or curious reader—that elusive creature—should remember Paul Davidson’s most handsome characterization of GCH (Davidson 1996: 911): “Through [Harcourt’s writings], one finds an author who is indefatigable in fighting the “good fight” for a civilized system, but is also, as Mark Perlman has noted, “unusually courteous” in a world where that virtue is rare’.

There is no doubt he was continuing the Cambridge tradition of ‘fighting the good fight for a civilized system’, but many who did so, at Cambridge, some of whom were his teachers, mentors, colleagues, or friends (and many became all of these, eventually), could not be characterized as being ‘unusually courteous’, even to each other—and, in the case of two celebrated Cambridge economists, by no stretch of the mind, could such courteousness even be imagined.

2 *The Yogi and the Commissar: Meanderings, Exhortations, and Explorations*¹²

‘Harold Lydall...posed for me the puzzle which subsequently gave rise to my best known article (apart from the 1969 survey of capital theory [Harcourt 1969a]), “The Accountant in a Golden Age” [Harcourt 1965a] (Harcourt 1999: 9).

¹¹The caveat given in the last sentence of footnote 1 above is most important.

¹²It must be remembered that *The Sleepwalkers* (heading of the next subsection), *The Ghost in the Machine*, and *Darkness at Noon*, also have readings that are relevant for the fabric out of which I am trying to carve the tapestry of *The Economics of GCH*. The materialist Commissar embodies the accountant, from Petty to Harcourt, via Marx, Hicks, and Lindahl; the idealist dreamer, the Yogi, the mythical Golden Age theorist from Ricardo to Harcourt, via Marshall, Keynes, Kalecki, Sraffa, and Joan Robinson. Myths do not have to be true to be believed; they just have to be constructed consistently, but with imagination—a task that GCH excelled at, as did the triple triptych he based himself on. The pure Koestler and the conscientious

Professor Geoffrey Whittington, a leading researcher in accounting theory, as a sometime colleague at Cambridge, and co-author of GCH, is eminently qualified to state that:

The economist is most likely to associate Geoff Harcourt's name with the capital theory controversies and the development of post-Keynesian economics ... However, he has also made an extremely important contribution to bridging the gap between *economists and accountants* in the related areas of income *measurement* and the *measurement of the rate of return* (Whittington 2007: 92; italics added).

'In the late 1940s in Melbourne', writes GCH (Harcourt 1999: 7), he started studying economics 'as a schoolboy', where one of 'the principal textbooks' was Hicks's *Social Framework*—a book that introduced economics on the basis of the social *accounting* he (Hicks) had learned from Myrdal and Lindahl (Hicks 1977: 143). It is natural, then, for Hicks (1956,¹³ 1982: 221; italics added) to note—as befits one who considered himself 'an accountant among economists' (Klamer 1989)—that 'In all its main forms, modern *economic dynamics* is an *accounting theory*',¹⁴ thus linking *capital theory* and *accounting theory* via dynamics to a *monetary production theory*—a draft title of what eventually became *The General Theory* (Keynes 1973: 381, ff and 409, ff). It is also Hicks, this time with his development of *A Neo-Austrian Theory* (Hicks 1973), who made the 'truncation theorem'¹⁵ an integral part of the Cambridge Controversies in Capital Theory.

As such, we have all the elements of the connection between the Yogi and the Commissar—between capital theory and accounting theory, mediated by

Harcourt were both moved by the prevailing injustices, at both individual and societal levels—hence the constant interplay between ethics and economics, between policy and theory, and between a theory of the empirical and its grounding in a measurement in theory.

¹³ It was to this essay that Tibor Barna (1957: 493) referred in his outstanding review of Robinson (1956), a review which GCH (Harcourt 2012b: 331) also found 'most favourable and knowledgeable'.

¹⁴ Adding (in Hicks 1977: 143; italics in original) that Lindahl 'was the father of Social Accounting theory'.

¹⁵ Nuti (1970), earlier, and Irving Fisher (1907: Chapter 4 and appendix), even before that, had broached this topic most felicitously; the latter did so as a critique of Böhm-Bawerk's third ground for the existence of a positive rate of interest, and thereby made the calculation of the *internal rate of return* (IRR) routine. The uniqueness of the IRR, under certain circumstances, came to dominate an aspect of capital theory (concisely summarized by Sen 1975)—and accounting theory. Incidentally, not only is Chapter III in this characteristically elegant book by Hicks titled *Social Accounting*, but he also declares, unequivocally (ibid.: 13): 'It is the post-Keynesians... who, to their honour, have wrought a classical revival'—a declaration, in his incarnation as simple 'John Hicks', which must gladden GCH quite unreservedly. I myself remember very vividly that GCH went to spend a day with Hicks in May 1973—just about the time *Capital and Time* was officially published.

money and dynamics. Before this unifying aspect is discussed, I have to make one detour, especially because I have mentioned *money*.

Not too many months after he was awarded the Nobel Memorial Prize in Economic Sciences, Prescott (2005: 523; italics added) felt he had the ‘right’—even ‘obligation’—to pronounce upon matters on which he was comprehensively ignorant:

In the 1960s there was the famous Cambridge capital controversy. This controversy bears on the issue ‘What is *money*?’ *The Cambridge capital controversy was a silly one*, as pointed out so clearly by Arrow (1989) ... I emphasize that this does not mean that a model with a single capital good, which is matched to the value of some capital stock statistic, is not useful in drawing scientific inference.¹⁶

Just for Professor Prescott’s educative information, I would like to quote from one of the most recent articles, by GCH, on the ‘Cambridge controversies in capital theory’:

The aspect of the [Cambridge, UK] critique that was most emphasized was set within the context of capital theory. It concerned the meaning and its corollary, the measurement of capital, and its marginal product in an explanation of the distribution of the national product between wages and profits. By ‘meaning’ is meant the concept of capital to be found in the two principal alternative ‘visions’ of how capitalism works: the classical/Marxian ‘vision’...on the one hand, and the principally Fisherian ‘vision’ of the mainstream, on the other (Harcourt 2015b: 244).

¹⁶First of all, Prescott’s interpretation of the meaning of ‘scientific inference’ is as rickety as his knowledge of the ‘Cambridge controversy on capital theory’. Secondly, the first clause in his last sentence is senseless, not only in view of the results in Zambelli (forthcoming), but also due to the central results on the ‘Cambridge controversy in capital theory’, all the way from Robinson (1953–1954) to Samuelson (1966), all summed up neatly in Harcourt (1972). Thirdly, Arrow ‘pointed out’ *nothing of the sort*—on the contrary he chided ‘our so-called new classical friends’ for their absurd assumptions, particularly on market clearing (cf. Arrow *ibid.*: 150). On the other hand, contrary to the views expressed on Arrow in his otherwise rare caustic review of Feiwel (1989) by GCH (Harcourt 1991), my own view is that Arrow was not only seriously apologetic on the merits of general equilibrium theory, but also embarrassingly incorrect on the technical issues of computation, computability theory, and metamathematics—particularly on the algorithmic nature of proof (especially in the kind of mathematics underpinning general equilibrium theory). Obviously, he is unaware that a pure mathematician of impeccable competence, G.H. Hardy (1929: 18; italics in original), had a tempered opinion about the nature of ‘proof’: ‘[T]here is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils ... The image gives us a genuine approximation to the processes of mathematical pedagogy on the one hand and of mathematical discovery on the other; it is only the very unsophisticated outsider who imagines that mathematicians make discoveries by turning the handle of some miraculous machine. Finally the image gives us at any rate a crude picture of Hilbert’s metamathematical proof, the sort of proof which is a *ground* for its conclusion and whose object is to *convince*’.

We now know, or should know, more than 60 years after Robinson (1953–1954), almost 50 years since Harcourt (1969a) and, above all, almost 80 years after Sraffa's enlightening, even frustrated, letter to Joan Robinson (Harcourt and Riach 1997: 131), what the 'Cambridge capital controversy' was about—and, above all, it was *not* 'silly'. It was about aggregation, (functional income) distribution, switches of techniques, capital reversals, and truncation. None of these issues are even remotely considered by Prescott.

Both classics, of 1965 and 1969, one a pioneering study of the eternal link between the accountant and the theorist, the other on the plain of pure theory, have 'stood the test of time' (Harcourt 2012a: 33). To go back to the Yogi and the Commissar, or to capital and accounting theory, their intimate connection comes to the fore via the implication of truncation of production *flows*—and, hence, to dynamics and to the relevant distinction between an internal rate of return (IRR) and an accounting rate of return (ARR).¹⁷ Both of these, themselves, are intrinsically dynamic, fundamentally monetary-inflationary, and expectational phenomena. This is why GCH adheres to the theories in *A Treatise on Money* (Keynes 1930a [1971]) and *The General Theory* (Keynes 1936 [1973]) and to a Kalecki–(Joan) Robinson-dominated PKE.

Incidentally, the Yogi's abode is the ideal—indeed, mythical—world of the Golden Age (Robinson 1956, 1969: 99, ff),¹⁸ of which by far the most lucid account, in the context of the kind of economics that is congenial to GCH, is that by Gautam Mathur (1969)—himself a Cambridge economist and an Indian patriot, if I may paraphrase GCH, and also a contemporary of GCH. It is not without significance that Mathur's (ibid.: 413, italics added) is a 'Revised version of paper (sic!) read at a seminar in May 1985 under the Chairmanship of Professor Geoffrey Harcourt to the members and scholars of Cambridge University, Faculty of Economics and Politics'.¹⁹

¹⁷The criticism by Kay (1976) of Harcourt (1965a) is quite irrelevant from many points of view, some of which are clearly pointed out by Wright (1978: 467–468, especially fn. 4) and Fisher (1984: 510, especially fn. 2). My own criticism of Kay (ibid.) is related to the erroneous remarks he makes on proposition II (ibid.: 450) and the notion of algorithm he invokes (p. 563), apart from the more elementary infelicities on pp. 563–564. Obvious space constraints preclude any further details on these points.

¹⁸In my reading of *The Accumulation of Capital* I found that the concept of the Golden Age was being used before it was formally defined! I must be wrong.

¹⁹The footnote from which this quote is taken goes on to thank, among others, a 'Dr Jayanti Ghosh'! I surmise that this refers to GCH's prize Indian pupil, Jayanti Ghosh, who was instrumental in getting Harcourt and Turnell (2005) published in the influential and important Indian journal, *Economic and Political Weekly* (Harcourt 2012b: 1).

GCH has continued to contribute to various aspects of capital theory, doctrine, historically, methodologically, analytically, and pedagogically, from that initial famous ‘survey’ (Harcourt 1969a), right through to Harcourt (2015b), by way of Cohen and Harcourt (2003),²⁰ for example.

I myself have been fascinated by his two important joint efforts with Vincent Massaro (Harcourt and Massaro 1964a, b) and the appendix to Chapter 4 of Harcourt (1972), on clarifying difficult points in Sraffa (1960). Even here, there is the important interaction between the Yogi and the Commissar, especially with regard to depreciation formulas in Champernowne and Kahn (1953–1954) and Sraffa (*ibid.*). The exposition of Sraffa’s device of *sub-systems* to demonstrate the possibility of reduction to dated quantities of labour, using imaginative diagrams, coupled to algebra, in Harcourt and Massaro (1964b), is most ingenious.²¹

But I would like to end this section with a remark on the continuing fascination for GCH of the accountant’s methodology. In a conversation with Jane Gleeson-White (Harcourt and Gleeson-White 2012), on the role of double-entry bookkeeping in the evolution of capitalism, interestingly discussed in her recent book (Gleeson-White 2011), I detect a greater adherence to Schumpeter than to Marx, in GCH’s vision of the evolution of capitalism—thus justifying the Veblen-Commons Award of 2010, sponsored by the Association for Evolutionary Economics, as if it needed any justification.²² On the other hand, the introduction of double-entry bookkeeping, without the prior, Fibonacci-inspired adoption of the Hindu–Arabic numerals to record the entries, in company, state, and national accounts, would have been a case of the story of Hamlet without the Prince.²³

²⁰This article has, for the last 10 years, been included in my reading list for lectures on advanced macroeconomics. Graduate students at universities in Italy, Ireland, and the USA have found it an exceptionally clear pedagogical introduction to important aspects of the Cambridge controversies in capital theory—and also a model example of writing clearly on intrinsically difficult concepts.

²¹I must confess being one of those who believes Sraffa ‘never makes mistakes; he just makes peculiar assumptions’ (*pace*, Sen 1974). Thus, I am in rare disagreement with GCH(!) in Harcourt (1972: 191) that the tractor example of § 81, in Sraffa (*op. cit.*), is ‘wrong’. In a personal conversation, GCH told me that Sraffa had agreed, at first, that there was a ‘mistake’, but then retracted almost vociferously! This is corroborated by the re-narration of this story in Harcourt (2012a, especially p. 42: ‘The Pope, according to Sraffa, as reported by GCH, is supposed to be ‘infallible’; but in the Saivaite Hindu tradition, Hindu Gods are fallible. In the dialogue between the Sage Naradar and Siva, the latter says to the former: ‘A mistake in composing poetry remains a mistake, *whether you show your divine eye or not*’.)

²²In my opinion, GCH deserved this award on his own; it was unfortunate he had to share it. It is analogous to Hicks and Arrow sharing the 1973 Nobel Prize—they deserved, surely, individual awards.

²³In my course on *The Evolution of Financial Systems*, where Gleeson-White’s book was a significant entry in my recommended reading list, I emphasized this theme, within the context of a Vicovian–Schumpeterian–Braudelian vision of the origins and evolution of a capitalist economy. The kind of evolution I envisaged was very different from any kind of blind adherence to the New Synthesis—that based on the synthesis

3 *The Sleepwalkers: Setting the Stage for a Changing Vision*

The 1960s was an exciting decade to be in Cambridge (Harcourt 2009: 204).

Distribution, growth, employment, money, and pricing, the five defining themes of the economics of GCH, are those that were developed in the changing visions of the basic works of those whose pioneering classics provided the soil for the fertile, sympathetic, generous, and consistently open-ended²⁴ theoretical structures he erected—and has left for posterity.

Distribution in its functional aspects, from Ricardo to Sraffa, *growth* as accumulation due to a judicious choice of techniques, from Smith to Robinson, *employment* stimulated by effective demand and due to a rejection of Say's Law, from his great Jesus College predecessor, Malthus, to his Cambridge apostle, Keynes, *money* (and finance),²⁵ from Marshall and the Cambridge tradition of Pigou, Keynes, and Kahn, and *pricing* in the Kaleckian lineage, were the five building blocks of the economics of GCH. Equilibrium in the short and long periods, expectations (in the sense in which the 'Swedes' of the 1920s and 1930s used anticipations—neither straitjacketed by the strictures of any kind of formal probability theory), returns to scale of many varieties, at many levels of aggregation and an indiscriminate switch between period and continuous analysis at the macroeconomic level and that between algebra and geometry, together with copious arbitrariness on transition dynamics, were those accoutrements to the fundamental five concepts. These accoutrements were the main causes of the paradoxes, perplexities, and confusions that plagued a full appreciation of the basics of the five defining themes.

In that 'exciting decade of the 1960s', GCH produced six of his undisputed and pioneering contributions to macroeconomic—and microeconomic, especially with regard to pricing and market forms—analysis—in

between Darwinian and Mendelian contributions. Marshall's visions were circumscribed by exclusive reliance on the former, reflected also in the epigraph he chose for his magnum opus: *Natura non facit saltum!*

²⁴ There are no *complete* theories, as GCH emphasized the Robinsonian precept, conjoined to Keynes's rule that there is a spectrum of *methodologies* to invoke, in theorizing for policy as an economist, just as there is the spectrum of *philosophies*, to understand 'man's changing vision of the universe'.

²⁵ GCH was 'there', long before the fashionable heterodox tradition of relying on Minsky to have originated that supreme notion of the finance motive, in Keynes. A theorist who 'soiled' his hands in the messy world of company finance, inflation, and depreciation, always coupled money *and* finance, money *with* finance—thus another invisible bridge was forged between Keynes and Kalecki, in the economics of GCH.

Harcourt (1963, 1965b, 1966, 1968,²⁶ 1969b) and Harcourt and Kenyon (1976).²⁷ These six, together with the four classics discussed in the previous section, form the core contributions of GCH to the *changing visions of the economics* that, eventually, became, in his competent and magnanimous ways, *Post Keynesian Economics* (PKE).²⁸

GCH himself considers Harcourt (1965b) his ‘favourite theoretical paper’! In an e-mail to me, dated 1 October 2015, GCH wrote as follows (italics added):

I was interested in your ranking of *The Accountant in a Golden Age* and the *Two-Sector Model* ... I suppose *I lean towards the second because the ideas in it came from me*, whereas [Harold Lydall] had asked me to look into his puzzle with the accounting rate of profit vs the economic rate of profit. I’ve always felt that I should have asked him and Deane Terrell to be co-authors. I’ve also been grateful to Ken Wright who went after Kay when Kay criticised the OEP paper—I asked Ken to do so as I did not have the required maths to do so myself.

Of these six, the traumas of publishing three of them—namely, those of 1963, 1966, and 1976—are documented by GCH in one or another of his many splendoured writings. I want, however, to highlight just one of them—that associated with the important critique of Kaldor’s distribution mechanism, in a full employment growth context, first published as Kaldor (1955–1956) and Kaldor (1957).²⁹

Much of the unfortunate details of the story of the beautiful and thoroughly competent—from a serious ‘economics of Keynes’ point of view—Harcourt foray into the thorny field of functional distribution of income theory has been told by GCH himself, in his highly readable essay in the Shepherd edited volume (Harcourt 1995b: 73–74). The ‘punch line’ of the story is when GCH, immediately after presenting the paper at the King’s College seminar for research students run by Kaldor, ‘remembering going up to one

²⁶ In a precise analytical sense, this paper is a link between the ‘Yogi’ and the ‘Commissar’ of the previous section and the five issues that form the core of this section. In particular, it provides the crucial link between theory and policy.

²⁷ But this really belongs to the vintage (sic!) GCH productions of the 1960s (Harcourt 2012a: 28): ‘The article had a long gestation period. I wrote the first draft in 1966’.

²⁸ To the best of this author’s knowledge, the reference to PKE by GCH was first made in footnote 1 of Harcourt and Kenyon (ibid.).

²⁹ It was in relation to these that Paul Samuelson (1964) first characterized—perhaps chided is a better description—a ‘fictitious’ being called *Jean-Baptiste* Kaldor. GCH has referred to this fictitious character in many of his writings, where he is critical—as in Keynes (1936) [1973]—of any kind of Say’s Law (of markets). In Harcourt (1991: 160) he reminds his readers that Paul Davidson had critically discussed Jean-Baptiste Kaldor’s theories of full employment distribution. Davidson (1968: 259) had, in turn, referred to the priority of Samuelson on this point.

particularly arse-licking graduate student afterwards' (ibid.), and picking 'him up by the lapels and [saying]: "Look, son, if you want to grease Kaldor at my expense, at least do your bloody home work first"'. I have never thought it good manners to ask GCH to reveal the identity of this particular 'arse-licking graduate student', nor has he, true to his civil nature, volunteered to identify the person. All I wish to add is that this kind of behaviour is, I think, more prevalent in our sorry profession now, than it was then—and I am a personal witness to this increasing 'trend'.

GCH ends this particular story by his characteristically magnanimous statement (ibid.: 75): 'I should add that Nicky and I subsequently became firm friends. You will find an affectionate and appreciative evaluation of him and his work by me in *Economica*, May 1988'. However, he does 'add', in Harcourt (2006a: 148, fn. 1): 'It took me about 10 years to get our relationship back on an even keel and we ended up friends. Just before he died in 1986 I received an appreciative note from him for my review...of his Okun Lectures...in the *Economic Journal*'.

On the technical side, GCH was fundamentally correct in pointing out the inconsistency implied in the pricing process between sectors, within and between periods in Kaldor's model (particularly in Kaldor 1957). But the period assumption in Kaldor's growth model is fundamentally unfaithful to the stance Keynes took on this issue. In his letter to Robertson of 20 May 1933, and in the first section of the draft Chapter 7 in *The Monetary Theory of Production*—appropriately, from the point of view of GCH—titled 'The Concept of an Accounting Period' (Keynes 1979: 26, 73–76), there is ample evidence that GCH was comprehensively correct in this aspect of his criticism of Kaldor.

In Kaldor (1957) there are two common infelicities, mainly because Kaldor was not careful (as usual!). They were not formally taken up by GCH, although he does so implicitly, but discussed in detail by Champenowne (1971, especially the appendix) and McCallum (1969), who both analyse *local stability*³⁰ and also for the *continuous-time* version of a *period* model, in which the justification in Kaldor was economic—and its inconsistency was, of course, pointed out by GCH. To analyse local stability and claim it has validity for the dynamics of a capitalist system is the customary vice of 'pretty

³⁰ It is more than intriguing that the two titles of the papers by Champenowne and McCallum, in leading journals, contain the words *stability* and *instability*, referring to the same economic model! Does it not indicate that the naive technical notion of stability is inadequate to encapsulate the rich connotations which it implies in an economic context? As Keynes wrote to Gerald Shove, on 21 April 1936 (Keynes 1973: 2—replacing 'the influence of expectations and of transitory experience' with 'stability'—or 'instability'): 'As soon as one is dealing with [stability], one is, in the nature of things, outside the realm of the formally exact'. I suspect that GCH is in agreement with this view of Keynes—as I am!

polite techniques'. To not know that even a 'little bit of nonlinearity'—of which all of the referred to Kaldorian models are copiously endowed, and neglected—can destroy any kind of formal equivalence between a continuous and discrete time dynamic model, is inexcusable.³¹

Finally, I should at least mention the highly innovative 'Keio paper' (Harcourt 1969b).³² It is a teaching model of Keynesian economics in the grand tradition of Hicks (1937) and Lindahl (1953—who uses the *geometry* of Goodwin's innovative four-quadrant alternative to the more famous two-quadrant, IS-LM *diagram* (cf. Harcourt 1985: 415–416).³³ In the opening paragraph GCH extols the virtues of the 'algebraic method' over the 'geometric'—'quadrant approach'. He adds (*ibid.*: 87; italics added):

The preference for the use of *algebra rather than geometry* arises from the view that the 'quadrant' approach *can mislead students*, who may settle for *mechanical drill*; it may confuse them about the applicability of their results and geometry does not always bring out clearly the limitations of the methodology used. The *dangers are more easily avoided*, it is believed, *when algebra is used*.

These strictures against the geometric method, and in favour of an algebraic method, can easily be reversed, even referring to the pernicious practice of the current frontiers in economic theory.³⁴

More importantly, GCH is, as always, absolutely candid on his comparative static analysis—essentially based on a judicious use of Marshallian *ceteris paribus* assumptions—and there is no pretence whatsoever of any kind of transition dynamics or its stability (*ibid.*: 87–88; italics in original):

³¹ As my maestro, and GCH's good friend, Richard Goodwin, pointed out, 65 years ago (Goodwin 1950: 319): 'Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusions as to behavior'.

³² I shall refer to the version reprinted in Harcourt (2012b), as Essay 3.

³³ In a personal letter to me, dated 2 January 1985, the late Bent Hansen wrote that this particular paper by Lindahl was prepared for a lecture visit to Australia—and it was in preparing this that Lindahl had first read Keynes (1936) [1973], fully over a decade and a half after it was first published!

³⁴ In Harcourt (1985: 418; italics added) he reports on Goodwin's preference for geometry as follows: '[Goodwin] wanted to be the Marshall of the twentieth century by teaching *linear* models entirely in geometry (he still prefers geometry and geometric proofs to algebra)'. I should point out that Goodwin used geometry for non-linear model analysis and algebra for linear analysis! In an e-mail to me, dated 24 September 2012, GCH wrote (italics added): 'In my lectures on macro to 3rd year Ugads in 1967 at Adelaide I used a *linear geometric version of Hugh [Hudson's] model*, see Essay 3 of On Skidelsky's Keynes and Other Essays'.

Nothing is said, formally about the process of getting from one equilibrium position to another or whether the economy will actually do so, and any statement about *changes* as opposed to *differences* requires an act of faith (which is common to all believers but is not always made explicit). That comparative statics results are so applied to process situations is not stressed enough in the text books.

How very true! I only wish my teachers, and the textbooks they chose to teach from—before Goodwin—had stressed this important fact, or at least advised us to keep a barrel of salt by our sides, when authors pontificated about dynamics and stability. I envy those who were taught Keynesian economics from this particular teaching model.

4 *The Gladiators: Australians and Cantabrigians*

I returned to Cambridge in 1982 to try to carry out one last research project—to document the intellectual history of those we may loosely think of as Keynes's pupils. I call it 'Joan Robinson and her circle', for I want to use her contributions as the focal point around which to put the writings of Kahn, Kalecki, Kaldor, Sraffa, Pasinetti and other gifted people most of whom worked at Cambridge in the Cambridge tradition before and after Keynes's death (Harcourt 1992: 9).

In this endeavour 'to carry out one last research project'—as in all his other research projects—GCH was, indeed is, eminently successful. To the six named above, one could easily add, 'as other gifted people', David Champernowne, Robin Matthews, Richard Goodwin, Richard Stone, James Meade, Frank Hahn, W.E.G. Salter, Hugh Hudson, Tom Asimakopoulos, John Cornwall, and many others.

Pride of place must, of course, be accorded Joan Robinson (Harcourt and Kerr 2009), whose theories, methods, and visions were brilliantly and as comprehensively—as any one mortal could study this complex, dazzling, scholar—dissected, analysed, and reassembled for relatively easy comprehension, by GCH. In all this there is one diagram that, surely, takes pride of place and, in more ways than can be imagined, for example, by Frank Hahn:³⁵ the so-called *banana diagram* (Robinson 1962, figure on p. 48;

³⁵ Hahn's views on Joan Robinson, neoclassical economics and method, as enunciated in the 'notorious' Feiwel (1989) volume, Hahn (1989), are misleading (on Joan Robinson), incorrect (on neoclassical economics), and ignorant (on many economic and mathematical issues). For example, his assertion that 'Linear programming, as we all know, is neoclassical economics' (ibid.: 895), is complete nonsense, to

Harcourt and Kerr 2009: Figure 8.1, p. 130).³⁶ It is a depiction of almost all the characteristics that define most strands of PKE. Above all, it encapsulates a notion of expectations—what the Swedes like Lindahl and Myrdal called ‘anticipations’—which does not rely on any formalization in terms of conventional probability theories³⁷—even logical and subjective ones, including Shackle’s innovative ideas of it, based on ‘potential surprise’.

I believe the whole story of the Keynes–Kalecki–Robinson-based PKE of GCH can be told in terms of a suitably generalized ‘banana diagram’—including one of the pet topics of GCH: the problem of the trend and cycle.³⁸ One possible generalization is indicated in Figure 45.1, but I think the ‘full’ dynamics implied in it would be what is called a homoclinic tangle in terms of modern dynamical systems theory. The ‘impredicative’, self-referential, dynamics would imply ‘loops’, curves that ‘cross’ themselves, and this kind of dynamics is alien even to frontier PKE research. But the whole of Chapter II of Robinson (1962) lays out the foundations of GCH-type PKE.

This, I think, is one example of how geometry can encapsulate the thoughts and ideas of scholars like Keynes and Joan Robinson, Harcourt and Kalecki, Wicksell and Myrdal, and many others whose prose was more Forsterian than Bourbakian.

One final point: I believe, almost passionately, that Mabel Timlin was the original Post Keynesian economist. Her geometry in Timlin (1942) was impeccably accurate—perhaps because that geometer par excellence, H.S.M. Coxeter³⁹ was her ‘draughtsman’! GCH is, I think, in agreement with this view.

express myself in the rude Hahn-mode, rather than in the courteous GCH-mode. Hahn either does not understand the nature and scope of linear programming—in 1989—or he does not know neoclassical economics or, more likely, both! Linear programming, in 1989, was not what it was at the time of Kantorovich or Dantzig; it was not even what it was at the time of the celebrated, pedagogical DOSSO.

³⁶The I curve—a ‘parabola’—in Robinson ‘represents the rate of accumulation as a function of the rate of profit that induces it’; it becomes the ‘Animal spirits’ function in Harcourt and Kerr. The A ‘curve’ in Robinson is a straight line, becoming a parabolic arc in Harcourt and Kerr; it ‘represents the expected rate of profit on investment as a function of the rate of accumulation that generates it’, in Robinson. True to the story of the interaction between the Yogi and the Commissar, Harcourt and Kerr show an interaction between actual and realized—really, therefore *ex post* and *ex ante*—values of the rate of profit and the rate of growth.

³⁷The most original and knowledgeable review of Keynes (1921) [1973] was by Émile Borel (1924) who easily recognized that the formal underpinnings of what came to be known as Keynes’s ‘beauty contest paradox’ was what Poincaré called ‘non-predicative definition in the theory of sets’ (*ibid.*: 58). This is one other way of characterizing a self-referential statement and no formal theory of probability can deal with it, except algorithmic information theory. It is this formalization that the Post Keynesian economists need to make sense of animal spirits—and Joan Robinson did so, albeit implicitly. She was an intrinsic mathematician, like Sraffa—but a non-orthodox one, again like Sraffa. Goodwin was the only one I am aware of who bracketed the two in this way (Goodwin 1989: 916).

³⁸I am afraid I do not agree with GCH bracketing Goodwin and Kalecki on this; they worked with quite different models and, in fact, Hugh Hudson was more Goodwinian on this issue.

³⁹It was Coxeter who taught Escher the geometry of loops, self-intersecting curves that generate ‘paradoxes’—of the kind that should be common in the PKE underpinned by the economics of Chapter 12

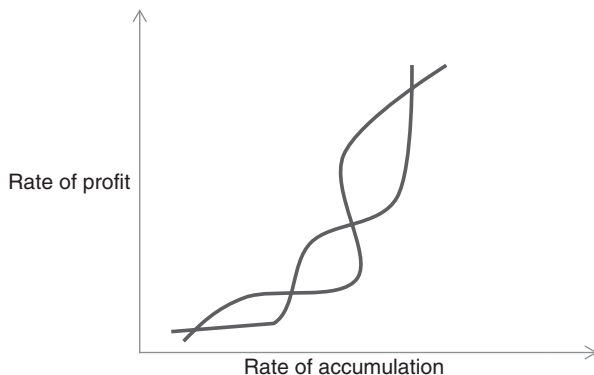


Figure 45.1 A 'generalized' JR-GCH Banana diagram (Source: Cf., also Harcourt and Kerr (2009: Chapter 9, especially Figure 8.1) and Robinson (1962: 48))

5 Conclusion

I was rather taken aback by Murray [Kemp's] too bleak a picture of Cambridge Ph.Ds (Oz-types) in the 1950s. May I try to put the record straight? ... I don't think it is true to say...that the Australian contingent all regretted going to Cambridge; and it is certainly not true that none of us (except me) got our Ph.Ds ... Most of us returned to Cambridge on leave, often on several occasions. We were most of the time a happy band and, I submit, not too bad a vintage. So Murray should either stick to theory or get his facts right (Harcourt 2006a: 146, 147–148).⁴⁰

GCH has elevated the noble art of reviewing and of writing review articles to new heights of scholarship and enlightenment (Repapis 2014).⁴¹ It is impossible to write of GCH's contribution to economics without mentioning

of Keynes (1936) [1973]. On the other hand, Matthews (1984: 210, fn. 2) points out: 'Chapter 12 was apparently written less carefully and in a more light-hearted spirit than most of the *General Theory*. It was not subjected to the scrutiny of the group of younger colleagues assembled by Keynes to help him (information from Richard Kahn)'.

⁴⁰ My own 'generation' of outstanding Australian economic Ph.D. students—all of whom completed (sic!) their doctorates in much shorter time than I did—were Roy Green, Chris Gregory, Murray Milgate, and David Vines, all of whom have gone on to distinguished academic careers, in Australia, Ireland, and England. There were other Australians, of my vintage, non-economists, who were as outstanding as the above economists—Adrian Wilson, for example, in philosophy and the history of medicine.

⁴¹ In the otherwise admirable paper by Repapis (op. cit.), there is the curiously incorrect assertion that (ibid.: 1,518): 'Famously, Alfred Marshall in his *Principles* avoided putting diagrams...in the main text'. This is an incorrect assertion, at least as far as the sixth and eighth editions of the *Principles* are concerned—the only two editions I have read. The first diagram appears, for example in the eighth edition, already on p. 64 (although this is Figure 2; on the other hand, Figure 1, appears on p. 511).

the role of reviews and review articles in this endeavour. Fortunately, the excellent piece by Repapis (op.cit.) makes any need for an effort by me, at least with respect to the reviews, redundant. On the other hand, time and space⁴² constraints make it virtually impossible to analyse the enormous contribution to economics made by GCH via the writing of review articles.

Yet, it would be grossly unfair not to acknowledge the finesse and panache with which GCH portrayed the works of Kaldor (1985, 1996) and implicitly also Kaldor (1970)⁴³ via Harcourt (1988, 1997a); Kahn (1975, 1977, 1978, 1984); Austin Robinson (Harcourt 1997b); David Champernowne (Harcourt 2001a); Skidelsky (Harcourt and Turnell 2005); Pasinetti (Harcourt 2009); Goodwin (Harcourt 1985); the work on measurement *with* theory at the Department of Applied Economics (DAE) (Harcourt and Kitson 1993); Schumpeter and evolutionary economics (via Harcourt 2004, 2011); Salter (Harcourt 1962); the theory of the firm and returns to scale (Blankenburg and Harcourt 2007); and many more.

In all this reviewing GCH has maintained the civility and courteousness that has been the hallmark of his writings—except just two! These exceptions are the rather ‘irritated’ reviews of Feiwel (1989) and the one on Heilbroner and Milberg (Harcourt 1997c)—and I agree wholeheartedly with these ‘exceptions’. Both of these works deserve to be criticized in the way GCH has done.

On the other hand, I am not sure there is enough justification to question Skidelsky’s stance on Étienne Mantoux, as Harcourt and Turnell (op.cit.) do. Mantoux was basically hostile to Keynes (cf. Mantoux 1937) and his calculations (in Mantoux 1946) were, to say the least, questionable, from any theoretical and measurement point of view. On the reverse side, I wonder whether GCH is too well mannered to criticize Stiglitz (Harcourt 1997d) or Laidler (Harcourt 2000). In the case of Stiglitz, his monomaniacal devotion to asymmetric information blinds him to the more basic shortcomings of the two fundamental theorems of welfare economics. As for Laidler, Chapter 2 of the book reviewed by GCH is basically based on a variety of secondary literature and sources who were incompetent or uninformed on the topics on which they chose to pronounce—and on which Laidler relied.

But it is too much to ask GCH to be aware of all the mathematical underpinnings of the two fundamental theorems of welfare economics, especially

⁴²The epigraph in Robinson (1962) is the Bergsonian quote: ‘Time is a device to prevent everything from happening at once’. Dharma Kumar is reputed to have added: ‘And space is a device to prevent everything from happening in Cambridge!’ My time and space constraints have caught up with me (*pace* GCH!).

⁴³When Sen (1980) asked Kaldor for his opinion on which were, in his—that is, Kaldor’s—opinion the best journals for economists, the answer had been immediate and unequivocal: the *Cambridge Journal of Economics* and *Oxford Economic Papers*!

since he does not even take them too seriously. It is certainly absurd to expect GCH to be aware of Laidler's blind reliance on secondary Swedish sources, especially since—to the best of my knowledge—GCH does not read or write that (or any other) Scandinavian language.

In *The Little Drummer Girl*, John le Carré constructs the following conversation between Joseph and the Drummer Girl (Chapter 5; italics added): “And after dinner, as your personal Mephistopheles, I shall take you up a high hill, and show you *the second best* place in the world. You agree? A mystery tour.” “I want the *best*”, she said, drinking her scotch. “And *I never award first prizes*”, he replied, placidly’.

GCH has learned, after a lifetime of fighting the good fights, to be satisfied with the ‘second best’. The quest for justice and fairness, the mitigation of inequalities and discrimination, the fights on behalf of the less privileged and minorities, the trade-off between equity and efficiency, the sanctification of the market as against a dogmatic reliance on centralization, on all these—and many other policy issues—he has learned to be patient with the obdurate and the arrogance of orthodoxy.

A Note on the References

All of the listed references—and more—have been used in the preparation of this manuscript, even if not all of the individual items are cited, explicitly, in the body of the text.

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Charles Hilliard Feinstein (1932–2004)

Avner Offer

1 Introduction¹

Charles Feinstein's achievement was to work out the structure and size of the British economy from 1965 and back to mid-Victorian times. This makes it possible to evaluate how well the economy has performed at any point in the past 150 years, and to compare it with other periods and other countries. There is a great deal more: a precocious apprenticeship, an extension of knowledge in related fields and further back in time, inspiring teaching, effective administration, and professional leadership. As a scholar, Feinstein was a master of both structure and detail. As a colleague, he combined authority with integrity and generosity. Beyond scholarship, his life also subsumed a longer arc: the quest for an equitable South Africa in his youth, and its resumption in his final years.

A life is formed by its times. Charles was the eldest child of Rose and Louis Feinstein, members of the liberal Jewish community of Johannesburg. Louis had immigrated as a child with moneyless parents from Latvia and rose to become a prosperous stockbroker. In politics, he was an 'armchair Marxist', and current issues were discussed critically at home. Charles excelled

¹This chapter is derived from Offer (2008) with the permission of the British Academy.

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at Parktown Boys' High School (modelled on English grammar schools) and graduated before he was sixteen. At Witwatersrand University, he studied economics, although with some regrets at not having chosen history. His best teachers left their mark: Helen Suzman in economic history, later renowned as the only anti-apartheid MP, and Ludwig Lachmann in economics, a refugee from Austria, and unusually for the time, a disciple of Hayek and an 'Austrian' economist of the top rank. After taking his degree in 1950, and at the insistence of his father, Charles qualified as a chartered accountant in 1954. It was 'the most boring period of his entire life'.² But he was good at accounting, and it left a mark. He also took a further ('honours') course in economics, and appreciated the discipline of dissecting difficult texts with the critical Lachmann.

Charles was determined to change the world as well as to understand it: He joined the Communist Party, the most racially inclusive opponent of apartheid. At the age of twenty-one, he chaired the youth wing of the Congress of Democrats, a small group of whites supporting the African National Congress. Selling the party sheet is a rite of radicalism. His friend Bob Hepple remembers how

Charles insisted that we had to 'connect with the Black masses,' and this led us to the bus queues every Friday night. The queues were notorious for muggings and stabbings, and you can imagine the astonishment of the waiting black passengers to see young whites ducking and weaving among them apparently impervious to these dangers and to the risk of arrest. Charles's innocent smile would soon melt any hostility (Hepple 2005: 1).

The economics that appealed to him were those of Karl Marx, and he submitted an honours dissertation on the labour theory of value. The external examiner W.H. Hutt, an early member of the Mont Pelerin Society and a ferocious critic of trade unions, rejected the thesis outright. Despite achieving a First Class in the exams, Charles failed his degree. He had applied for graduate work in Cambridge, and reported this setback anxiously. The left-wing economist Piero Sraffa generously deferred a decision on admissions until Charles could get there in person. When the day to leave arrived, hundreds of black youths came to chant their farewells at the railway station—his period of active struggle was over.

²From the transcript of the address at Charles's funeral, composed jointly by members of his family, and read by Alan Stein.

2 Historical National Accounts

Charles was attracted to Cambridge by the presence of the Marxist economist Maurice Dobb there, and the two remained close for years afterwards. Charles planned to investigate whether rich-country wealth arose from exploiting the colonies. But Joan Robinson (an eminent Cambridge Keynesian) said to him at a party: ‘How can you explain the prosperity of the Scandinavian economies if it is all due to empire?’ (Thomas 2002: 289). He decided to focus instead on the metropolitan core of the globalizing British economy.

The dissertation (supervised by Robin Matthews) contained the kernel of all of his future work, and in two instances, a good deal of its substance as well. Its core were two long quantitative chapters, one of them a detailed estimate of ‘The Net National Income of the United Kingdom, 1855 to 1914’ derived mainly from income tax data (and from Bowley and Wood’s wage series). The other core chapter covered capital formation and overseas investment over the same period. Together, these were the essential components for any encompassing estimate of Victorian national income.

It was a golden age for the study of trends in the Victorian economy, a good deal of it at Cambridge, with national income estimates by J.F. Prest and by James Jeffreys and Dorothy Walters, a brilliant study of inverse cycles of migration and economic activity by Brinley Thomas, long-run UK national income estimates from Phyllis Deane and W.A. Cole, the abstract of British historical statistics by Brian Mitchell and Phyllis Deane, building cycle and capital formation estimates by Karl Maywald and Bernard Weber, and estimates of foreign investment by Albert Imlah and A.G. Ford. Alec Cairncross’s remarkable book *Home and Foreign Investment, 1870–1913* (1953; itself a revision of a pre-war Cambridge thesis), provided Charles with a title, and a model whose findings he was able to complement and extend.³

Cambridge in the 1950s still basked in the afterglow of Keynes. To place Charles in this setting, we need to sketch in some history of national income accounting. Estimates of the aggregate income, output, and wealth of nations go back to William Petty and Gregory King in the seventeenth century, and continued periodically with growing sophistication in many countries (see Studenski 1967, Carson 1975, and Vanoli 2005). In 1933, Simon Kuznets

³References may be found in the bibliographies of Charles’s dissertation, ‘Home and Foreign Investment: Some Aspects of Capital Formation, Finance and Income in the United Kingdom, 1870–1913’ (Cambridge University PhD thesis, submitted March 1959) and of his first two books, Feinstein (1965, 1972).

used national accounting estimates from fifteen countries to compare their aggregate incomes before and after the First World War, and estimated US national income back to 1850 (Kuznets 1933: 206, Table 1 and 216, Table 2). For our purpose, however, the starting point is interwar Britain. The Great Depression of the 1930s had dented the belief in *laissez-faire*. In his *General Theory* of 1936, John Maynard Keynes questioned the doctrine that labour and capital would always be fully employed. His concept of aggregate demand called out for empirical estimation.

Sophisticated national income estimates had already been published for the 1930s by Colin Clark, then a Lecturer at Cambridge, who drew on previous work by Bowley and Stamp. The USA was further ahead, with a set of estimates prepared initially by Kuznets, and published annually by the federal government from the mid-1930s onwards. The outbreak of war turned the problem upside down: no longer a shortfall of demand but rather an excess of it, that Keynes anticipated when fully employed workers would be chasing fewer goods.

In February 1940, Keynes published a small book on *How to Pay for the War*,⁴ which included an estimate of national income, derived from the work of Clark. From his wartime position in the Treasury, Keynes welcomed a more sophisticated set of national accounts prepared in the same year by two temporary civil servants, the economists James Meade and Richard Stone. These accounts were published with the 1941 budget as a White Paper, and every year thereafter. There are several different ways to compile national accounts. The Meade–Stone approach, which was informed by Keynesian macroeconomic concerns, prevailed in Britain, and has also provided the template for successive United Nations (UN) models which have diffused across the world.

‘National accounts’ provide a comprehensive quantitative double bookkeeping model of the economy as a whole, with total income on one side, expenditure on the other, and output as a check on both. The income on one side appears as expenditure on the other. Each aggregate table is constructed bottom-up from many statistical series covering particular segments and sectors of the economy. The ability to monitor the movement of the economy on an annual, quarterly, or even monthly basis is immensely useful to government, business, commentators, academics, and voters. It did not take long for the numbers to enter into everyday use.

To place the series in longer comparative perspective, it was necessary to extend them back into the past. In the USA, Kuznets published retrospective accounts for the interwar years in 1941, and a similar effort was started by Stone in Whitehall in the same year. After the war, Stone went to Cambridge to head

⁴ Anticipated in November 1939 by two articles in *The Times*.

the new Department of Applied Economics, and he took the project with him. It required more than 30 years to finish, and it was Charles who would bring it to completion. Wages and salaries were estimated first by Agatha Chapman, using readily available statistics. Expenditure was more difficult. It consisted of three parts: consumer expenditure, government expenditure, and savings/investment. In 1954, Stone finally published a large volume on consumer expenditure; two additional volumes took 10 more years. The complementary volume on capital formation, started by Maywald in 1950, also progressed slowly.

In 1958 Charles took a research position in the Department, where he adapted national income series for immediate use, in his role as the ‘statistician’ for its ‘Cambridge and London Economic Service’. Maywald submitted his completed study on capital formation in 1959, but Brian Reddaway (who had followed Stone as Director) refused to publish it without further revision. They failed to agree, Maywald departed, and the task fell to Charles. He found much to revise and to add, and the book was finally published in 1965 as the fourth in the series (Feinstein 1965). Like the dissertation, it conveyed a mastery of design, exposition, and detail. The interwar national accounts project had now been 25 years in the making. Stone’s interest had waned, and the ship, so near to completion, seemed destined to remain on the stocks. At that point, in 1965, Charles stepped forward: ‘It seemed to me extremely unfortunate that all this work had been done on the components...but nobody was going to pull it all together and provide the key series for GDP. So I went to Stone and Reddaway and said I would like to do this’ (Feinstein quoted in Thomas 2002: 290).

Charles proposed to create a seamless series from 1855 up to the present. For the post-war period, he would use the existing official statistics. For the Edwardian and interwar years, he relied on the Cambridge project, to which he had made a large contribution. For 1855–1914, he would further develop the series available in his own dissertation. His classic volume was published in 1972 (Feinstein 1972). It drew on prior efforts over decades, but was still an extraordinary achievement for a single scholar. With little research assistance, without using computers, it was completed in a remarkably short period of time. In the words of Paul David, ‘Charles brought to these undertakings a capacity for the sustained, painstaking “unglamorous” work of mobilizing the available statistical sources, sifting and synthesizing the contributions of others, removing the conceptual inconsistencies and improving upon the quality of the data wherever possible, presenting the results transparently and evaluating with utter candour the strengths and limitations of the results’ (David 2005: 10). The volume had a set of tables with estimates of the standard national accounts series and an explanatory section, which described the origins and manipulation of every series, with their approximate margins of error. It was

not intended for light reading, but provides transparent descriptions of complicated procedures, with occasional flashes of wit in the footnotes.

During the 1950s and the 1960s, a new wave of historical national accounts appeared in several countries, framed to conform with the templates set out by the UN, or some variant thereof. In this wave, Charles's volume stands out for its integration of form with content. The physical shape was inherited from the series: a distinctive oversized red volume, laid out attractively on thick cream paper, by Cambridge University Press. It was a sourcebook, with no explanatory aspirations. But for British economic history, it was an act of closure as definitive as Liddel and Scott's *Greek Lexicon* in another discipline and age. It strikes me as the most elegantly reasoned, organized, and presented of its wave.⁵ Like Liddel and Scott, national accounts are never truly finished: they need to strike a balance between availability and perfection. Charles knew when to stop. The tables provided a foundation for others to build upon. In this role, the book has endured well. Despite some minor subsequent revisions (by Charles and by others), it remains the first port of call even in its original form.

In 1963, Charles became Assistant University Lecturer in Economic History and Fellow and Director of Studies in Economics at Clare College. The man he replaced in the Faculty of Economics had lectured on the USA, but Charles decided to teach Russia instead. He went to Moscow for a few weeks to study economic history and Russian. In the absence of a market system, the Soviet Union had a completely different system of national accounts (the Material Product System). Soviet economic performance was impressive at the time, and interest in it was high. It has since emerged that Charles's lectures had introduced Russian economic history to most of the current British academic specialists in the field. Charles began to move away from communism in 1956, after Khrushchev's 'secret speech' denunciation of Stalin and the invasion of Hungary, and he left the party in 1960. The time in Moscow intensified his disillusion. But he remained on the left, and edited a Festschrift for Maurice Dobb in 1967, and in the same year he gave evidence in court in support of the Oilfields Workers Union of Trinidad. The Russian interest also attracted a Fulbright Fellowship, which took the family to Harvard in 1967–1968. Charles studied Russian microfilms there but did not publish the papers he wrote. He never lost interest in Russia, continued to teach the subject, and facilitated two conferences on communist economics in 1980 and 1995. In 1978, he moved to a chair at the University of York, where he gave a set of three Inaugural Lectures on the 'Soviet Economy and Society since Stalin'. I found as a co-examiner in the 1980s that he still considered the benefits of Soviet industrialization to

⁵This is not a judgement on the accuracy, completeness, and methodology of any of the other projects.

outweigh its human cost. But he only wrote two brief studies of the USSR, both of them critical and both after its demise (Feinstein 1992, 1997).

At Clare, his commitment and ability were soon manifest. A few Fellows regarded him as a dangerous Marxist (he was active in the peace movement and Campaign for Nuclear Disarmament (CND)), but the College elected him Senior Tutor in 1969. He had earlier helped (at University level) to negotiate co-education in the first three colleges, and Clare had just admitted women students. Student unrest had spread to Cambridge. Charles handled these issues with ‘flexibility, humanity and complete integrity ... The students found in him someone willing, in a way many other senior members would not have been, to engage with them in patient and good-humoured discussion about student grievances’ (Hepple 2005: 7). He was firm but knew when to be lenient.

3 Methodology of Historical National Accounting

Charles later said that his contribution was to provide the data, not to test hypotheses (Thomas 2002). This was not only a matter of taste or temperament. It was a coherent methodological position that was shared, and argued for, by his mentors and models among the fathers of national accounting, particularly in Cambridge, and which reflected the primacy they gave to observation over theory.

They did not reject mathematical logic or statistical rigour. What they doubted were the deductive models of partial and general equilibrium originating with Walras, Samuelson, and Arrow-Debreu, which implied that markets necessarily allocated resources efficiently, that market outcomes were always for the best, and that they rewarded everyone justly. Simon Kuznets, their doyen, thought that the task of theory was to specify the variables to be measured. Theory was not immutable, but merely identified a set of empirical regularities, which needed to be revised in the light of new knowledge, and of changing social values. It was risky to accept data without understanding how they came into being. More than one model could be fitted to any set of data, and a good statistical fit could not by itself guarantee correctness. Charles met Kuznets several times at Harvard. Kuznets’s method, like that followed by Charles, was essentially inductive: ‘[F]rom measurement to estimation to classification to explanation to speculation’, though Charles might hesitate before that final step (Lundberg 1971: 460; see also Fogel 2000).

A similar scepticism was expressed by other leaders of national accounting. Richard Stone chose a dialogue from *Crochet Castle* (1831) by Thomas Love Peacock for his epigraph in the first volume of the Cambridge interwar

national income series. Mr Mac Quedy praises the modern political economy, 'the science of sciences'. He is mocked by THE REV. DR FOLIOTT. 'A hyperbarbarous technology, that no Athenian ear could have borne. Premises assumed without evidence, or in spite of it; and conclusions drawn from them so logically, that they must necessarily be erroneous' (Stone and Rowe 1954: xxiv). Wassily Leontief, the interwar inventor of input–output analysis (a technique that partly overlaps with national accounting and is now integral to it), made similar points in two iconoclastic articles (Leontief 1937, 1971).

Kuznets, Stone, and Leontief were not mindless empiricists. All three of them eventually won the Nobel Memorial Prize in Economic Sciences.⁶ Kuznets originated seminal cyclical theories of economic activity and inequality. Leontief described himself as a mathematician and theorist. He later wrote, 'When I developed input–output analysis it was as a response to the weaknesses of classical-neoclassical supply-and-demand analysis ... I felt that general equilibrium theory does not see how to integrate the facts ... My feeling is that the fundamental theoretical understanding of economic fluctuations is as a dynamic process' [i.e. not an equilibrium one] (Leontief quoted in Foley 1998: 117–119). Stone's consumer expenditure research stimulated the development of standard econometric methods for dealing with serial correlation (Cochrane-Orcutt and Durbin-Watson) and also applied a simple pioneering neoclassical model of the individual consumer.

Governments had taken up national accounting in the 1930s because of the market failures of depression and war: it provided the means to design interventions and to monitor them in pursuit of prosperity and full employment; the data were published and available to all. Observation and deduction both have a role in science and are needed to discipline each other. The sceptical position was scientifically sound: a preference for observation over speculation as a way to the truth, and a reality check on theoretical constructs. Experience has borne it out: like other ventures of the 1930s (radar, electronic computation, nuclear physics) what began as an academic problem ended up as an elaborate state technology within less than a decade. Like those other technologies, it is enduring and pervasive. It works unobtrusively in the service of government, business, and scholarship. Its deductive rivals (especially computable general equilibrium) are less modest in their ambitions, but (with their assumption of rational behaviour) resemble belief systems as much as fail-safe techniques (which is not to underestimate the power of belief, and indeed its necessity). National accounting made it easier, and therefore necessary, for governments to provide those services that only governments can provide, or those they provide more efficiently (education,

⁶This does not confer infallibility but indicates standing in the discipline.

health, social insurance, central banking, infrastructure), and thus to satisfy the aspirations of voters. Indeed, national accounts may have gone beyond measuring the preferences of voters and into shaping them, by setting up a target for affluence and helping to monitor it. This success in devising a working model of the economy is one reason why governments have become so large and indispensable, why even conservatives have to be social democrats now.

Charles published his *National Income* towards the end of the heroic phase of historical national accounting. What next? Simon Kuznets was co-ordinating a project to compare and explain economic performance in seven different countries. Robin Matthews had signed up Charles to join him in the British volume in the late 1960s (together with John Odling-Smee). Imminent publication was announced in 1972, but it required 10 more years. The study moved beyond national accounting, to examine the determinants and scale of growth rates in different sub-periods. It drew on ‘growth accounting’, developed by Edward Denison and John Kendrick in the 1950s. This was based on the Solow neoclassical model of the same decade, in which economic growth was seen to arise out of increments of labour, capital, and knowledge. The flow of output in growing economies rose faster than the flow of economic inputs (labour, capital, natural resources). This large unexplained positive bonus was referred to as the ‘residual’, or more technically as ‘total factor productivity’ (TFP), that is, that part of growth not accounted for by growth in the inputs of capital or labour.

TFP represented ‘any contribution that may arise from increasing returns to scale and from the effects of technical progress and advances in knowledge, of shifts in resources between sectors, and of changes in the extent of obstacles to more efficient use of resources (e.g. restrictive practices on the part of management or trade unions). It will also reflect any errors in the measurement of inputs and output, and in the specification of the relationship between them’ (ibid.: 15). The study was a painstaking and immensely detailed breakdown of growth and TFP by sector and sub-period. It had a bearing on the perennial issue of performance and putative decline, both in international comparison, and in different periods of time. But it had taken too long, and its Keynesian assumptions were out of tune with the times. Although it stimulated a great deal of further research, of the seven projected country volumes only two others were ever published (France and Japan).⁷

The mood had already shifted in economics, from the empirically minded, social democratic, Keynesian consensus of the 1950s and 1960s, towards the efficient market and rational expectations models of the 1970s, which were associated with Chicago economics and its market-liberal rejection of

⁷ A tinge of regret by Matthews in von Tunzelmann and Thomas (2007: 162).

the state. A similar movement also arose within the discipline of economic history. In the USA and Britain, it was previously concerned with the development of industries, technologies, firms, and social institutions such as labour unions and government regulation. In the 1960s, a new approach to the past emerged in the USA which came to be known as the 'new economic history' or 'cliometrics'. It premised that individual rationality and market equilibrium provided a good explanatory framework for the economic past. It typically postulated a causal mechanism suggested by deductive economic theory and sought to measure it by means of a statistical test of the explanatory power of each of a cluster of quantitative variables on the 'dependent variable' to be explained. Identifying the relative importance of labour and capital as independent sources of economic growth also followed this procedure, but did not really count, since for Charles and his colleagues it was primarily an empirical investigation with no particular theoretical agenda.

By the mid-1970s, cliometrics had achieved some dazzling counter-intuitive findings, most notably Robert Fogel's study which scaled down the contribution of railways to economic growth, and his joint study of slavery with Stanley Engerman (*Time on the Cross*), which showed that slavery was profitable. More relevant to Charles were Donald McCloskey and Lars Sandberg on the rationality of entrepreneurs, and McCloskey's landmark 1970 macroeconomic article, 'Did Victorian Britain Fail?'. Previous writers on British economic stagnation suggested that it had. McCloskey's approach implied that the notion of failure was meaningless in neoclassical terms. From that angle, no slack was possible—capital and labour were assumed to pursue economic advantage to the hilt, and underperformance could only arise from resource deficiencies, not human failings. But however original, acute, and stimulating, these studies had none of the finality of national accounting: they depended on speculative theory, and their findings have remained a matter of controversy.

Charles hosted the first British cliometric conference at Clare College and was a natural candidate for the cliometric movement, perhaps even to lead it in the UK. His jointly authored *British Economic Growth* of 1982 incorporates the Solow neoclassical production-function growth model as the base of its TFP estimation. But the volume (like Kuznets, its general editor) explicitly eschewed 'sophisticated statistical or econometric methods', on the grounds that 'A less formal approach to the data reduces the risk that all the emphasis is placed on a single explanation, which may in reality be false' (Matthews et al. 1982: 10). In the late 1970s, time series analysis, which was a key cliometric tool, turned out to depend for its validity on an implicit assumption (quite often inappropriate) of a stationary (i.e. stable) relation among its independent and dependent variables. In the next decade, new tools (testing for

stationarity) emerged to deal with this problem. In the meantime, however, this issue had silently invalidated a good deal of prior cliometric work. The methods of national income analysis were not seriously affected.

An obvious progression from national accounting was to model the economy as a market equilibrium ('computable general equilibrium'). This was the economist's equivalent to the historian's 'seamless web', the assumption that everything affected everything else. Unlike the scepticism of conventional history, it also embodied the neoclassical assumptions that markets were for the best. The procedure was to take some subset of variables from the economic universe, to specify their interrelation by means of simultaneous equations, to insert empirical parameters from the historical evidence, and to solve for a market equilibrium. It required strong assumptions, namely the standard economic ones that outcomes represented the equilibrium of market transactions, and that the relevant market relations were adequately captured. If these premises were accepted, and if the model was unique, then computable general equilibrium made it possible to estimate the respective impacts of changes in prices, technology, or economic policies, and to simulate the effect of alternative, counter-factual scenarios, while taking feedback effects into account. In its top-down encompassing explanatory ambitions, this approach was the most far-reaching application of the 'new economic history'. It was a theory-intensive high-tech alternative to the data-centred, bottom-up and largely descriptive approach of the national income accountants.

An early exponent of computable general equilibrium in economic history was Jeffrey G. Williamson, at Wisconsin and later at Harvard. His first study appeared in 1974, and in 1986 he published *Did British Capitalism Breed Inequality?* This tested a famous Kuznets hypothesis, namely that in the long-run course of economic growth, inequality would first increase, before it began to decline once again. Crucial industrial skills would grow scarce in the early stages of industrialization, increasing earning differentials. Economic growth would stimulate investment in these skills, and their abundance in later stages would reduce the skill premium and hence inequality overall.

Charles accepted the book for review with few prior expectations, and his article eventually occupied thirty pages in *The Journal of Economic History* (Feinstein 1988). He was not familiar with computable general equilibrium, but no one had a more intimate knowledge of British historical statistics. He criticized both the data and the model. It was not that the model was inappropriate (though the reviewer's tone might sometimes suggest it), but that models could not deliver with poor data. Implicitly this was Leontief's view again, that description came before theory and was not inferior to it. Partly it arose from a difference of analytical temperament, a preference for measurable reality over abstract modelling.

But it also arose (I think) from another tension which the protagonists were loath to probe or even to admit, namely between those who regarded numbers as the servants of history, and those for whom history would vindicate economic theory and the primacy of markets. It would not be far-fetched (though not entirely true either) to associate cliometrics with the market-liberal doctrines with which it coincided. Charles hoped that the dust-up with Williamson left few lingering resentments, and that it encouraged respect for data integrity. It is still a focal point for a methodological divide that remains charged. It confirmed Charles in his convictions. Theories came and went, good data endured.

Charles chose instead to estimate capital formation backwards to the eighteenth century. This was an essential preliminary for complete national accounts and also impinged on the core preoccupation of economic history: fixed capital formation (machinery, buildings, infrastructure), or aggregate saving, its close analogue, had long been seen as the main driver of economic growth, in theories of growth from Adam Smith to Karl Marx, and on to W.W. Rostow. Kuznets had always suspected otherwise, and a key volume by Kendrick and Pech in 1961 established that it was not so crucial, and that human creativity (captured statistically within ‘total factor productivity’) was the decisive factor (Kendrick and Pech 1961). Instalments of the capital formation series came out during the 1970s and 1980s, and in their final form as the second half of *Studies in Capital Formation in the United Kingdom, 1750–1920*, which Charles edited with Sydney Pollard in 1988 (Feinstein and Pollard 1988). Despite his expertise, Charles took no part in the theoretical debates convulsing Cambridge on whether capital was a coherent and tractable category. His job, he said, was merely to measure its historical cost. But the acrimony of the debate unsettled him.

4 As a Colleague

After another year at Harvard (1986–1987), Charles took an appointment as Reader in Social and Economic History at Oxford, and Fellow of Nuffield College. In 1989, he was elected to the Chichele Chair of Economic History, at All Souls College, which he occupied for 10 years.

Charles participated in the CLARE Group of economists who agonized over economic decline in the late 1970s (Matthews and Sargent 1983).⁸ He understood from the outset that expectations and emotions had an effect on the work effort:⁹ ‘The root of the problem in the British economy was to do

⁸ Membership extended well beyond Clare College.

⁹ McCloskey eventually explored the effects of emotion much further than Charles ever did.

with labour relations and a combination of attitudes on the part of the workforce that were detrimental to productivity, reinforced by employers' refusal to recognize what would have been necessary to overcome those attitudes', he told Mark Thomas (Thomas 2002: 294). These class-rooted attitudes were also 'an extremely powerful factor in Britain's early post-1945 problems'. He made some headway on a history of the British economy in the post-war years, but the book was not finished. In conversation (and at his lectures), it struck me how strongly he felt about these dilemmas, and what he saw as the mindless unreason of the unions. He actively supported the Social Democratic Party (SDP) when it broke away from Labour in 1981, but he moved no further to the right. When Margaret Thatcher announced her resignation in 1990, 'Charles was fairly jiggling around the room with delight' (St. John n.d.).

Charles was my colleague for more than two decades, first at York and then at Oxford. In conversations over the years, he surprised me several times with a robust endorsement of an acquisitive and self-interested human nature. He had a taste for hard-nosed self-regarding theories of motivation, whether Marxist or neoclassical (like his mentor Maurice Dobb, who expounded both versions at Cambridge). But (again like Dobb), his own personal practice belied these beliefs.

5 Social Justice

To pick up the thread of Charles's research: after working for so long on productivity and capital formation, he turned to welfare after all. Capital formation had become 'rather arid; one was dealing with things that had no human interest, whereas once I got started on issues of wages, that opened up questions such as the standard of living' (Feinstein quoted in Thomas 2002: 295). The 'Williamson curve' controversy had a bearing on what had been the most salient debate in British economic history during its golden age of the 1960s, between 'pessimists' and 'optimists' about the effects of the Industrial Revolution on the living standards of British workers. It was the height of the Cold War. Led in Britain by Max Hartwell, the 'optimists' regarded rising standards of living as the vindication of capitalist industrialization. The 'pessimists' were led by two brilliant left-wing historians, Eric Hobsbawm and E.P. Thompson, who were not comfortable with numbers and stressed the unmeasured detriments of industrial and urban life. Hartwell was somewhat more numerate, but the debate drew mainly on the fragmentary surviving evidence of prices and wages. By the 1970s it seemed that the optimists had won it on points. Charles took no part in this debate. He was not yet an historian.

After Charles's work on capital formation, wages and profits remained the main components still missing for estimating national income during the Industrial Revolution. Estimating real wages had several attractions for Charles. His nineteenth-century wages series had come almost entirely from the work of predecessors. He now decided to reconstruct the series himself from the primary sources upwards. He had previously questioned the 'optimistic' assessment of wage growth represented by Williamson's book. Another approach to the standard of living had emerged in the 1990s. Human heights appeared to provide an index of well-being which might capture childhood deprivation. Historical height data assembled by Floud, Wachter, and Gregory showed that during the first half of the nineteenth century, when the standard of living might have been expected to rise, heights had actually diminished. Some optimists retreated into an ad hoc defence that the decline in heights might have reflected a preference for other goods rather than those that enhanced health.

Charles extended wage series back to the eighteenth century. Four articles on wages came out between 1990 and 1998, and the effort culminated in his magisterial Tawney Lecture of 1998, 'Pessimism Perpetuated' (Feinstein 1998). This (and the article which followed) re-examined Williamson and concluded that the pessimists had been right all along: real weekly working-class earnings lagged far behind the growth of the economy during the first 75 years of modern economic growth and broadly stagnated until the 1830s. It may also be seen as the closing of a circle, bringing Charles back to the social-justice preoccupations of his youth, and, for those who only knew him later, an unexpected alignment with the left-wing historians of the 1960s. Charles himself denied that this was his intention—he did not know, he said, where the findings would lead—but he was not dissatisfied.

A more open return to his roots had already begun. As an opponent of apartheid, Charles had never been able to return to South Africa. He was thrilled by its overthrow in 1990, went back to visit, and felt the tug of his youthful ideals. Starting in 1992, he began to spend the summer vacation months in South Africa. In most years, he taught a course in economic history at the University of Cape Town. He and his wife Anne purchased a house on the slopes of Table Mountain, which became a source of pleasure and a base from which to participate in the country's transformation. Anne Digby was a noted social historian, and she also began to work on South African topics. After retirement from Oxford, even more of their time was spent there. In 2002, he began an overview study of South African economic history. None had been written for a long time. The end of apartheid offered a chance to understand the distortions it had inflicted. He presented the Ellen MacArthur

Lectures in Cambridge in 2003 and converted them into his book on South Africa the following year. It appeared just too late for Charles to enjoy the praise it received (Feinstein 2005).

When Charles retired in 1999, he stood at the pinnacle of the discipline, admired, and respected by a wide circle of colleagues and students, family, and friends. He was celebrated with a retirement conference in 1999, and a *Festschrift* in 2003 (David and Thomas 2003). Retirement was darkened by illness, which he endured with stoic courage. But the burdens of administration had lifted, and those final years became among his most productive. In addition to the South African volume, and to a pioneering statistical textbook for economic historians (Feinstein and Thomas 2002), he embarked on another grand project, a ‘social accounting matrix’ for the UK in 1851.

Input–output analysis had been devised by Leontief in the 1930s to trace the supply chains in the economy and their mutual interaction. In the 1960s Stone recast the national accounts in the form of an extended input–output matrix, which included productive factors, households, intermediaries, government, and international flows in addition to industries. This was adopted into the UN national income standard in its revision of 1968 (Bos 1992: 24–25 and Vanoli 2005: 169–171). Effectively it was also a form of general equilibrium analysis, and highlighted similarities with the neoclassical equilibrium modelling approach, with which of necessity it coincided at many points. In its painstaking, encompassing detail, Charles’s final project was nevertheless a valedictory statement of his belief that understanding was not to be found in overarching theory, but in empirical knowledge, disciplined within an accounting framework. A complete input–output table for 1851 would push our secure knowledge of the economy back by more than half a century. It was four-fifths completed at his death, and is due to be finished by his long-standing collaborator Mark Thomas (Feinstein and Thomas forthcoming).

It was not easy to get to know Charles as a person. Despite unmistakable warmth and sympathy, he was private and reserved, and small talk was often an effort. During his last few months, he let down his guard a little, and we had several long conversations. There was a cultured side to Charles, a love of music and literature (Haydn, Bruckner, Paul Celan), which he kept hidden from colleagues. Now he was rereading the novels of Trollope, and we talked about them. It has struck me since that there were affinities between the two men. I think of them both as engineers, each with his own distinctive approach to the same Victorian infrastructure. Trollope, apart from being a novelist, had been a senior official at the Post Office. He devised the red pillar box, and established universal deliveries in Britain. Charles in his turn deconstructed the Victorian

economy and reassembled it as a dynamic machine. Trollope's novels convey a sense of stability and order. Underneath the surface narrative, society rolls along in firm grooves of convention, habit, and self-interest. Charles revealed a similar incremental regularity in economic life. But on almost every page, Trollope also wove in the nuance of emotion and personality, without succumbing to sentimentality. Charles had a similar kind of emotional intelligence in his leadership roles, robust, broad-hearted, and broad-minded. This underpinned his authority as a colleague and administrator, and his appeal as a teacher and friend. He was not only respected, but also loved. He was an immensely able man, and also a good and moral one. His accomplishment endures.

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Amartya Sen (1933–)

J. Gay Meeks

1 Introduction

One day there will have to be another chapter. Sen's path-breaking contributions happily show no sign of slowing down in his 82nd year, so this account of them must be incomplete. But there is no disguising the fact that it will anyway suffer from incompleteness. This is partly because of the difficulty of doing justice to his vast existing list of publications in this limited space, with the permitted bibliography very restricted.¹ But it is not only that space is short and that more generally, as Sen says, 'description inevitably involves selection' (Sen 1980a: 367). It is also that so wide-ranging a list of writings invites diverse views on how best to choose.

That is not the only difficulty this author faces in writing this chapter on Sen for *The Palgrave Companion to Cambridge Economics*: in seeking to do justice to him, it is not straightforward just to focus on Cambridge or just on economics either.

¹A familiar disclaimer in accounts of Sen. The omissions here will still seem glaring to some, so, as Sen once put it in relation to creating an unusually long list, 'I am steeling myself against elevated eyebrows' (Sen 1992: xiv).

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Take location first. Sen was awarded the Nobel Prize in Economic Sciences in 1998 for his contribution to welfare economics, and there is a strong sense in which all Nobel Laureates, wherever they are based, are world figures. But, further and more perplexingly for the purpose, this is a man so intimately linked with three continents that he truly spans East and West. A citizen of India, born in 1933 in Santiniketan, West Bengal, he gained his early education in Dhaka (his family's traditional home) and Santiniketan. He took his first degree (BA 1953, Economics major, Mathematics minor, with First Class Honours) at the University of Calcutta (Presidency College) and writes in his Nobel Laureate biographical note (Sen 1998, on which I lean heavily in this section) that this was a very formative period.² He found the academic ethos of Presidency College 'captivating', enjoyed the excellent teaching and the mathematical approach to economics, and was bowled over by reading Arrow's newly published *Social Choice and Individual Values*. During this period, Sen's thinking was also influenced by the political situation in India: this made him acutely aware of 'foundational disputes' between single-minded pursuit of distributional equity on the one hand and tolerance of plurality on the other, foreshadowing the central focus of his work in economics.

It was at this point that he came to Europe; and here begins his association with Cambridge economics. Notwithstanding his BA, he was entered for the Tripos³ (BA 1955) before admission to the PhD (awarded in 1959). He pays tribute to outstanding fellow students and staff whom he had met at Calcutta, and in Cambridge too he welcomed the chance to meet some exceptional economists among student contemporaries who shared his interest in foundational issues (including Samuel Brittan, Mahbub ul Haq, Michael Nicholson, Luigi Pasinetti, Pierangelo Garegnani, Luigi Spaventa, Geoff Harcourt, and Charles Feinstein), as well as among the Faculty. But, with the ideals of toleration and pluralism he had brought with him from India, he did not find the atmosphere in the Faculty congenial, calling this a 'desert of constant feuding' (between proponents of Keynes such as Richard Kahn, Nicholas Kaldor, and Joan Robinson and sceptics such as Dennis Robertson, Harry Johnson, and Michael Farrell). This was an awkward battleground to traverse for someone with 'close relations with economists on both sides', only a little moderated by the presence of 'a number of fine teachers who did not get very involved' (including Richard Stone, Brian Reddaway, Robin Matthews, Kenneth Berrill, Aubrey Silberston, and Robin Marris).

Trinity, his chosen college, was a welcome 'oasis'. There, a much more liberal spirit of friendly academic exchange prevailed among the Economics

² Also drawing on his 'Conversation' with Klammer (1989). Unattributed quotations in this Introduction come from that autobiographical note (Sen 1998).

³ Noting 'quite a drop' in sophistication in the economics taught, a 'common sense' approach being favoured (Sen in Klammer 1989: 137).

Fellows—Maurice Dobb, Piero Sraffa, and Dennis Robertson—in spite of their radically different political and methodological views. Sen ‘had the good fortune of working with all of them and learning greatly from each’.

However, he relates that from Joan Robinson, his admired and ‘brilliant’ but also ‘vigorously intolerant’ PhD supervisor, his views often met with ‘stern reproach...for not being quite true to...neo-Keynesianism’. He had not been able to find an eager supervisor for his preferred thesis topic—the theory of social choice⁴—so, encouraged by Dobb, had settled instead on studying choice of techniques in relation to development planning. This marked the start of his extensive research on economic development, referred to in the Nobel citation. The main results in the thesis having been established remarkably swiftly—in a single year—but an elapse of two more years being needed before submission, he was then able to arrange leave from Cambridge, with the appointment of Amiya Dasgupta as his (contrastingly tolerant) overseas supervisor as he returned—or was it escaped?—to Calcutta.

In India, he was appointed at 22 to a Chair in Economics at the new Jadavpur University (not without, as he puts it, an ‘understandable...storm of protest’). But before long he was partially back in Cambridge, as his forthcoming thesis had won him a four-year Prize Fellowship at Trinity, giving him freedom to pursue research of his own choosing. While relishing this opportunity, he again punctuated his time back in Cambridge with a year’s leave, taking this in 1960–1961 at MIT (incorporating also short visits to Stanford and other American universities). He found MIT ‘an inspiring place’, enjoyed discussions with Paul Samuelson, Robert Solow, Franco Modigliani, and others, and writes that it was ‘a great relief to get away from the rather sterile debates that the contending armies were fighting in Cambridge’.

Perhaps then it is not surprising that, after his Prize Fellowship ended and after teaching for two years as Lecturer and Fellow of Trinity, in 1963 he left Cambridge for India once more, becoming Professor of Economics at the Delhi School of Economics and the University of Delhi—where he found the ‘intellectual atmosphere...dynamic’—with further visiting spells in the USA, this time at Berkeley and at Harvard (where, together with Kenneth Arrow and John Rawls, he taught a renowned joint course on social justice). During these years, he was able to carry out the important work on the theory of

⁴Robinson slammed this as ‘ethical rubbish’; Sraffa was ‘skeptical in a friendly way’; and Dobb, though first arousing Sen’s interest in the problem and helpful in discussion of it, still ‘did not share [the] enthusiasm’. Robertson, seen as ‘the natural heir’ to Marshall, thought the area ‘sensible’ but was firmly wedded to a traditional cardinal utility approach (from Sen in Klamer 1989: 138–140). Sen acknowledges Robertson as important to him in scepticism about the behaviourism of revealed preference—perhaps he may also have been significant in exemplifying a memorably witty way of writing? See Robertson (1952).

social choice that the rather different research interests of staff at Cambridge had precluded, his masterful text *Collective Choice and Social Welfare* being published in 1970.

As Sen commented, looking back, ‘the peripatetic life seems to suit me’. The facts certainly bear this out, for he returned to the UK in 1971—but this time to the ‘gratifying’ intellectual atmosphere of the London School of Economics, where he held a Professorship until 1977. Next came a move to Oxford, where he was Professor of Economics and Fellow of Nuffield for three years, before holding the Drummond Chair of Political Economy at All Souls. He travelled to Cambridge in 1985 to deliver that year’s Tanner Lecture at Clare Hall, on ‘The Standard of Living’ (Sen 1987a). Oxford was where he remained until 1987, when a very sad personal loss led him to change continent again, returning to America and soon to a sustained time at Harvard as Professor of Economics and Philosophy and as Thomas W. Lamont University Professor.

In 1998, Cambridge came into its own once more in a new way when Sen was appointed Master of Trinity (where he remains a Fellow). As mentioned above—happily for Cambridge—it was also in 1998 that he was awarded the Nobel Prize. Among his publications during his Trinity tenure were the highly influential books, *Development as Freedom* (Sen 1999a) and *Rationality and Freedom* (Sen 2002). I believe he was not much involved in Faculty affairs, but he gave a series of lectures on rationality and he was working in Trinity on ideas that would subsequently appear in *The Idea of Justice* (Sen 2009).⁵ But this full-time Cambridge period came to an end in 2004 when he resumed his post at Harvard, which he holds at the time of writing (though still returning to Cambridge each year, with research projects held at the Centre for History and Economics, first at King’s, then at Magdalene).⁶

During the sequence of later moves in his career, Sen has never forsaken his very deep ties with India: remaining an Indian citizen, he frequently heads back to the mother country.⁷ He calculated in 1998 that ‘after my student days in Cambridge in 1953–1956, I guess I have never been away from India for more than six months at a time’; and in recent years, he has been heavily

⁵ Initial ideas were presented at Columbia in 1984 (see Sen 1985a), concluding ones at Stanford in 2008; the work was done in both Harvard and Cambridge (Sen 2009: xxi).

⁶ First on democracy, then on India in the global world—see his books (not covered in this chapter), *The Argumentative Indian: Writings on Indian History, Culture and Identity* (Sen 2005a), *Identity and Violence: The Illusion of Destiny* (Sen 2006a), *An Uncertain Glory: India and Its Contradictions* (Drèze and Sen 2013), and his introductory essay (‘Violence and Civil Society’) to the Report of the Commonwealth Commission he chaired on Respect and Understanding (published in *Peace and Democratic Society* (Sen 2011a)).

⁷ As well as Bangladesh.

involved in pursuing a ‘life-long commitment’ to re-establish the ancient centre of learning at Nalanda. This culminated in his appointment (2012–2015) as the first Chancellor of the new Nalanda University.

A peripatetic life indeed!

In sum then, Cambridge shares with Calcutta the honour of being Sen’s *alma mater*. He often returns, has held several Cambridge posts (one at the start of his career, the others after a long time away), and—adding up the various spells—has amassed a good many Trinity years. Cambridge can safely claim parentage of his doctoral work with its application to economic development and—very much through the role of his College—has a particular stake in the special character of his economic thinking and in his recent books. But I must be careful in speaking of his relation to Cambridge economics not to provoke protests across the globe from the prestigious universities mentioned above that fostered his ideas and made him their Professor.⁸ His national and international awards, honours, and professional roles form a dazzling array, too numerous to list—although the most recent I am aware of (the inaugural Charleston-EFG John Maynard Keynes Prize) gives another Cambridge connection, with Sen in acceptance celebrating ‘the economic wisdom Keynes brought us’ (Sen 2015), and about to give the prize lecture on—in an echo of Keynes’s title of 1919—‘The Economic Consequences of Austerity’.

Take the discipline issue next. Just as Sen cannot be pinned down to one university, country, or continent, neither is his leading work confined to economics. He is a distinguished philosopher—witness his Professorships at Harvard. His interest in the subject is by no means confined to its relevance for economics (he remarks: ‘While I am interested both in economics and in philosophy, the union of my interest in the two fields far exceeds their intersection’), but what he has drawn from the intersection of the fields has been highly distinctive. Fortunately for this chapter, that interdisciplinary aspect of his approach to economics has strong roots in Cambridge, for these dual specialisms date back to his 1959 election to the Prize Fellowship at Trinity. As already noted, this allowed him, as he gratefully acknowledged, ‘four years of freedom to do anything I liked (no questions asked)’: what he liked⁹ was

⁸ Martins’s *The Cambridge Revival in Political Economy* (Martins 2013)—springing from a lecture by Harcourt on ‘The Cambridge Tradition’—links Sen’s economic thinking to Cambridge roots, drawing on the connections with Dobb, Sraffa, and Gramsci (on which see also Sen 2009: 119–122), while Boianovsky (2014) explores links with Robertson. Sen acknowledges these influences as significant, but the present chapter—whose theme might invite a Cambridge economics over-possessiveness of Sen’s work as a whole—is concerned to allow also for his stress on the crucial role of influences from elsewhere.

⁹ In this ‘influenced...by Sraffa...and Broad...encouraged by...Dobb and...Robertson’ (Sen 2009: xxi).

to branch out into philosophical analysis, broadening his study both because of the intrinsic value of the subject and because of its bearing on the logic of social choice, as well as on related issues of equity and democracy that had already captured his attention in his student days in Calcutta. Gaining this professional expertise in the second discipline meant that thereafter he has been able to bring insights from philosophy to illuminate economic issues as a true specialist in both fields, a position that, though historically familiar, had become very uncommon in the 1960s and 1970s and which he has done much to re-establish. The significance of this is recognised in the Nobel award: 'By combining tools from economics and philosophy, he has restored an ethical dimension to the discussion of vital economic problems' (Nobel Prize website 1998).

There is another connection to the Cambridge Economics Faculty here, for Sen's work in this area furnishes much of the material in an optional M.Phil. paper in the philosophy of economics which has been running continuously since the early 1980s.¹⁰ This was still an unconventional subject area at the time of its launch, but Sen (then in Oxford) encouraged its pursuit; and in Cambridge, Professors Singh, Deane, Hahn, and Matthews all supported my establishing the new course.

In the light of Sen's academic biography, this chapter takes licence to focus on the role the early philosophical impetus derived in Cambridge has gone on to play in his work. So space is given to a selection of early papers, each with a philosophical dimension, in which Sen's constructive questioning of standard economic presumptions began, and which led up to the influential, philosophically rich, volumes appearing in or worked on during his Trinity Mastership. Sen frequently refers back to these feisty early arguments and the debates they provoked, and his tenacious pursuit of the main lines of thought—from first moves pinpointing the problematic issues, through a brimming series of extensions and adjustments, to a major drawing together of all the elements of the case—allows some short-circuiting of description of the intervening play. But even within the selected portion of the chosen strand of Sen's economic thought, the sample presented is restricted, perhaps biased, in that the arguments highlighted are those the author reckons best geared to give the flavour of the rest and tempt readers to explore further, by representing a central aspect of his very distinctive method of approach and showing how it has sparked new areas of debate. So extensive is the printed menu of Sen's work that, for all the restrictions imposed on it here, the diet remains really rich.

¹⁰In Economics until recently, now in Development Studies: I am grateful to many graduate students from both for highly stimulating discussions of Sen's ideas.

The unifying theme is the importance of gaining a better understanding of the role of reasoned scrutiny in individual and social choice, allowing a broad informational base. Within this, the material is split into three sections. A relatively brisk section relates to the years 1960–1970, whisking past choice of techniques and growth theory to give brief witness to a powerful rescue of social choice theory, meeting Nero and the impossible Paretian Liberal. The next section introduces the Rational Fool, taking Sen's discussion of values and reasons extending beyond self-interest as illustrative of and central to his critique of standard modelling of choice behaviour. The last and longest section has Sen pummelling Bentham and (more politely) Rawls as he builds up the case for addressing a great variety of issues from the alternative perspective of capabilities. With economic development seen as the expansion of capabilities, in the sense of freedom to choose the life one has reason to value, it becomes evident that in the development of Sen's thought, the 'foundational disputes' that caught his teenage eye have never been far away.

2 1960–1970: From Optimum Savings to Social Choice

It would be an interesting exercise to establish how many of Sen's methods and ideas can be detected in some way in his doctoral *Choice of Techniques: An Aspect of the Theory of Planned Economic Development* (Sen 1960)—formal proofs not allowed to drown out intuitive explanation, spurious mathematical precision challenged, utility measurement and comparison queried, divergence of market and social values explained, choice central with unavoidably political aspects recognised, assumed homogeneity of developing countries debunked, and more. The substance of the thesis is summed up with customary clarity by Atkinson:¹¹ by creating an elegant analytical framework for discussing choice of technique in circumstances of scarce capital but plentiful labour, it establishes 'a practical tool for investment appraisal'¹² in the determination of a shadow price for labour that takes into account 'the full economic implications of employing an additional person...[which may include] reduction in surplus for investment' (Atkinson 1999: 184). With continued work in the 1960s on investment and distribution in neoclassical and Neo-Keynesian models of growth, Sen was the ideal editor of a collection of papers on growth theory in the Penguin Modern Economics Readings series (speaking to 'both sides' of the discipline now

¹¹ See also the informative summary in Riskin (2006: 541–542).

¹² Followed up in a UNIDO manual of 1972 that Sen co-authored.

an advantage, together with rare skill in conveying mathematical results in non-technical language): *Growth Economics* (Sen 1970a) with its fine selection of papers and wonderfully clear and exciting Introduction became the indispensable guide to this subject.¹³

Meanwhile, Sen was vigorously pursuing his prime interest in social choice theory, publishing more than ten papers relating to it during the decade, crowned by the classic volume mentioned in the Introduction, *Collective Choice and Social Welfare* (Sen 1970b). Stylistically, it experiments with alternation of formal chapters proving results with informal ones explaining their significance—surely a pronounced experimental success¹⁴—but that is not its key claim to fame. Having set himself the task of investigating ‘the relation between the objectives of social policy and the preferences and aspirations of members of a society’ (ibid.: 1) in the wake of what appeared to be the gloomy implications for this of Arrow’s Impossibility Theorem,¹⁵ Sen identified precisely where and why such problems arose,¹⁶ explored the consequences of relaxing each of the conditions underlying Arrow’s result (generating a substantial technical literature, some signalling new problems), and restored a degree of hope to social choice theory with a groundbreaking argument about the possibility of making partial interpersonal comparisons, a move sufficient to overcome the impossibility. He argued also for bringing other forms of information to bear,¹⁷ and he introduced the concept of ‘non-basic’ value judgements in which the judgements reached are evidence dependent. The ‘far-reaching impact’ of this work was emphasised in the Nobel citation, to which the reader is referred for a fairly compact formal treatment (Nobel Prize website 1998)—and to Sen’s Nobel Lecture for a wider and more informal one (Sen 1999b); see also helpful coverage of this very extensive literature in Sen (1977a, 1986, 2011b), Atkinson (1987), Suzumura (2002), and Anand (2008). Sen concludes *Collective Choice and Social Welfare* with the comment that while ‘pure’ systems of social choice may have theoretical appeal, it may be more useful to work with the ‘impu-

¹³Including as it does a key extract from the other ‘bible’ on this, the Hahn and Matthews survey of growth theory (Hahn and Matthews 1964).

¹⁴As the Nobel Committee judged, ‘the style...provided a new dimension’ (Nobel Prize website 1998).

¹⁵The proof that, for a finite set of individuals and a finite set of at least three social states, if certain seemingly mild and reasonable conditions (unrestricted domain [sometimes expressed through two axioms], Pareto efficiency, independence of irrelevant alternatives, and non-dictatorship) are to be simultaneously satisfied, there exists no social choice procedure for aggregating individual preferences into preference orderings for society as a whole.

¹⁶This included, for instance, establishing necessary and sufficient conditions for the existence of a choice function under majority rule (possible inconsistencies in the majority method having been exposed in the eighteenth century by Borda and Condorcet).

¹⁷See Morris (2009: 5).

rities’ of ‘partial interpersonal comparability...partial cardinality...restricted domains...intransitive social indifference...incomplete social preference...and so on...while purity is an uncomplicated virtue for olive oil, sea air and heroines of folk tales, it is not so for systems of collective choice’ (Sen 1970b: 200).

Two especially memorable illustrations of how supposed ‘impurities’ can factor in are selected here. First, on interpersonal comparisons, observing that influential previous discussion had failed to distinguish ‘between *some* comparability and *total* comparability of units’, Sen presses home the point by considering what might reasonably be concluded about aggregate Roman welfare when Rome was burnt ‘while Nero played his fiddle’. The judgement that ‘the sum total of welfare went down’ assumes some comparability, but the comparisons need not be precise, and ‘one-to-one correspondence’ of every Roman’s welfare units is certainly not required. (Similarly, neither precise knowledge of individual welfare functions nor exact correspondence between welfare units is necessary for it to be reasonable to assert that the sum total of welfare under the existing distribution of money income ‘is less than what could happen with a more equal distribution’ (ibid.: 99–100; italics in original).)

Second, the book’s discussion of ‘non-utility’ information¹⁸ draws, among other cases, on a now famous example from a paper of the same year through which Sen had shown its relevance by presenting an impossibility result of his own. This was ‘The Impossibility of a Paretian Liberal’ (Sen 1970c). Suppose a prudish character who would prefer not to read an available copy of a licentious book nevertheless prefers reading it himself to opting for this access to depravity to be given to an individual who is already all too lewd. But the lewd person, though eager enough to read such a book, nevertheless would prefer that the prude be given so eye-opening an experience. Then it is apparent that if the book is to be read by one of them, both individuals happen to prefer that the reader should be the prude—a Pareto-superior outcome, then, to its being read by the lewd. Yet on liberal principles, what a person reads, there being no harm to others, is a matter for that individual’s own preference only, so the book would be read by the lewd person who relishes the prospect, not the prude who does not. In these circumstances, it is impossible simultaneously to meet both Paretian and liberal requirements. This hypothetical combination of attitudes neatly demonstrates then that, when preferences have what has become known as a ‘meddlesome’ dimension (here by including the private/personal reading materials of oth-

¹⁸ Though as Sen allows, this description might be challenged under the broader utilitarianism of John Stuart Mill, that champion of liberty—the broadening indicating a recurrent form of defence of utilitarianism.

ers within their domain), this means that the potential for conflict between Paretian considerations and liberal principles cannot be eliminated.

This result led to much discussion. Various escape routes from the impasse, of varying subtlety, were mooted and are reviewed in Sen (1983, 1999a: 363–364, 2009: 309–314). One line would be just to side with the stated preferences: if this is what the individuals do in fact prefer, would it not be reasonable—even liberal—to indulge them? But, as Sen points out, without entering into some form of contract—peculiarly in the context—the prude might cheat by merely trying to *look* as if he is reading; and also this ‘solution’ involves sidestepping the issue by means of a reinterpretation of what liberalism requires. (Equally one could simply side with liberal principles, but again this seems something of an evasion.) Sen argues that the resolution is to recognise the conflict and face up to evaluating priorities, accepting that a political dimension enters in (Sen 1999b: 364).

An alternative path some others had taken was to eliminate the blockage by disallowing meddlesome—anti-liberal—preferences in welfare calculations. This move chimes in with a more general tendency to resort to preference ‘laundering’ or ‘cleansing’ to rescue utilitarian thought from problems it can face over inclusion not only of illiberal but also of misinformed, miscalculated, short-term, externally swayed, antisocial, even vicious, preferences. There is a fine account, complete with devastating thrusts, of this sort of procedure in the Introduction to *Utilitarianism and Beyond* which Sen edited jointly with eminent Cambridge philosopher Bernard Williams (Sen and Williams 1982: 1–21). In the case of misinformation or lack of care in reasoning, the adjustment of an individual’s preferences towards what are deemed her ‘true’ preferences leaves a very significant problem—‘a “true” preference is not necessarily an actual preference at all’ (ibid.: 10)—while more radical laundering in line with what are conceived to be preferences consistent with morality begs more questions still.

The Sen-Williams Introduction gives a succinct and pivotal insight into Sen’s dissatisfaction with the utilitarian underpinning of the foundations of much of economic theory, building on earlier themes and presaging future ideas, with a focus on the informational limitations of both utilitarian and Rawlsian theories. In the utilitarian context, with respect to judging states of affairs solely in terms of ‘people getting what they prefer’ (ibid.: 3),¹⁹ several perils were identified. One was that of reducing ‘all interests, ideals, aspirations, and desires’ to factors that are regarded as ‘on the same level, and all representable as preferences, of different degrees of intensity perhaps, but otherwise to be treated alike’ (ibid.: 8). Another came in characterising utility in a dual way, defined on the one hand

¹⁹The brand of utilitarianism most familiar in modern economics.

in terms of choice and on the other as having ‘a particular content...usually in the form of maximising the person’s “self-interest”, or “well-being”’ (ibid.: 12). A third was the loss of information on welfare levels through focus on sum-ranking only. And a fourth was the malleability of preference under social conditioning. In the Rawlsian context, while the shift away from derived utility to a concentration on the availability of rights and opportunities was welcomed, an informational gap came in insufficient recognition of differences in people’s needs. I take up Sen’s development of these themes below.

3 Choice, Self-Interest, and Commitment

In a forceful critical analysis, Sen challenges the standard picture of rational choice in which this is narrowly viewed as (roughly) a matter of an individual ranking attainable options in a complete and transitive preference ordering and selecting one to which no other is preferred. It is a neat, perhaps again a ‘pure’, picture but one which in his view sells reason short.

Sen’s critique has many branches, compellingly drawn together in the Introduction to *Rationality and Freedom* (Sen 2002). Here I focus on what I take to be the central limb, brought to prominence through the outspoken thrust of a famous early paper, ‘Rational Fools: A Critique of the Behavioural Foundations of Economic Theory’ (Sen 1977b). This branch of Sen’s case has attracted the hatchets or at least the secateurs of dissenters in a still ongoing tussle. Pettit, a relatively restrained opponent, explains what he takes to be at stake, writing: ‘One of Amartya Sen’s most distinctive claims, and perhaps also one of his most controversial, is that there is an altruistic attitude toward others that does not make sense within the terms of rational choice theory, however broadly...construed’ (Pettit 2005: 15). Pettit views Sen as ‘mistaken’, but on a ‘charitable’ reading guilty only of ‘terminological infelicity’, exhibiting ‘idiosyncrasy’ rather than ‘intellectual oversight’ (ibid.: 31–32).

But whose terms are infelicitous? Sen contends in ‘Rational Fools’ that the term ‘preference’ is ambiguous: in preferring A to B, you may simply be choosing it or you may be judging that A will make you better off (if in terms of pleasure, by giving you pleasure directly, or else indirectly through help to someone with whom you are in sympathy).²⁰ The risk then comes from eliding the two possible meanings, so that whatever is chosen is simply assumed to make the chooser better off, when in fact choices may be made for

²⁰Hausman (2012: 21–22) appears to be dazzled by the first meaning when he alleges Sen is ‘mistaken’ in seeing the second.

reasons that are not to do with one's own welfare—out of what Sen calls 'commitment' (Sen 1977b: 326).²¹ Sen holds that 'much of traditional economic theory' makes this elision, thus neglecting the distinction between the preference concepts and assuming 'the invariable pursuit of self-interest in each act', whereupon (provocative words) 'the *purely* economic man is...close to being a social moron...a rational fool decked in the glory of his *one* all-purpose preference ordering' (ibid.: 329, 343, 336; italics in original).

Sen's point was developed in a significant series of further contributions. For instance, an initial example (ibid.: 328–329) of Boy 1 offering Boy 2 the chance to choose a larger apple from a pair, but miffed when Boy 2 does so, successfully showed Boy 1 had not made the offer from sympathy but left a question hanging about his commitment—did his discomfiture come from affront to the etiquette he had felt one should be committed to or from frustration of a sneaky strategy to get the apple for himself without seeming impolite? In his important 1997 paper contrasting maximisation with optimisation,²² Sen explored game-playing possibilities of this sort in a familiar case of choosing fruit from a dinner party bowl, under the heading 'Strategic nobility'. Go to that paper also for illustration of a range of possible reasons, some corresponding with commitment, for not rushing to take the single comfortable chair available at a garden party (though perhaps—in what Sen calls 'chooser dependence'—content to occupy it if specially ushered to it by the host).²³

Some of the examples on which attention has focused—the 'hanging crimes' of college life—may seem on the trivial side, but to mount objection on that score would be to miss the nature of philosophical thought experiments, in which keeping distraction surrounding more harrowing matters out of examples designed to pinpoint issues of principle can be very much the name of the game. Many weightier applications are offered by Sen, both in relation to remarkable individuals—'We do not have to be a Gandhi, or a Martin Luther King Jr, or a Nelson Mandela, or a Desmond Tutu, to recognize that we can have aims or priorities that differ from the single-minded pursuit of our own well-being only' (Sen 2009: 18)—and in relation to major economic concerns, such as work motivation (comparing Japanese experience with that in other developed countries), and environmental responsibility towards other species and future generations (Sen 2005b: 9–12).

The 'rational fools' assertion met from the first a prickly reception. Careful criticism did not make the mistake of supposing Sen was insulting the actual par-

²¹ Sen allows for committed behaviour of negative, as well as positive, type.

²² Buridan's ass had no way of optimising but if content to maximise, Sen argues, need not have starved.

²³ Here too is discussion of 'menu dependence' (as Sen illustrates this, you would choose to visit your acquaintance to take tea until you realise that cocaine is also on the menu (see also Sen 1993: 502)).

ticipants in economic markets by dubbing them fools: the target of the epithet was the folly of an economic tradition that in lacking sufficient structure tended to prompt much too narrowly conceived a model of man. But there was, and still is, widespread objection to the claim that the conception was indeed narrow.

Hahn, an early challenger, did not disagree about variety of motivation: ‘Clearly Sen is correct at a level of analysis which concerns states of mind’, continuing however, ‘But I think he has confused others...when it comes to action’. In the reaching of a decision, will not any commitment considerations enter, along with other influences, into the formation of a person’s preference ranking, whereby the merits of available options are weighed against each other? Then the standard model, encompassing this process, would not presume shallow thinking: it is a matter of drawing the thinking together—as Hahn put it, of having ‘an integrated personality’ (Hahn 1991: 7–8).²⁴ Recently, Hausman has vehemently defended the standard model of choice against Sen on very similar grounds, agreeing with him on motivations, disagreeing on how to model them, and insisting that ‘economists should employ a single notion of preference’ as ‘total subjective comparative evaluation’—preference, all things considered (Hausman 2012: 21, 64).²⁵ But Sen, who is not unsympathetic to viewing maximisation, generously understood, as a necessary (though not sufficient)²⁶ condition of rationality, is already well aware of the theoretical scope for capturing factors of concern in a maximisation exercise by broadening the maximand or introducing fresh constraints (Sen 2002: 41–42). This does not eliminate the potential pitfalls he finds in not following the strategy of giving commitment²⁷ a place in the foreground (Sen 1991, 2002, 2005b, 2009): leaving it tucked away, however neatly, in the shadows increases the risk of final decisions being interpreted as self-interested after all (albeit sometimes self-interested in terms of the extended self-interest onto which Becker and—extending further—Jolls, Sunstein, and Thaler have shone a spotlight) (Sen 2002: 37–42).

Inspired by the range of ‘moral sentiments’ adduced by Adam Smith (whom he much admires), Sen urges recognition of further richness within the commitment concept itself. Commitment can take the form of choosing non-self-interested goals, such as the pursuit of social justice, and this is one aspect of rationality (‘It is the power of reason that allows us to consider our obligations and ideals as well as our interests and advantages’ (Sen 1999a:

²⁴Though perhaps revealingly he amplifies this with ‘a man knows what he wants’.

²⁵Hausman notes that ‘people sometimes use the term...“preference” in other ways’, but says that, among other reasons, ‘the justification for retaining the word “preference” for...total subjective rankings...is that it matches economic practice’ (Hausman 2012: 64).

²⁶He points to the need for reasoned scrutiny of goals—of what to maximise.

²⁷Also, when relevant, menu dependence, incomplete preferences, and process sensitivity.

272)). But commitment can also take the form of ‘following rules of...behaviour that restrain our inclination to be guided exclusively by the promotion of our own goals’, whether self-interested goals or not, thus making room to ‘let others be’ (‘Being considerate of the desires and pursuits of others need not be seen as a violation of rationality’ (Sen 2009: 193)).

Several critics have called into question the coherence of this second, norm-based, variety of commitment. One contention is that in acting so as to allow for the goals of others, you must in some sense be adopting them yourself. In rebuttal, Sen presents the imagined case of a plane passenger who is enjoying the sunshine through an adjacent window but agrees, when asked, to lower the shade/blind so that a neighbour can better pursue his goal of playing a computer game—a game which the chooser regards as silly and a poor use of his neighbour’s time: it would seem odd then to say that the chooser has adopted the playing of this game by his neighbour as a new goal of his own. Another widely voiced protest is that Sen’s second category of commitment clashes with our understanding of action, for ‘how could one ever fail to act on one’s own goals?’ (Pettit 2005: 15). If one’s original goals are ‘restrained’ (or ‘displaced’ in Pettit’s terms), must not a new goal—that of ‘being considerate’, say—have come into play? But for Sen, there is nothing out of joint about exercising restraint over one’s goals and no necessity to represent this—with, it might be held, a loss of informational richness—as itself goal-driven. He is unshaken, then, in holding both forms of commitment to be significant aspects of our exercise of reason that are all too liable to be overlooked if forced into the standard confining mould.

It could be contended that some defenders of the standard approach against Sen’s challenge have stretched the meaning of its central concepts beyond normal parlance. If you hold back from what you aimed to do, for them you are still doing what you aim. No loss there then. Again, self-interest can appear ubiquitous: as Sen put it in *Identity and Violence*, a frequent argument drawn from supposed demands of rationality—‘an allegedly knockout argument’—takes the form of asking, ‘If it is not in your interest, why would you have chosen to do what you did?’ (Sen 2006a: 21). But if every action, however seemingly unselfish, is in this sense driven by self-interest, the term surely loses any bite—worse, it risks engendering serious misunderstanding if still taken to have teeth. Terminology that better marks the range of different behaviours—as through Sen’s concept of commitment—may be felicitous after all.²⁸

²⁸ Admittedly, it can take practice to retain the names of some of Sen’s importantly distinct categories. But Cambridge M.Phil. students, set the challenge of trying to devise more readily memorable names for his illuminating but complexly interwoven concepts of ‘self-centred welfare’, ‘self-welfare goal’, and ‘self-goal choice’, found Sen’s terms very difficult to replace.

4 Bentham, Rawls, and Capabilities

Another early paper, ‘Rawls versus Bentham: An Axiomatic Examination of the Pure Distribution Problem’ (Sen 1974)—the axiomatic approach testifying again to Arrow’s influence—once more foretold future direction. The analysis concerns how a given income might best or most justly be distributed according to Benthamite utilitarianism and on ‘Rawlsian’ maximin criteria (but here relating to levels of utility, not to advantage in terms of Rawls’s preferred focal variable of ‘primary goods’—‘things that every rational man is presumed to want’ (Rawls 1971: 62)). Sen proposes three apparently reasonable axioms, capable of being met simultaneously, and then shows that neither of the leading moral systems he is comparing fulfil them all, both therefore being ‘essentially incomplete’—classical utilitarianism in focusing on the relative magnitude of welfare gains and losses, not on relative welfare levels, Rawlsian maximin in doing the reverse (and with specific concentration on the situation of the worst-off only). Sen concludes: ‘It is not surprising that the utilitarian approach and the maximin approach both run into some fairly straightforward difficulties since each leaves out completely one of the two parts of the total picture’. Then he throws down the gauntlet with his closing remark: ‘[A] more complete theory is yet to emerge’ (Sen 1974: 309).

Enter the new-named ‘capability approach’, derived from the long tradition of concern for human flourishing that extends back through Marx and Smith to Aristotle. This tradition was rescued by Sen from its prolonged era of relative obscurity (overshadowed by utilitarianism) and has been developed through a major series of publications (including Sen 1989, 1999a, 2009).²⁹ The prominent factor in his resurrection of this line of thought was recognition of the diversity of individual need.

This recognition was already apparent in *On Economic Inequality* (Sen 1973: 16–18, 79–81), based on the 1972 Radcliffe Lectures at the University of Warwick. Here, Sen presented important results on the measurement of inequality, another of the achievements the Nobel committee celebrated. An enlarged edition (1997) reprinted the original but added a substantial annexe ‘After a Quarter Century’, co-authored with Foster, which ‘takes the story...on’ (xii of the 1997 edition of Sen 1973). It updated coverage of the technical literature on measuring inequality and argued for a new index of poverty. The Nobel tribute points up the problem that traditional headcount measures could mask the degree of poverty of the poor-

²⁹For more commentary on capabilities, see the excellent short summaries in Clark (2006), Atkinson (1999), Riskin (2006), Agarwal et al. (2003), and Robeyns (2011). Morris’s introduction (2009) covers Sen’s other work too.

est: the new poverty index devised draws also on description of income distribution and on the Gini coefficient measuring income inequality—conforming to Sen’s general pattern of broadening the information base.

The 1997 edition repeats the original’s stark complaint against utilitarianism. This was that utilitarianism is ‘fundamentally...very far from an egalitarian approach’ (ibid.: 18). Sen presented the charge diagrammatically, using what became a familiar leading example—that of a person with disabilities who, it was assumed, might in some circumstances be at a marginal utility disadvantage, needing more resources to achieve the utility level enjoyed by someone else (a further assumption—which Mill might have thought harsh—being that each person’s utility is unaffected by the other’s situation, depending only on their own income). Then while an equal distribution of a given income ‘cake’ would leave the disabled person worse off than the other in terms of his utility level, the maximisation of utility overall compounds his utility deprivation by giving him less income too.³⁰ Utilitarianism’s ‘ill-deserved egalitarian reputation’ (ibid.: 16) has links with the all too heroic assumption that individuals’ utility functions were—or at least could reasonably be treated as³¹—identical: on Sen’s analysis, the doctrine becomes, as the annexe put it, ‘especially counter-intuitive when some people are better “utility producers” than others are’ and the heroic assumption breaks down (ibid.: 110).³²

Rejecting utilitarian insistence on maximising the sum total of utility (‘sum-ranking’), the original 1973 text, while retaining exclusive focus on utility information, considered other ways of utilising it.³³ But the strategic shift away from this ‘welfarist’ framework was imminent. The line of thought is aptly summarised in the later monograph *Inequality Reexamined* (Sen 1992) which sets out in detail the philosophical dimensions of the case for a focus on capabilities. While maintaining the attack on utilitarianism’s distributional implications, it also recognises a crucial respect in which nevertheless an ‘egalitarian foundation *is*...quite central to the entire utilitarian exercise’: this is that utilitarianism ‘attaches exactly the same importance to the utilities of all people in the objective function’ (ibid.: 14; italics added).³⁴ In other words, a marginal gain in the utility of a homeless person would be treated equally with—given just as much weight as—the same marginal utility gain for a princess. Sen observes that in fact ‘a common characteristic of virtually all

³⁰ Sen makes use of an axiom ruling out such a result—the Weak Equity Axiom (Sen 1973, 1974).

³¹ See Meeks (1985).

³² The annexe’s comment on ‘profound...unconcern with inequalities...in...individual utilities’ refers to individual utility levels—their ‘total utility’—not utilities ‘at the margin’ (ibid.: 110).

³³ By 1997, the annexe had brought capabilities into this book.

³⁴ Sen correspondingly here qualifies the 1973 indictment of utilitarianism as not egalitarian with ‘in some important respects’ (Sen 1992: 13, fn. 4).

the approaches to the ethics of social arrangements that have stood the test of time is to want equality of *something* for all concerned, without which the approaches would ‘tend to...lack social plausibility’—the key discriminator between them then being the question, ‘Equality of what?’ (ibid.: ix; italics in original).

Equality of What? had been the title of Sen’s Tanner Lecture at Stanford in 1979. It introduced the idea of seeking equality in basic capabilities and remains a very valuable introduction to the core reasoning behind the capability approach. Using again the case of a person with special needs as the focus of his critique, Sen expands his argument into the thesis that neither utilitarian nor ‘welfarist’ nor Rawlsian approaches are sufficient templates for social arrangements. The charge against utilitarianism is as before. Against other welfarist approaches relying on utility information alone (such as seeking to equalise individual levels of utility), Sen now points out the inadequacy of inferring need simply from mental reaction and emotional response; for what if the person with disabilities has such a ‘jolly disposition’ or perhaps such a ‘low aspiration level’ that in spite of practical difficulties faced in achieving, say, basic mobility, he is not at a disadvantage in terms of (marginal or total) utility after all? We might still want to recognise a special need for resources (just as we might want on the other hand to disallow claims of special need arising from those who ‘have to be deluged in champagne and buried in caviar to bring them to a normal level of utility, which you and I get from a sandwich and a beer’) (Sen 1980b: 217, 214–215). As to Rawls, while he had tellingly exposed the hazard in vesting moral prescription in totting up the mere utilities irrespective of their source, Sen detected an element of ‘fetishism’³⁵ in Rawls’s alternative metric of social primary goods, protesting that, although the disabled person on this goods-focused basis would not be penalised, no special provision would be made for him either (ibid.: 215–216). Hence, if the contention was that such provision should be made, ‘despite there being no marginal utility argument (because it is expensive), despite there being no total utility argument (because he is so contented), and despite there being no primary goods deprivation (because he has the goods that others have), the case must rest on something else’. That ‘something else’ is, Sen believes, ‘the interpretation of needs in the form of basic capabilities’, ‘a person being able to do certain basic things [including] the ability to move about...to meet one’s nutritional requirements...to be clothed and sheltered...to participate in the social life of the community’ (ibid.: 218).

Yet protests may be made about Sen’s critique of the rival theories’ treatment of the critical case. ‘Happiness is happiness’, the utilitarian Richard Layard might be heard to say, no matter what gives rise to it; so if a jolly

³⁵ Echoing Marx’s concept of ‘commodity fetishism’.

disposition does the trick, well and good. Sen's rejoinder, identifying deprivation here in all but feelings, would evidently be that the person concerned might yet very reasonably expect more (see Sen 2009: 273–276). Rawlsians might perhaps jibe at the allegation of 'fetishism' in the primary goods concept, the list including not just income and wealth but also the far from materialistic social goods which may go to the disabled person: '[R]ights and liberties, powers and opportunities...self-respect' (Rawls 1971: 62). But that would overlook the point, perhaps not immediately transparent in the fetishism term, about interpersonal variation in the *effective* value of all these 'goods'. The argument that diversity among human beings and in the situations in which they live leads to 'conversion of goods into capabilities [varying] from person to person substantially' (Sen 1980b: 219) will relate to conversion not only of material resources but of social, political, and legal openings as well. In the context of rights, Tawney's example of a 'merely formal' or 'nominal' right that has no 'practical power' may come to mind—'the right of all who can afford it to dine at the Ritz' (Tawney 1931: 234). In the same spirit, but taking into account the impact of a range of conversion factors much wider than ability to pay, Sen is concerned with whether opportunities for an individual are 'substantive' across the board.³⁶ The approach can be seen then, says Sen, as a 'natural extension' of Rawls's concern with primary goods, 'shifting attention from goods to what goods do to human beings' (Sen 1980b: 218–219).

A cordial to and fro between Rawls and Sen ensued. Rawls (as Sen acknowledged) had himself referred to capabilities in motivating the primary goods concept, and he appeared to feel Sen was underestimating the extent to which what he could say on the matter was very much what Sen was saying. In a protracted series of exchanges about the 'hard cases' of special need, there was evidence over the years of some convergence in view—or at least in interpretation. But the meeting of minds on this remained incomplete, and 'hard cases' were only illustrative of Sen's general stress on the variety of human needs (varying with age, sex, bodily size, state of health, season, location, work conditions, social context, etc.) requiring to be centrally addressed, not left as mere discretionary corrections.³⁷ The most fundamental divergence from Rawls's mode of thought, as Sen sees it,³⁸ was brought out in *The Idea of Justice*. Dedicated to Rawls's memory,

³⁶ The standard language of opportunity sets in economics does not bring to the fore this deep-seated role of non-income factors in giving (or denying) substance to an apparent power of choice.

³⁷ Sen (2009: 260).

³⁸ For a challenge, see Sugden (1993).

perhaps titled with his great book on justice in mind, this pays deep tribute to him personally³⁹ and to his ‘far-reaching contributions...[as] the leading political philosopher of our time’. However, Sen is also firm in favouring a profound shift in the nature of ideas of justice from those currently dominant in political philosophy—a shift away, as he puts it, from the hypothetical ideal structures of ‘transcendental institutionalism’, ‘aiming only at the characterization of perfectly just societies’ (as in Rawls), and towards ‘realization-focused comparison’, ‘a theory of justice that can serve as the basis of practical reasoning...[by including] ways of judging how to reduce injustice and advance justice’ in the world we actually inhabit (Sen 2009: ix, 7–8).

The discussion of the fetishism charge against Rawls may be carried over in part to the ‘basic needs’ approach to development (dating from, and very prominent in, the 1970s), since Sen lodges the same complaint about this; but in a spirited defence, Stewart replies that there is more room for concern with human flourishing and individual achievement in the basic needs approach than Sen allows (Stewart and Deneulin 2002; Stewart 2006). Some have seen the basic needs and capability approaches as complementary, some as having much in common and as having to face some similar problems. But with its focus on the poor, the basic needs approach has no ambition towards ‘completeness’, whereas the capability approach extends beyond concern with basic capabilities to considering capabilities in general. Stewart graciously concedes not only that ‘the capability approach applies to rich as well as poor people... [and is less] broad-brush...[as it] focuses more on individual capacities and needs...[including] the special needs of certain groups’, but also that it ‘has a much more elegant philosophical foundation’ (ibid.: 18).

Responding to both basic needs and capability approaches—drawn together in Mahbub ul Haq’s human development approach—from 1990, the United Nation Development Programme’s Human Development Index (HDI) incorporated measures of longevity and education as well as income: Sen was involved in its initial construction. Writing about it in 2006, he explains that while economic expansion in terms of a standard national income measure is important, this is ‘not for its own sake’ but for the sake of human flourishing (Sen 2006b: 257): evidence he cites in ‘Development as Capability Expansion’ (Sen 1989) and later in *Development as Freedom* shows the two phenomena are far from always going hand in hand. The HDI’s incorporation of additional information marks a strategic but still limited extension, but Sen holds it is its very ‘boorishness’ (Sen 2006b: 257), in deliberately

³⁹Rawls similarly acknowledged his debt to Sen.

making only a 'heroic selection' (ibid.: 259) of factors relevant to well-being, that enables it to pose a punchy challenge to the informational narrowness of the usual performance indicators. He also stresses that 'no one number can... capture the breadth of the human development approach' (ibid.: 257, 260). The capabilities metric can in principle offer an alternative, more subtle but complex, perspective on performance. A rival alternative might come in the form of the happiness measure favoured by Layard—but the rivalry here may be to some extent disarmed by the inclusive character of the capabilities concept, which can allow subjective measures of well-being to be accommodated alongside other factors. Sen was Chair Adviser for the Sarkozy *Commission on the Measurement of Economic Performance and Social Progress*: warning of 'GDP fetishism' as the Chair, Stiglitz, dubbed it, the 2009 *Report* gives a thorough treatment of the issues at stake.

The sensitivity of the capability approach to groups with special needs is attracting particular current attention. In a moving and powerful keynote speech at the World Bank's Disability Conference in 2004, Sen built on his earlier World Bank lectures (which became *Development as Freedom*) to argue that 'overlooking or ignoring the plight of the disabled is not an option that an acceptable theory of justice can have' (Sen 2004: 2).⁴⁰ He shows the importance of allowing for the conversion handicaps as well as the earning handicaps that are likely to affect the 'truly gigantic' number of people living with disabilities (estimated by the Bank at 600 million at that time, about 10% of the world's population). Conversion handicaps—the greater need for resources to help in leading, so far as possible, flourishing lives—substantially magnify the social deprivation of many who are already, in developing countries, 'the poorest of the poor', lacking even the most basic capabilities (ibid.: 7). This is very relevant material to the aim of tackling exclusion which has been highlighted as a desirable target for the UN's Sustainable Development Goals, due at the time of writing (mid-2015) soon to be agreed on—in succession to the Millennium Development Goals, on which Sen had surely also had a notable influence through the human development perspective and also as a result of his disturbing findings on 'missing women' (Sen 1990).⁴¹

Personal and social conversion factors are central to individuals' capability to achieve particular 'functionings'. But, as Sen has increasingly stressed, the capability approach goes beyond concern with achieved functionings, for freedom plays a key part: the focus is on achievable combinations of functionings and on a person freely choosing from among these the combination they

⁴⁰ See Qizilbash (2011a) and Sen (2009: 258–260).

⁴¹ See also Atkinson (1999).

most ‘have reason’ to value. A person’s capability set shows, in effect, their scope for choosing among alternative ways of living, and development can be viewed as a process of removal of ‘unfreedoms’.

Sen emphasises the ‘plural features’ of freedom (Sen 2009:101) and indeed alerts us to an almost bewildering array of them. Some can be illustrated through the example of the functioning of being adequately nourished, which also brings out some of the contrasts between the capability and other approaches. The degree to which one achieves this functioning will depend not just on available food but also on size, age, energy expended at work, digestive illness, and so on. This might not be at all apparent in a utilitarian or other welfarist approach (a ‘sunny temperament’ might disguise the degree of hunger, and Sen reports evidence of malnourished Indian women who did not feel themselves to be so, having believed it proper to give their husbands most of the food). Yet just going by achieved nutritional state could itself be misleading. In a case Sen often refers to, a fasting priest who freely rejects the opportunity of nourishment—out of commitment let us say⁴²—importantly still had the capability of being well fed and in his exercise of choice enjoys what Sen calls ‘process freedom’. In contrast, a famine victim, forced to be similarly malnourished, lacks in Sen’s terms both ‘process’ and ‘opportunity’ freedom. (His famous empirical study of famines (Sen 1981) analyses the sources of such suffering, finding it could arise even when food is available, sometimes resulting instead from lack of ‘entitlement’ to it, e.g. through lack of means to buy it.) On the other hand, if a state of being well nourished results from being force-fed against one’s will (say, during a hunger strike), the functioning is achieved but without process freedom. Sen’s capability perspective is in principle sensitive to and inclusive of all these factors, emphasises the importance of individual ‘agency’, and gives the possibility of including at least some process aspects as well as opportunity aspects within the capability concept.⁴³ It is in this sense that he sees development both ‘as capability expansion’ (Sen 1989) and ‘as freedom’ (Sen 1999a).

The constructive claim made for the approach by Sen himself has been a moderate one. Though reckoning to have helped to fill the gaps in alternative approaches, he does not claim to have provided a panacea. This is a project in progress, not a full and final system, and in any case emphatically offers a framework of analysis, not a hard and fast set of decision rules. I am given to understand that Sen may prefer to speak of having concern for capabilities,

⁴² Sen also makes use of distinctions between the concepts ‘well-being achievement’, ‘well-being freedom’, ‘agency achievement’, and ‘agency freedom’, which relate back to the discussion in Section 3.

⁴³ Sen’s terms ‘culmination’ and ‘comprehensive’ (process-inclusive) outcome can also be applied here.

rather than more grandly and exclusively of the capability ‘approach’. From the outset, he explained he did not hold other approaches to be ‘morally irrelevant’ (Sen 1980b: 220). He allows, for instance, that happiness matters,⁴⁴ that gains signify as well as levels, that equality is not the only factor of moral concern, that liberty counts. He is aware that ‘there are many ambiguities in the conceptual framework of the capability approach’, rejects dogmatism about them, and recognises that pragmatic considerations, for example, of data availability, will limit how far practical application can match some of the theoretical sophistications (Sen 1989: 45, 48–51).

However, the capability approach has not gone unchallenged. Doubts have been raised *inter alia* about whether it is operational, whether (relatedly) it is too broad and embraces contradictory elements (such as conflicting concepts of freedom), whether it is too narrow and misses vital factors (such as collective capabilities and power structures), whether it is paternalistic.⁴⁵

The question ‘how far Sen’s framework is operational’, sharply posed by Sugden in 1993, was answered unfavourably in the extreme by Dasgupta, who contends that ‘discussions founded on “capabilities” end nowhere’, the approach being ‘open ended’ and as a guide to policy ‘altogether too flabby’ (Dasgupta 2009: 624). Among those more sympathetic to the general direction of thought, some nervousness could still be found over workability, on several scores—including how to cope with the sheer weight of information potentially relevant to capability expansion, how to deal with the counterfactual nature of options not selected but which it was valuable to have, how to treat trivial multiplication of capabilities⁴⁶ and the existence of damaging ones, and, in forming public policy, how to judge between aggregative and distributive concerns (spilling over into the breadth worry) and between different people’s ideas of the good life (spilling over into the paternalism one).

On the other hand, testament to workability comes from a rapidly growing set of empirical findings from researchers who have found it fruitful to relate the capability perspective to a considerable variety of problems (see Clark 2006; Sen 2009: xxiv). Moreover, Atkinson (1999: 185–186) has suggested a deeper defence, writing: ‘[T]here is...more than one way in which an idea of this kind can be operationally effective ... [A] concept is effective if it causes people to think in a different way’.

⁴⁴ See, for example, Sen (2009: Chapter 13) and Bruni et al. (2008).

⁴⁵ A number of leading critical papers are gathered together in Wood and Wood (2007: volume II). See, for instance, Stewart and Deneulin (2002), Evans (2002), Gasper and van Staveren (2003), Hill (2003), and Sugden (1993).

⁴⁶ Williams (in Sen 1987a: 98) discusses the potential triviality of gaining the capability to choose yet another new laundry product—‘Bloppo’.

Sen, acknowledging complexities from the beginning, went on to address them—no doubt with some stimulus from debate with his critics. On valuation issues and policy trade-offs, he gives us a lot to digest. Some conceptual conundrums were taken up in *The Standard of Living* discussion (Sen 1987a: Reply), and some technical matters in *Commodities and Capabilities* (Sen 1985b) and in Foster and Sen's annexe to *On Economic Inequality* (1997 edition of Sen 1973). But I take the central planks in the reply to be on the broad lines that difficult valuation issues are not peculiar to the capability perspective; that putative lack of specificity may turn out to convey versatility; that it is unrealistic to hanker for completeness; that partial comparabilities may sometimes suffice; that 'fuzzy' valuation and being 'vaguely right' may be better than spurious precision;⁴⁷ that values are typically fact dependent, giving objectivity a role;⁴⁸ that it is for individuals to decide on their own weighting of functionings; and that, while social evaluation depends on some kind of reasoned consensus on trade-offs, which might indeed be elusive as a universal feature, this may well be attainable at least in the pressing cases of 'outrageously unjust arrangements' (Sen 2009: 26).

The lines of response are summed up at various points in *The Idea of Justice*. So, for example, on the issue of judging what weights to attach to capabilities in social assessment, Sen emphasises the role of 'our own continued scrutiny and...the reach of public discussion' which render it an 'absurdity' to claim that the approach is 'usable...only if it comes with a set of "given" weights on the distinct functionings in some fixed list of relevant capabilities'. Moreover, he illustrates how the policy maker's choice of weights might usefully reflect the exercise in hand—whether assessing poverty in relation to health policy or assessing 'the inequality of overall advantage of different persons' (ibid.: 242–243).

Critical demand for muscular policy rules is out of step with Sen's down-to-earth recognition of 'the possible plurality of robust and impartial reasons that can emerge from searching scrutiny', as a result of which there may be 'a good deal of incompleteness and unresolved conflicts'. Alert to plurality within each of the ideals of equality, freedom, and justice, he does not duck the fact that reasoned resolution strategies are liable to fail from time to time. In connection with competing ideas of justice, he draws on a favourite example to show that three competing claims could be supported by a justification

⁴⁷ 'For substantive social theories...it may be...both terribly limiting and altogether unnecessary to shun ambiguities' (Sen 1989: 57, fn. 14). See also Sen (1970b, 1986, 1987b).

⁴⁸ As in the original discussion of non-basic value judgements in 1970. The triple 'entanglement' of fact, convention, and value is thoroughly explored in Putnam (2002), who specifically relates Sen's development thinking to these issues. See also the fierce argument on 'entanglement' between Putnam and Walsh (2007, 2012) and Dasgupta (2007, 2009).

it is hard reasonably to reject.⁴⁹ Each of three children might have a claim on a flute, one because only she knows how to play it (getting happy fulfilment from doing so), a second because he is the poorest, and a third because she made the flute herself. On the information given, each claim has its own claim to validity, and ‘it is quite possible no unanimity may emerge’ (Sen 2009: 12–15, 201, 399).

However, as Sen points out, the flute might find its home on a modest change in the children’s circumstances, and the argument should not inhibit the wider quest for partial orderings that he encourages—writing that ‘The agreement to emerge from “a public framework of thought” can be of a partial yet useful kind’ (Sen 2009: 135). Rather, the story of the flute trio indicates his reason to be wary of rules meant to cover all cases. This restraint is evident again in his treatment of the question whether it would be ‘right to presume that we should demand equality *of* capability?’ (ibid.: 295; italics in original). It may at first seem surprising that Sen writes firmly: ‘[T]he answer is no’ (ibid.).⁵⁰ But Sen argues there is no overriding dictate to pursue equality, because aggregative concerns can matter as well as distributive ones, process freedom can be at odds with opportunity freedom, and so on.⁵¹ Tidy solutions are not guaranteed.

A further question of how to balance policy aims comes in Sen’s discussion of ‘the Lee thesis’—under which a clutch of democratic freedoms in Singapore were to be forgone for the sake of the rapid economic growth that did in fact ensue. Sen opposes the thesis on the basis of questioning the causal connections it presumed (planting a counterfactual doubt about potentially even higher growth) and placing stress on the exceptional power of those democratic freedoms (through their intrinsic worth as well as for their several instrumental values) (Sen 1999a: 148–154). But again, there are contrary concerns: Sen does not retreat from observing American democracy’s lack of success in easing the severe deprivation in many African American communities. There remain some demanding questions to pursue in this context: for instance, how might his analysis relate to the character of current Rwandan development?

The charge that the capability approach is too broad tends to focus on the wisdom of seeking to encompass such a plurality of, possibly competing, free-

⁴⁹ Following Scanlon (1998).

⁵⁰ This does not imply that there cannot be particular situations in which equalisation might be justified.

⁵¹ He also offers in support the argument that it would not be fair process to seek to reduce the prevailing disparity in male/female life expectancy rates (a ‘natural masculine handicap’ (Sen 2009: 296)), devoting more medical attention to men than to women with the same health problem. It is instructive to pinpoint how this could relate to the earlier argument on disabilities.

doms. One of Gasper and van Staveren's fears is that too general a celebration of development 'as freedom' may tend to mislead, if it obscures the fact that freedoms are heterogeneous and not all benign. They suggest paying more attention to values such as caregiving, which involve patterns of relationship and interactions of individual freedoms. In *The Idea of Justice*, interfamily distribution of duties is now discussed in terms of 'cooperative conflict' (Sen 2009: 166–167).

In that book also, in addition to the freedom concepts already mentioned, Sen explains the republican concept of freedom advocated by Pettit. This might be illustrated again through the functioning of being adequately nourished: freedom in the republican sense would be lacking if the only way of sourcing food for a housebound person was for it to be brought to her by state-funded social workers or charity volunteers, the achievement thus being dependent on the support of others. While accepting another aspect of freedom is identified, Sen is unconvinced it should rule the roost, objecting that Pettit—insufficiently pluralist—is not affording due weight to other freedoms at issue: in our example, being well nourished is achieved (and perhaps with a greater degree of agency, even if small, in comparison with the force-feeding case).⁵² He holds that republican freedom can be seen as adding to the capability perspective, not demolishing it (Sen 2009: 305–308). The book describes yet a further freedom combination for consideration too, in which loss of process freedom combines with success in terms of opportunity and achievement freedom, and where the enforced outcome (but for the enforcing) is just what would anyway have been chosen (to continue our nutrition functioning theme, the uncommon case, say, of a schoolchild who loves eating greens, but who is obliged to eat them by a disobliging dinner lady). Again, there remain some interesting puzzles to pursue here—not neglected by Sen himself—for instance, over how fruitfully the various freedoms might themselves be counted as capabilities and how usefully regarded as distinct.

A number of papers have suggested there are missing elements in Sen's framework, because its starting point is determinedly with the individual. Some have expressed this as proposing extension rather than overthrow (the framework is a forgiving one). The suggestion would then be that greater allowance be made for the impact of the power structures, global corporations, 'collective' capabilities, and so on which are mentioned in this strand of the literature

⁵²Where state-funded provision is concerned, the 'republican' view on 'dependency' as opposed to entitlement may be at odds with, say, a Scandinavian understanding. Sen considers state provision of education in this context (Sen 2009: 307–308). In the UK of 2015, with respect to the nourishment functioning set of cases, a stronger sense of unfreedom might perhaps result from enforced reliance on a food bank than from the personal care of a home help.

(see, for instance, Stewart and Deneulin (2002), Evans (2002), Gasper and van Staveren (2003), and Hill (2003)). But Sen protests in *The Idea of Justice* not only about the dimensions of his work dealing with how society affects the individual but also that he is very far from founding his analysis on the atomised individuals of methodological individualism: rather, he follows Marx in viewing individuals as necessarily social beings (Sen 2009: 244–247).

A remaining challenge came to the fore through the emphasis Sen placed on mental adaptation to deprivation, when arguing against (classic) utilitarianism.⁵³ Expanding the ‘jolly disposition’ line of thought, he had described many sets of people—including oppressed minorities, sweatshop workers, subdued housewives—who might adjust desires to adverse conditions ‘to make life bearable’ (Sen 1999a: 62–63). Then (like the malnourished Indian wife above) such an individual—say, a sweatshop worker—may profess herself content (and may believe she is content) in spite of suffering deprivation in terms of what she is able to achieve. While the capability perspective, Sen argues, corrects for welfarist blindness towards her achievement deficit, it might now seem that the same problem of mental adaptation could come to haunt the capability approach itself—as Burchardt has argued, on examining whether youngsters’ recognition of value in entrance to higher education or professional training seemed to be conditioned by social context (Burchardt 2009).

Is there paternalism here? To place emphasis on a reason to value that a person does not (yet?) accept as a reason might seem reminiscent of some of the ‘preference laundering’ Sen had questioned (in company with Williams), but he contends that valuations are naturally the subject of critical reappraisal, in a way in which feelings are not (e.g. Sen 1989: 57, fn. 13). The paternalism charge is generally agreed to be stronger⁵⁴ against Nussbaum’s version of the capability approach since, unlike Sen, she believes it necessary to draw up a list of ten capabilities regarded as essential for human flourishing. Resisting calls to follow suit, Sen is surely far from suggesting imposing valuations (though, as with social choices, fairly confident of potential rewards from reasoned scrutiny—citing suggestive evidence, for example, of anti-smoking campaigns, in support: see Sen (1989, 2009: 275)). But Sugden, a long-standing but worthy opponent of Sen, was confident in pouncing on what he seemed to see as paternalism in Sen’s position, protesting, ‘my concern about Sen’s approach is that it allows collective judgements about rational desire to override individuals’ actual desires’ (Sugden 2006: 50). Steeped as he is in the ethos of

⁵³ See an illuminating analysis of varieties of adaptation in Clark (2012).

⁵⁴ Though in the view of Qizilbash (2011b), still not very strong.

social choice theory, Sen seemed taken aback by this onslaught, writing, ‘I am surprised by the extent to which Sugden sees an odd picture of despotism in the recognition of the need for critical assessment and public reasoning’ (Sen 2006c: 88–89). In response, Sugden allowed that ‘Given Sen’s emphasis on democratic judgment, it would be wrong to suggest that his concept of ‘reason to value’ is paternalistic or elitist’ but still maintained that ‘Because [Sen’s capability] theory is grounded on the idea of “reason to value”, it does not allow a robust formulation of each individual’s freedom to choose how to live her own life’ (Sugden 2008: 305, 308). Qizilbash, carefully analysing the debate, grants much of the ground to Sen before giving Sugden the last word: ‘Sen’s... [approach] can...withstand much of the force of Sugden’s critique, [but] the unease which motivates [it] cannot be easily dismissed’ (Qizilbash 2011b: 26).

Perhaps ironically, the potential influence of powerful social groupings in distorting personal choice—alongside the susceptibility of mental adjustments to circumstances of deprivation—is a key factor motivating Sen’s emphasis on attending to what a person has ‘reason to value’, rather than simply going by what she values—or seems to value. That valuations are liable to be culture dependent had already been flagged up in *Equality of What?*; since then, a very substantial and increasing part of Sen’s work on issues of justice, freedom, and well-being has been devoted to exploring the role reasoned scrutiny can play. The weight of the many interlocking arguments now assembled and still being assembled to support that role may begin to take its toll on at least some elements of the mentioned unease.⁵⁵ *The Idea of Justice* gives more detail than the previous books on how the ideal of impartial public discussion is intended to work. Because of the perceived danger of parochialism, discussion is to be tempered by exposure to the alternative views and experiences of those from elsewhere—engagement with people who may have other values serving a function similar to that of Smith’s ‘impartial spectator’. In order to benefit fully from this ‘participatory freedom’, individuals—of both sexes—should have access to sufficient facilities for learning to equip them with ‘basic educational skills’. Sen then argues that participatory freedom and access to education cannot *reasonably* be ‘choke[d] off...on grounds of traditional values (such as religious fundamentalism)’ (Sen 1999a: 32). Malala Yousafzai, the fellow Nobel Laureate gunned down on her way to school in Pakistan, would no doubt agree with that.

But some perplexing issues may remain. While overt coercion creates doubt over how genuine apparently chosen behaviours are (except when

⁵⁵Qizilbash concludes that Sen’s recent contributions ‘go quite far in addressing...[the] worry. Sugden may not yet be convinced by the moves. But it is clear that in making them Sen...recognize[s] the unease which motivates his critique’ (Qizilbash 2011b: 51).

defined by Malala), in less transparent contexts, where any conditioning is too unobtrusive to detect, is it possible for anyone to tell—even in an open participatory process—how far the individual views we would expect to respect do reflect true agent autonomy?⁵⁶ There may be some special cases to cover too: how satisfactorily will the educational requirement linked with ‘participatory freedom’ relate to members of an ‘uncontacted’ Amazonian tribe whose priority—or whose headman’s priority?—seems to be to remain contactless? Must they lose their unrecoverable status in order to have ‘reason’ to value it?

5 Conclusion

A conclusion is asked for but, just as presenting Sen purely as a ‘Cambridge economist’ posed difficulties at the outset, so does adding an appropriate ending.

I will not be so bold as to try to sum up even this selected portion of his work, still less to ‘sum up’ Sen. But perhaps I might enlist help, referring the reader to the fine summings up by others, already mentioned. It is well worth reading why Atkinson judges him to be ‘the compleat economist’.

With this chapter’s focus on philosophical links, I note how many appreciations of Sen’s achievements mention that he has, as Atkinson writes, ‘enlarged the perspective’ of economists (Atkinson 1999: 77, 90–93). They do not usually mention that this is *brave*. Sen has broken what Leijonhufvud playfully portrays as the Econ tribe’s set of ‘taboos against association with... other tribes’. Miscreants may be shunned or pilloried for ‘endangering the moral fiber of the tribe’. Worse still, he has been seen conversing with the lowly ‘Devlops’ caste who are ‘suspect[ed]...even of relinquishing modl-making’ (Leijonhufvud 1973: 329)! Yet he could all the while, as a famed virtuoso of the formal proof, have settled in to being fêted by the ‘priestly’ Math-Econ.

There remain ongoing debates on some of the issues in this chapter, and new books and papers are in the pipeline.⁵⁷ We can look forward to those. Replete though the present list of publications is, there is much more to come from Amartya Sen.

⁵⁶ See Sumner (1996).

⁵⁷ June 2015 saw Sen delivering public lectures on aspects of persuasion and democracy.

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James Mirrlees (1936–)

Huw Dixon

1 Introduction

Sir James Mirrlees was born in 1936 at Minnigaff, just outside Newton Stewart in Galloway, south west Scotland. He developed an interest in mathematics which took him first to Edinburgh University in 1954 (direct entry into the second year) where he won a Napier Medal for his final year examinations. In 1957, at the age of 21, he went to take a second undergraduate degree in mathematics, this time at Trinity College, Cambridge. He entered directly into Part II of the Tripos and became a Wrangler and subsequently went on to Part III. It was at this stage that Mirrlees developed an interest in economics, arising from discussions with fellow students. He was put in contact with Pierro Sraffa, the University Lecturer in Economics at Trinity College. Mirrlees later wrote: ‘[I]t was indeed economics I wanted to do, because I kept discussing it with economist friends, and they didn’t make sense to me; and because poverty in what were then called the underdeveloped countries, seemed to me what really mattered in the world, and that meant economics’ (Nobelprize.org 1996).

His formal studies in economics began when he took the Diploma in Economics: a one-year ‘crash course’ constituted from parts of the final year of the economics degree. His official supervisor was Richard Stone, but he also worked initially with David Champenowne and attended lectures and

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seminars given by the Cambridge luminaries of the day: Kahn, Kaldor, and Joan Robinson. Mirrlees was pointed in the direction of Frank Ramsey's theory of optimal saving. Frank Hahn soon arrived and as Mirrlees wrote, 'at some point in all this I had discovered Samuelson and mathematical economics' (ibid.). As noted, he also discovered Ramsey's theory of saving (Ramsey 1928). However, Kaldor needed a research assistant to help him out with the mathematics, and the result was the paper of Kaldor and Mirrlees 'A New Model of Economic Growth' published in the *Review of Economic Studies* in 1962. He went on to complete his PhD in 1964 with the thesis title *Optimum Planning for a Dynamic Economy*, which included his subsequent article 'Optimum Growth when Technology is Changing' (Mirrlees 1967).

Mirrlees's first stint at Cambridge ended in 1969 when he moved to Oxford at the age of 33 to become the first Edgeworth Professor of Economics based at the relatively new Nuffield College where he remained until 1995. This was a period of great productivity, during which he wrote many important papers both with colleagues and PhD students. He had a particularly close working relationship with Peter Diamond, with whom he collaborated several times, as well as Sir Nick Stern, Ian Little, and others. Whilst he had started supervising PhD students at Cambridge—one of his first was Sir Partha Dasgupta, who graduated in 1968—his list of doctoral students in these Oxford years is long and varied, including Sir John Vickers, Jesus Seade, Peter Neary, Jiang Wei Ying, Anthony Venables, Tim Besley, Gareth Myles, and many others. He was also co-editor of *Econometrica* over the period 1980–1984.

I had the pleasure and privilege myself of being Jim's PhD student¹ in the years 1982–1985. I had done a degree in philosophy and economics at Oxford prior to going to Nuffield and so always felt a little underprepared for working with the embodiment of mathematical rigour in economics. However, he was always genial and encouraging. My thesis topic of Edgeworthian Oligopoly was not his area of expertise. However, he gently helped me understand how I could express and analyse my disparate and disorganized ideas in terms of a formal mathematical model. Part of this is to choose carefully the propositions or theorems you prove: they should distil the essence of what you are trying to communicate. I remember, after some discussion on a particular issue, he said 'now, that is a Theorem worth proving', which meant that we had found (after a few alternatives) the exact way of expressing the key idea. Then there was the proof. Once you had a theorem to prove, the task became purely mathematical. Economic intu-

¹ Or, to use the correct Oxford terminology, a DPhil student. PhD and DPhil are simply alternative abbreviations of 'Doctor of Philosophy'.

ition might be a helpful guide, but in the end a proof is a mathematical procedure of moving symbols about. Once you had the proof there was also the issue of ‘elegance’: the proof was best concise and no longer than strictly necessary. Any elegance my proofs might have I owe entirely to Jim. Thirty years later, when involved in a proof, I often think back to those supervision meetings I had with Jim in his college room.

In 1995, Mirrlees moved back to Cambridge, as Professor of Political Economy and Fellow of Trinity, and had the pleasure of living in College for six years (his first wife Gill had died in 1993). In 1996, he was awarded the Nobel Prize in Economics, along with William Vickery. The Nobel citation was ‘for their fundamental contributions to the economic theory of incentives under asymmetric information’ and in Mirrlees’s case he had ‘Developed methods of analyzing the problems of incomplete, or asymmetrical, information. Specialized in work in optimal taxation’. Whilst his main theoretical work was over, his interests in the fields of taxation and growth in developing countries continued. Indeed, in many ways his role in the realm of practical policy was about to begin. He had been involved in advising the Pakistani government in the late 1960s whilst visiting the Pakistan Institute of Development. However, in later life, he was involved with the Institute of Fiscal Studies’ (IFS) Mirrlees Commission and advising the Scottish government.

In 2001, he married again, to Patricia. Lady Patricia Mirrlees had worked in Beijing over the period 1977–1987 on the magazine *Chinese Literature* and also in Macau at the University of East Asia and so had long-standing connections with China. When James Mirrlees retired from his Cambridge Chair in 2003, he took a part-time research position as ‘distinguished professor-at-large’ in the Chinese University of Hong Kong along with other time-to-time positions at the University of Macau, the University of Melbourne, and Peking University. As a Nobel Laureate, he was invited to give many varied lectures in this period, covering all kinds of topics. These included how one should use labour subsidies to reduce unemployment in Spain, how GDP and inequality were likely to grow in China, what sort of taxes there should be, the serious problems of derivatives creating moral hazard and economic crises, economic comparisons of India and China, the policy implications of non-rational behaviour, and why the Mediterranean economies should relinquish the euro. Much but not all of the content has remained unpublished.

In 2006, when asked by Lawrence Lau, then vice-chancellor of the Chinese University of Hong Kong, to become Master of a new institution, Morningside College, in the University, Mirrlees agreed (to his own surprise), because he was a strong believer in the value of small colleges in the

Oxford/Cambridge style. Morningside took its first students in 2010, and he became a full-time Professor in the University. The College acquired its building in 2011, with a Master's Lodge, in which he and his wife currently live. The post is set to continue until the end of 2017. He intends to return to live in England when he eventually retires from Hong Kong.

2 Economics and the Economist

Mirrlees has had a long, varied, and distinguished academic career. His fields of interest have remained quite stable over time: growth, development, and taxation. Whilst he was awarded the Nobel Prize primarily for his work on optimal income taxation, this was closely related to his earlier work on optimal growth. As an economist, he is very much a product of Cambridge, and his convictions about economics were established during his early years in Cambridge. His views about the role of the economist and economic theory written in the 1960s and in more recent years are really quite similar and show a vision and integrity which is rare amongst academic economists.

Mirrlees has a strong belief in the need for economic theory to be rigorous and grounded in sound mathematics.² His mathematical approach to economics had as its starting point the work of Frank Ramsey, which developed into his work on optimal growth and taxation. In the late 1960s and 1970s, he was working at the frontier of mathematical economics and was able not only to develop models but also to solve them in a manner that opened up new paths to a whole generation of economists to explore. He sees the role of the economist very much as a technical adviser who provides practical solutions to the design of optimal policy. He is not a believer in the free market: for him the unfettered market will not yield the socially optimal outcome. For example, he has long seen income distribution and the need for equality as central to social welfare. Taxation is needed not only for redistribution but to fund the welfare state and the various benefits and goods it provides. The problem of optimal taxation is how to balance these aims with the distortions caused by taxes that tend to reduce welfare.

As befits the first Edgeworth Professor at Oxford, Mirrlees's view of social welfare has always been largely utilitarian. Although it has become fashionable more recently to adopt a utilitarian or 'happiness' framework, for most of his career the orthodoxy was that utility should be only ordinal and that car-

²For a recent statement of this view, see his lecture 'Mathematics and Real Economics' given at The Inaugural Conference at King's, Institute for New Economic Thinking, in 2010. Available at: <https://www.youtube.com/watch?v=-39znKX8kC8>.

dinal utility in the utilitarian sense³ was neither needed nor useful. However, Mirrlees stood out against the orthodoxy and has long been an advocate of using aggregate consumption (suitably adjusted for issues such as inequality) as a reliable indicator of welfare. In a 1977 lecture entitled ‘The Economic Uses of Utilitarianism’, he stated: ‘Some economists, when evaluating alternative economic policies, are utilitarians. At any rate they look at something they call the total utility of the outcome. This paper is intended to argue in favour of this procedure’.⁴

Mirrlees’s conception of utility was similar to Edgeworth’s and his contemporary and colleague Amartya Sen’s: it was a real or ‘factual’ thing rather than a subjective feeling, a person’s conception of their own well-being. The issue of measurability was one that Mirrlees believed could be overcome as was the issue of interpersonal comparisons. This was reflected in his notion of ‘isomorphism’: it is sometimes possible to treat individuals who are the same with respect to some way of comparing their experiences as ‘isomorphic’. In this case, the outcomes of economic (or social) policies ought to be evaluated by adding their individual utilities, ‘because everyone ought to agree to have every other individual treated as one of his alternative selves’ (ibid.: 83). For example, in the optimal income tax problem, you treat individuals as isomorphic with respect to their after-tax income and leisure. That Mirrlees’s view remains utilitarian is reflected in his more recent discussion of the Human Development Index. He argues that per capita consumption and not income should be used (since utility depends on consumption) and that lifetime utility would be better than simple life expectancy.⁵

3 Growth

Mirrlees himself plays down his role in the 1962 paper with Kaldor, ‘A New Model of Economic Growth’: ‘In the end he [Kaldor] generously made me a co-author’ (Nobelprize.org 1996). However, it stands as a milestone in his career: it became a much-cited paper which was part of standard courses on economic growth for two decades after it was written. It also put the name of ‘Mirrlees’ into the international limelight for the first time.

There are several economic ideas in the paper. These reflect the interests of Kaldor which were very much part of the Cambridge environment of the

³ As opposed to the Von Neuman–Morgenstern sense of a useful summary statistic combining preferences over lotteries.

⁴ Reprinted as ‘The Economic Uses of Utilitarianism’ in Mirrlees (2006).

⁵ See Mirrlees’s contribution to ‘What is Development?’ at the Institute for New Economic Thinking’s ‘Changing of the Guard?’ conference in Hong Kong in 2013. Available at: <https://www.youtube.com/watch?v=LNK65LjMcD0>.

time. What interests us in this chapter is the way that Mirrlees uses his mathematics to bring these ideas to life. He developed a dynamical system of differential equations. The use of continuous time was a natural continuation of his interest in the Ramsey model. The Kaldor–Mirrlees model consists of some of the then standard Cambridge assumptions: Investment comes from savings which are a constant proportion of profits, and there is a vintage model of investment which drives productivity. Mirrlees takes these and develops a mathematical system of equations: he then jumps around differentiating integrals, integrating differential equations searching for a solution to the system of equations in ‘steady growth’. Existence and uniqueness are established in an appendix. The model also includes the ‘Golden Age’ assumption that expectations are fulfilled. For me, one of the interesting features of the paper is Section 15: Mirrlees ran numerical simulations of the model which were partly calibrated. Numerical simulation is very easy now due to software. In 1962, this would have been a much more laborious matter! It is in fact hard to believe that this model was written in 1962. The methods used by Mirrlees would at that time have been largely unknown to all but a few economists, mostly in the USA. The article itself had a distinguished life: probably due to the advanced nature of the mathematics, it was more cited than read. However, the citations died out after the neoclassical growth model became the orthodoxy in the 1970s.

The interest in development and growth continued. The issue of optimal growth in a Ramsey model with technology followed. It is illuminating to look at the opening paragraph of Mirrlees (1967: 95):

Ramsey’s famous paper created the subject of optimum economic growth. The chief deficiency in the subject as he left it, despite some illuminating comments, was the unrealism of his assumptions. I do not intend to make the shallow and pointless complaint that his assumptions are not exactly right. The point is that we have not, up to the present, known how to calculate optimum policies for models that might be a useful approximation to economies we know. It must now be assumed that technological change is an important influence on growth: models that neglect it are useful for developing analytic tools, but give no guidance for applications.

One can see here that Mirrlees views technological change as important for growth (and development, an issue to which we will come). He wanted to extend the Ramsey model to include this feature and that is exactly what he did. However, it also displays his belief that there is a need to analyse models that ‘approximate’ real economies.

Again, for me the most interesting thing about the paper is its great care and attention to detail. The paper was at the forefront of mathematical economics. Following Ramsey, economists tend to look at infinite-horizon models. For obvious reasons, the mathematics developed for physics and engineering only looks at finite-horizon models. Optimization in a finite horizon has both an initial condition and a terminal condition. These are usually enough to pick out a particular path as the optimal one. However, in infinite-horizon models the terminal condition is absent. Economics of course went on to develop the appropriate transversality condition: the condition that the value of the shadow price vector tends to zero as time tends to infinity. However, in the mid-1960s when the paper was written all this had yet to be sorted out. Mirrlees ends up with a saddlepath in his Figure 1, although he does not use the term ‘saddlepath’. He rules out the ‘explosive’ paths on economic grounds:

Paths reaching the z -axis cannot represent the optimum development, for x becomes negative, and they therefore do not even represent feasible developments of the economy ... Similarly if one...followed a path lying below RB [i.e. the saddlepath], there would be a lower rate of consumption at every subsequent date, which is obviously inferior. These paths we have rejected accumulate capital unnecessarily: with their associated prices, they fulfil all the competitive conditions, but they are not efficient (ibid.: 99–100).

The mathematical analysis looks at special cases (the production function is Cobb–Douglas or CES) and also looks at the solution when there are increasing returns and non-constant growth rates of the population and/or technology. The analysis is both innovative and exhaustive.

What did the young Mirrlees see as important in his work? Again, we can let him speak for himself:

Is it worth while to calculate optimum policies, and discover the features of optimum growth paths? We can, I think, make three kinds of claim for the usefulness of such a model as is discussed in this paper. First, it may be claimed that it helps us to judge the performance of actual economies; second, that it provides a method of estimating the optimum rate of investment for a government that determines the aggregate distribution of output between consumption and investment by fiscal and monetary means; third, that it provides a means of estimating accounting prices for the use of an economic administration that takes detailed economic decisions, and for its guidance in establishing a system of taxation (ibid.: 123).

The world view Mirrlees shared at the time was one of economic planning ('administration' and 'decisions') and a guide to taxation—the subject of which he was soon to move on to.

4 Triumph: Optimal Taxation

Mirrlees had been developing his understanding of economics and the mathematical modelling of it in the 1960s. What happened soon after his arrival at Oxford was that the problems he examined were at the epicentre of economic theory. He started to look in detail at the problem of income and commodity taxation leading to his development of the mathematical framework which was to have a much wider application across a significant range of economic models. The way Mirrlees formulated the issue was that optimal taxation was a problem in what would later be called asymmetric information: the tax had to be designed in a world where the underlying abilities (potential earnings) of individuals could not be observed. This meant that not only was there the standard trade-off between efficiency and (non-lump-sum) taxes but also an informational problem (how to tax people when you could not observe ability). The triumph was not simply to understand this problem, but to solve it. Depending on the social welfare function (the maximand), the aim of the tax regime is partly redistributive: you want to tax higher-ability households and transfer money to lower-ability households. However, you do not know who the higher-ability households are; all you can observe is their income and infer ability.

The key to the 'Mirrleesian' approach is the 'sorting' or 'single-crossing' condition, which says that the optimum consumption of households is a non-decreasing function of their ability (it is stated as condition B on page 182 of Mirrlees 1971). In his own words: 'It is equivalent to assuming that (in the absence of taxation) the consumer's demand for goods is an increasing function of the real wage rate (at any given non-wage income)'. This of course is very closely related to the *Revelation principle*. Mirrlees did not think of the model as an information revelation problem, but an optimal taxation problem can be thought of equivalently as one where the household faces a tax based on its ability and has to report its ability. The design of the optimal tax will then involve the household reporting truthfully its ability. In the fields of mechanism design and imperfect information, this became a key point. It is often referred to as the Mirrlees–Spence single-crossing condition; however, in Spence's work there are only finitely many types (two), whereas Mirrlees covered the case of ability being a 'continuous' variable. It was working out how to deal with this problem

that proved more useful than the application of optimal income taxation within which Mirrlees developed it. As he wrote a quarter of a century later:

In the income-tax problem, relatively simple conditions, easily checked for the particular model I was using, implied that the solution of the equations did give an optimum: the conditions were sufficient as well as necessary. When the computations were done, one knew one had the right answer, not just an answer that might be right (Mirrlees 1997: 1,317).

It is interesting to see where the solution came from. Mirrlees had been dealing for some years with the Ramsey growth model: this involves choosing consumption as a function of time. In the optimal income tax problem, you have to choose consumption as a function of income. It was the genius of Mirrlees to make the link that the same Pontryagin maximum principle applied to both, although in the 1971 paper he prefers to use the calculus of variations in his derivations—as had Ramsey—because it was more intuitive and familiar. The dynamic optimization framework Mirrlees had been working with on the optimal growth problem could thus be adapted and applied to the problem of income taxation. No one had ever done this before, and it provided the tools for use in a whole range of similar problems in different fields of economics over subsequent decades—in particular contract theory, the principal–agent problem and what has become known as Mechanism Design.

The work on taxation also extended to look at another of Ramsey's interests: optimal indirect taxation. Ramsey had derived the famous result that the tax rate on a commodity should be equal to the inverse of the elasticity: it is better to tax goods that have inelastic demands since this will lead to less welfare loss. With Peter Diamond, Mirrlees considered an extension of Ramsey's result. What happens if you have production? How should indirect taxes then be chosen? The result was the *Diamond–Mirrlees Efficiency Theorem* (Diamond and Mirrlees 1971a, b). In essence, the optimal indirect tax must involve productive efficiency (in the standard Pareto sense). Optimal indirect taxes cannot involve any distortion of the prices faced by producers (assuming no market imperfections such as externalities). Hence, indirect taxes should simply be on final consumption goods. Producer and intermediate goods prices should not be subject to taxation.

What does the optimal income tax schedule look like? This depends on the assumptions made about the distribution of abilities and the preferences of households. The original Mirrlees paper considered some explicit examples, and many more researchers went on to explore special cases. One interesting result that seems to be fairly common is that the marginal tax rate might be declining in income towards the top end, perhaps with a zero rate for the highest income. At

the bottom income levels, the marginal tax rate could be low and even negative. To understand this, the special case of a degenerate distribution of types where everyone is the same can be used. Here, the incentive compatibility constraint falls away, and you can tax everyone the same. Efficiency will then dictate a universal lump-sum tax with zero marginal rate. If there is a little inequality, you want to introduce a small marginal tax but keep the average tax high enough to cover the required tax revenue. For the lowest-ability workers, the lump-sum element will result in low or possibly negative consumption that may need to be compensated with a lower, zero, or possibly negative income tax. This in turn means that the marginal tax rate will be increasing as you move to the middle-income earners. The zero top rate comes about because the real wage and hence marginal productivity of the top earner is the highest; hence, the disincentive effect of income taxation is the highest. The inverted U shape is quite common: the marginal tax is highest for middle (modal)-income earners. However, it is also possible to have the marginal rate declining everywhere, or indeed and approximately a 'flat tax'.

5 Development

Prior to his move to Oxford, following the suggestion of Amartya Sen, in the late 1960s Mirrlees joined the India Project run by Paul Rosenstein-Rodan for the MIT Center for International Studies, spending time at MIT (during which he met Bob Solow and Paul Samuelson) and then in India. He applied his attention to the issue of development. The end result was the work with Ian Little and the 'Little-Mirrlees' method of cost-benefit analysis for developing countries. This was nothing less than a general framework applying appropriate shadow prices to public sector activities to enable efficient allocation of resources by the public sector. Distributional issues were included in choosing the shadow price of labour, as were distortions such as currency controls. Prior to Little-Mirrlees, the main approach had been to employ market prices to undertake cost-benefit analysis in developing countries: whilst there are limitations to using market prices to measure opportunity cost and marginal benefits in developed countries due to distributional and market failures, in developing countries these limitations become huge and market prices can be highly misleading (see Mirrlees 1969a; Little and Mirrlees 1969, 1972, 1974). Again, this is an instance of Mirrlees wanting to do things properly rather than taking the easy option. Also, we see the expert economic adviser looking beyond and behind the market to design policy that can increase welfare and promote economic efficiency. Ian Little also shared Mirrlees's utilitarian perspective (see Little 2002).

Mirrlees's interest in development has remained, more recently focusing on the issue of taxation (see Mirrlees 2011a). Mirrlees is very much aware of the problem of collecting taxation in developing countries: he believes that governments should use 'all the kinds of taxes that really work in lower income countries' (ibid.). Here, of course, his experiences in India were highly instructive to him: a sales tax is the most practical to raise, but was in effect an 'urban' tax with little chance of being levied in rural areas. In order to make a sales tax progressive you need to add subsidies. An income tax is harder to make work: he is of the opinion that when he was in India, it was 'totally ineffective', although nowadays that is not true in India and China:

With the urban sector growing rapidly in many countries, only a few people paying tax should be quite a temporary phenomenon. For example, a lot of people now pay tax in People's Republic of China. There will always be many people in rural areas that you should be able to get into the tax system. But it actually becomes very inefficient including large numbers of low income tax payers as you end up only collecting a small amount of tax and it usually costs almost as much to collect the tax. So I come back to thinking that a sales tax is a pretty good way of doing it (ibid.).

6 The Economist as Adviser

The Mirrlees Commission, which reported in 2010, aimed to update the earlier IFS Commission on Taxation chaired by Cambridge economist James Meade three decades earlier in 1978. Not only was Mirrlees chairing it but some of the distinguished members were also his erstwhile PhD students, including Timothy Besley and Gareth Myles. The title 'Tax by Design' certainly reflected the Mirrleesian world view that we have met in his writings on optimal tax and growth:

Tax by Design is both an imperative and a description of our approach in this review. Our aim is to set out the principles on which a 21st century tax system should be based and then to apply them in suggesting concrete policy recommendations to improve the UK tax system. To that end, we use insights from economic theory and empirical research to discuss the impact that the tax system has on people's behaviour, and the resulting trade-offs that policymakers have to make between the various and often conflicting objectives that they might wish the tax system to achieve (Mirrlees et al. 2011a: 1).

Mirrlees himself spoke as chairman of the committee:

The review shows that the UK system falls short of the ideal in costly and inequitable ways. It discourages saving and investment, and distorts the form they take. It favours corporate debt over equity finance. It fails to deal effectively with either greenhouse gas emissions or road congestion. The revenue it raises, and the redistribution it does, could be achieved in less costly ways. We propose both a long-term vision of a better system, and directions for reform. Some of the recommended reforms involve tweaks to current policy; others involve radical change, and are probably for the longer term. It is undeniable that some of the proposed changes would be politically difficult. But failure to reform imposes enduring costs (Mirrlees quoted in IFS 2010: 1).

The report is over 500 pages long and includes a great deal of detail. However, the three main recommendations were:

1. The tax system should be designed as a whole, being both green and progressive.
2. The tax system should seek neutrality. Tax systems should not distort people's behaviour by treating similar activities differently without very good reason.
3. The tax system should achieve progressivity as efficiently as possible. That means relying on the rate schedule of personal taxes and benefits—rather than inefficiently distorting the tax base—to achieve redistribution.

We can see very much the Mirrleesian theme of the need to redistribute income resulting in progressive taxation. However, the tax system needs to be efficient and take into account the distortionary effects it creates. Also, it is necessary to look at the benefits system as a whole. Mirrlees himself is typically very modest about his role in the commission bearing his name: 'I wish I had been in the UK during that period: it was difficult to get properly involved so far away in Hong Kong; but I liked what we produced' (private correspondence).

In 2007, the devolved Scottish government set up the Council of Economic Advisers (CEA) in which James Mirrlees was and still is a member. He was also a member of the Fiscal Commission Working Group (FCWG), alongside Joseph Stiglitz and others advising on fiscal issues that would face an independent Scotland. Although he has always kept his own personal views on Scottish independence private, he made a decisive public intervention in the 2014 referendum (*The Telegraph*, 24 August 2014). He argued that in post-independence negotiations, the Scottish government should not agree to take on its share of the British national debt unless currency union and sharing of the pound was agreed. His reasoning was that '[i]t is hard to see how Scotland

can take on the debt unless there is a full currency union'. Earlier in 2014, he had also suggested that an independent Scotland would have to look at the 'alternative of a Scottish pound' separate to sterling, with Scotland launching its own central bank and borrowing at a higher rate to protect the new Scottish pound. However, he believed that the currency would have to be pegged to the pound to prevent inflation rising sharply: 'It would probably require some borrowing initially, at a rate a little higher than UK's current borrowing rate, but initially lower than the rate of inflation, so not very expensive; and it has the advantage of somewhat greater policy flexibility in the longer run'.⁶

On the issue of currency union with England, Mirrlees argued against the Governor of the Bank of England, Mark Carney. He stated that 'My views are based on economic principles and observation of monetary unions elsewhere. They are about what is technically possible and identifying a clearly workable and preferable option'. The advantages of a currency union were

reducing transaction costs and exchange-rate risk, encouraging trade, competition and economic efficiency. Surely it is better for the English to be able to use their money in Scotland without paying to change it? That is not a trivial benefit. Nor is the absence of exchange risk, which otherwise has to be added to investment risk.

He also argued that the Eurozone experience did not provide a good example for an Anglo-Scottish union. Whereas the Eurozone membership was diverse, 'In contrast Scotland and the rest of the UK are very similar economies. Though there are some risks of asymmetric shocks, institutions can be built to adjust to them; and the costs are small relative to the advantages mentioned'.⁷

Earlier, in 2013, Mirrlees presented evidence on the Scottish government's *Revenue Scotland and Tax Powers Bill*. In his discussion,

Efficiency is a concept that, for economists at least, means something more precise and restrictive than it did for Adam Smith. As a (joint-)author of an 'efficiency theorem' in tax theory, I am strongly attached to this property, but it is a property of the whole production of the economy, as influenced by the tax system as a whole. A main requirement is that there should not be taxes on transactions between producers, except to counter externalities such as atmospheric pollution and congestion (Mirrlees 2013: 2).

⁶ *The Scotsman*, 15 February 2014.

⁷ *The Scotsman*, 13 February 2015.

7 Conclusion

In this brief chapter, I have focused on what are for me the main themes of Jim Mirrlees's career as an economist. This can be summarized as the application of mathematics to the design of economic policy (taxation in particular), for the betterment of society in the utilitarian tradition. In his early career, he was working at the forefront of mathematical economics, at first developing the work of Frank Ramsey. However, his moment of genius was to adapt the same mathematical techniques to the issue of optimal taxation and his formulation of the single-crossing condition. His framework was to provide the springboard for decades of research by many economists, in many fields but particularly in public economics. It was for this that he was awarded the Nobel Prize.

However, there are several avenues of his research I have not been able to cover. Within his list of publications are some real treasures. My personal favourite is his 'The Optimum Town', written in 1972 (Mirrlees 1972a–c): starting from first principles, he sets up the location problem and works through the conditions to derive an elegant set of results. Another is his 1976 article, 'The Optimal Structure of Incentives and Authority Within an Organization'. Articles such as these embody the same elegance and scope as his work on taxation.

Mirrlees always viewed his work as trying to solve a problem that was of practical relevance to the economy and society. From his utilitarian perspective, he was always concerned with issues of equity, although this had to be achieved in a manner consistent with efficiency. His ideas have affected policy design for decades. In his later career, he was also able to have a more direct policy role, an economist as adviser. He was also a prolific supervisor of PhD students and has a real commitment to the highest values of higher education, as reflected in his role in the setting up of Morningside College.

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Robert E. Rowthorn (1939–)

Paul Ormerod

1 Introduction

Robert (Bob) Rowthorn's writings embody the fine tradition of Cambridge radicalism. His interests have ranged widely, from the problems of Britain's regions to more general questions of economic development, to key issues relating to the distribution of income between capital and labour, and to important social questions such as marriage, family structure, and immigration. In short, throughout his career, Rowthorn has worked as a political economist, addressing issues of importance to policy, rather than simply displaying mathematical prowess in abstruse areas of economic theory.

Rowthorn could so easily have followed a conventional academic career, his CV filled with technical papers in journals such as *Econometrica* or the *Review of Economic Studies*, unconcerned about major political issues. He grew up in South Wales, but his background was very much that of the conservative middle class, his father being a senior officer in the police force. He won a scholarship to Jesus College, Oxford, where he read not Philosophy, Politics, and Economics (PPE), but mathematics. Rowthorn was an outstanding student, winning the prize for the best results of his year in the final examinations. A research career

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beckoned, and his graduate work began in mathematics, before he switched in 1962 to take the BPhil in economics, then, as now, a two-year course.

In the 1960s, when Rowthorn was receiving his initiation into economics, Cambridge scholars such as Nicholas Kaldor and Joan Robinson were major world figures within academic economics. Indeed, it was to Cambridge, and its challenges to orthodox thinking, to which Rowthorn was attracted, becoming a Research Fellow at Churchill College in 1964. Rowthorn's politics were increasingly left wing; in fact, for many years he was a member of the Communist Party. The British Communist Party, it must be stressed, by the 1960s had become very distinctly non-Stalinist, and over the next couple of decades, many interesting contributions to political economy were made by its members. Rowthorn himself has never been willing to subscribe to a dominant orthodoxy of whatever kind. Indeed, throughout his entire career, his work has challenged received opinions on a wide range of topics. In the atmosphere of the Cambridge of the 1960s, he flourished. His intellectual qualities were obvious, and by the early 1970s, he had established himself as a Fellow of King's, a University Lecturer, and a member of the Faculty Board.

To modern-day academics, continually harassed by the demands of the Research Excellence Framework, the fact that Rowthorn published no papers until 1970 may seem incredible. But Cambridge, and King's College in particular, has over the years proved the ideal environment for a scholar of his abilities. Freed from the pressure of producing a constant stream of articles in so-called top journals, Rowthorn has been able to apply himself to whatever topic he considered important.

In return, he has been a true servant of both the College and the Faculty. Within King's, he was a dedicated teacher of both economics and econometrics for decades. He has served on the College Council, the Estates Committee, the Investment Committee, and the Senior Salaries Committee. Within the Faculty, he rose to become a Professor in 1991, serving on many committees and becoming Head of Department in 2002.

Rowthorn developed close international academic links, having visiting appointments at many universities, especially in Australia, Italy, and Japan. In the late 1990s, he had several spells at the International Monetary Fund (IMF). At a stage in his career when many academics are effectively resting on their past achievements, during the decade of the 2000s, when well into his 60s, Rowthorn paid many visits to the Santa Fe Institute. Santa Fe is renowned of course for the innovative, multidisciplinary nature of its work.

His work has not been confined to the world of academe. In addition to the IMF affiliation already mentioned, Rowthorn has been a frequent consultant to both the United Nations Conference on Trade and

Development (UNCTAD) on global integration and structural change and the International Labour Organisation on employment and wages. These topics have been central to his academic writings, and as a political economist, Rowthorn has combined both theory and practice with these links. In keeping, too, with his long-standing interest in regional policy, he has consulted with the European Commission and various UK ministries over the years. His interest in policy remains just as strong as it has ever been. In 2014, for example, he and I collaborated on a paper for the Mayor of London considering long-term scenarios for the UK economy in and out of the European Union.

Rowthorn has never been afraid to court controversy. Perhaps the most notorious episode, which almost ended his career, was the famous Garden House riot in 1970. Greece at the time was in the final throes of a military government. A major promotional event for the regime was due to be held at the Garden House, then, as now, one of the leading hotels in Cambridge. As a charismatic young Assistant Lecturer, Rowthorn had great status amongst the enraged student left. A demonstration took place which rather got out of hand. Substantial damage was caused to property, and several students were convicted and sent to jail. After what were perhaps a few anxious moments, Rowthorn survived the episode unscathed.

During the past 15 years or so, again at a stage in his career when many academics have laid their laptops to rest, Rowthorn has provoked the rage of the metropolitan liberal elite with a series of powerful articles in political journals such as *Prospect*. In this journal, he wrote a seminal attack on the concept of multiculturalism, one which gave other prominent centre-left intellectuals the opportunity to express their own long-harboured doubts. He ran a study group and wrote extensively on family structures, pointing out the damage caused, particularly to the poor, by the fashionable idea that all such structures are of equal merit in terms of outcomes. More recently, he has intervened in the debate on the impacts of immigration.

2 The Distribution of Income Between Labour and Capital: Inflation and Growth

One of the key themes of the Cambridge School was the emphasis which it placed on the distribution of income between labour and capital, and its importance for the macro economy. This was the topic of one of Rowthorn's earlier and most influential papers, 'Conflict, Inflation and Money', published in the *Cambridge Journal of Economics* in 1977. He expanded the article

into a book, *Capitalism, Conflict and Inflation*, which was awarded the Isaac Deutscher Memorial Prize in 1981. The Cambridge view of the role of income distribution in macroeconomics is in sharp contrast to that of mainstream, neoclassical economics. General equilibrium, for example, is consistent with *any* distribution of income.

The Cambridge view was inspired by the works of David Ricardo. Although his work was almost entirely theoretical, Ricardo wrote as a political economist, seeking to understand the key issues of his day. A crucial one, of course, was the entirely new phenomenon of industrial capitalism. This was clearly something completely different to anything which had ever existed before. But in the early nineteenth century, when Ricardo was writing, it was not at all clear that the system was sustainable. It might disappear just as quickly as it had emerged. Ricardo placed great emphasis on the distribution of income between capital and labour. An appropriate balance between the two had to be struck in order for long-run economic growth to continue.

Marx also placed great emphasis on the role of factor shares in the evolution of capitalism. It was only later in the nineteenth century, following the work of Jevons and Walras, that economics lost this focus. Economic theory became concerned instead with the problem of refining and making more precise the conditions under which prices which cleared all markets could be found. In many ways, it was a very strange problem for economics to focus on. The theory refers to the most efficient allocation of resources in a purely static world. Given a fixed amount of resources of all kinds, including labour, could a set of prices be found which would clear all markets? It was strange because by the late nineteenth century, the Industrial Revolution was a century old. For the first time in human history, a social and economic system had emerged in which the total amount of resources available was being continually expanded. There may be, indeed there were, short-term fluctuations in total output, but the underlying trend in total output was unequivocally upwards.

The luminaries of the Cambridge School, such as Kaldor, Robinson, and Sraffa, tried to revive the view that income distribution plays a central role in capitalism. This is the background to Rowthorn's 1977 paper. A key policy question of the time was inflation. During the 1950s and 1960s, inflation had been positive but low throughout the West, at generally similar rates across the main economies. During the 1970s, for a variety of reasons, not least the quadrupling of the oil price in 1973–1974, inflation surged into double figures in many countries. Some, such as Germany, succeeded in bringing price rises back under control. In others, such as Italy and the UK, it rose to an annual rate of more than 20%. The various attempts to squeeze inflation

down to permanently lower levels were the central theme of economic policy in the West for at least the next two decades.

Rowthorn addressed this crucial issue of political economy from a Cambridge perspective. In his model, labour and capital aspire to particular shares of national income. Workers try and meet their target by securing increases in the nominal wage, and capitalists in turn use price increases in an attempt to secure theirs. If the two shares are compatible and sum to total income, inflation remains constant. But if the sum of the desired shares exceeds national income, the result is accelerating inflation. One problem which capitalists face is that there is resistance on the part of the workers to reductions in the real wage. This is overcome by the use of monetary policy, which affects not inflation directly, but output and hence unemployment. The Marxist concept of the 'reserve army of labour' is invoked, with unemployment being used deliberately as a means of controlling inflation.

This brilliant article explained both the contemporary experience in the West in the 1970s and more general aspects of the inflationary process in the capitalist economies. The paper has received almost 500 citations, but it surely has the potential to become what I might term a 'sleeping giant'. Two such papers spring immediately to mind. Armen Alchian's 1950 paper (Alchian 1950) on evolution and economic theory was decades ahead of its time, anticipating many of the developments in the mathematical theory of evolution in the 1990s and beyond, and dealing ruthlessly with the argument that it is 'as if' firms adopt profit-maximising strategies because only the fittest, following such a rule, can survive. Herbert Simon's 1955 paper (Simon 1955) on behavioural economics is the other, in which he introduces the concept of satisficing, a term which has subsequently been transformed in meaning by mainstream economists and safely neutered. For many years, both papers suffered from relative neglect, but they now have around 5,000 and 11,000 citations, respectively. The theoretical analysis of inflation set out by Rowthorn in his model is, to my mind, of similar intellectual stature.

The oil price shock of 1973–1974 had an obvious and immediate impact on inflation in the developed economies by increasing the cost of inputs. But it was the distributional consequences which explained the subsequent huge divergence in inflation rates between countries. In the short term, until the OPEC countries worked out how to spend their new-found surpluses, the oil price rise was an effective tax on the West. It transferred real national income from the oil-consuming to the oil-producing economies. The willingness of labour to both recognise and respond to this was a crucial determinant of inflation in 1975 and 1976. Rowthorn's concept of real wage resistance was the key factor. In Germany, workers were willing to accept real wage cuts,

and by 1976 inflation was only 6%. In contrast, in the UK it was still around 15%. The attitudes of the British labour force were undoubtedly militant, exemplified by the miners' dispute in early 1974 which led to the defeat of the Conservative government in an election called explicitly on the question 'Who Governs Britain?' But the institutional structure of wage setting itself was a major barrier to real wage reductions. In late 1973, the government had agreed a system under which wage increases were brought in almost instantaneously after a particular rate of inflation had been reached. Real wage resistance was effectively institutionalised.

Inflation did moderate from its 1975 peak, in part due to the effects of higher unemployment and in part due to the income policies negotiated with the trade unions by the then Labour government. But even by 1981 it was still in double figures. The Thatcher government, elected in 1979, used tight monetary policies to try to control inflation. But the immediate effect was to generate a sharp recession, comparable in size to that of the 2008–2009 financial crisis drop in output. The unemployment rate, already very high by post-war standards, doubled. Exactly as Rowthorn's model predicted, monetary policy was used to discipline labour and control inflation, which fell sharply.

The breakdown of the Phillips curve meant that economics needed a new theory of inflation. Rather it needed a theory, because the curve was simply an empirically observed relationship, with no formal theoretical basis other than a sort of handwaving around supply and demand. Milton Friedman and Edmund Phelps rose to the challenge and developed the so-called natural rate of unemployment in the 1960s. As Friedman stated in his famous Presidential Address to the American Economic Association in 1968:

[T]he [natural rate] is the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is embedded in them the actual structural characteristics of the labour and commodity markets, including market imperfections, stochastic variability in demands and supplies, the costs of gathering information about job vacancies, and labor availabilities, the costs of mobility, and so on (Friedman 1968: 8).

The natural rate fairly quickly morphed into the non-accelerating inflation rate of unemployment (NAIRU). The NAIRU is not necessarily time-invariant and can change over time, as the various factors set out by Friedman above evolve. But we might reasonably think that such changes would generally proceed only slowly. More generally, at any point in time, it is implied that there is a unique rate of unemployment consistent with constant inflation. Rowthorn's model has many similarities with this mainstream approach.

A key difference, however, is that for any given set of institutional structures and any imperfections which they might introduce into the workings of the labour market, non-accelerating inflation is compatible with a wide range of unemployment rates. The key determinant is the consistency of the demands for factor shares by labour and capital.

This prediction of the model appears to be supported by the evidence. Over the 1871–2014 period, for example, the statistical technique of fuzzy clustering identifies three distinct regimes¹ in inflation/unemployment space for each of the three main Western economies over this period, the USA, Germany, and the UK.² The most frequently observed is the cluster with both low unemployment and low inflation. High growth, and consequently low unemployment, may make it easier for the demands of labour to be met and is therefore consistent with low unemployment. This cluster, for example, characterises the post-war period until the early 1970s. The second cluster contains years with a similar level of inflation to the first, but with an average unemployment rate which is more than doubled. Typical observations in this cluster are from the early 1980s to the late 1990s, when, as mentioned above, monetary policy was used as an instrument to regulate real output. The final cluster, the least frequent in occurrence, has unemployment similar to the first cluster, but very much higher average inflation, well into double figures. Characteristic years here are the First World War, a time of acute struggle between capital and labour and a time when the government feared insurrection on Soviet lines, and the enormously disruptive period embracing most of the 1970s. These are times when historical evidence makes it very clear that not only was consensus over the distribution of factor shares lacking but labour was sufficiently powerful to try to enforce its claims. The episode of the 1970s ended as described above, and the experience of almost a century ago was terminated abruptly by the recession of 1921, by far the most serious drop in output in a single year ever experienced in the UK.

Rowthorn's appreciation of the importance of the distribution of income between labour and capital has been a consistent theme in his writings. Indeed, one of his most recent papers uses his understanding to mount a devastating theoretical and empirical critique of Piketty's best-selling book *Capital in the Twenty-First Century*. Published in 2014 in the *Cambridge Journal of Economics*, a journal much favoured by Rowthorn over the years, the article

¹ With the exception of the hyperinflation in Germany from 1921 to 1924, which is a unique fourth cluster.

² See Ormerod et al. (2013).

embodies the hallmarks of his work. A complete grasp of economic theory is combined with careful and meticulous understanding and analysis of the data. It also, as an aside, illustrates Rowthorn's very generous nature. Although his criticisms are powerful, he nevertheless tries to see good in everything and describes Piketty's book as 'brilliant' (Rowthorn 2014a: 1,275).

Rowthorn's summary of the book could not be bettered:

[I]t shows how the share of income accruing to wealth-owners has increased dramatically in recent decades. It also provides a simple explanation of this development based on the standard neoclassical theory of factor shares. This theory establishes a link between the capital intensity of production and the share of profits in total output. The nature of this link depends on the elasticity of substitution between capital and labour. When this elasticity is greater than unity, an increase in the capital-output ratio leads to an increase in the share of profits. This, in essence, is Piketty's explanation for the increased share of wealth-owners in national income. Thus, the shift in income distribution is due to the over-accumulation of capital: there has been too much real investment (*ibid.*).

As Rowthorn points out very clearly, the evidence suggests just the contrary. Many studies, in an area in which Bob has been actively involved over the years, show that the elasticity of substitution between capital and labour is in fact less than one. The capital-output ratio, as conventionally measured, has either fallen or been constant in recent decades. The apparent increase in the capital-output ratio identified by Piketty is a valuation effect reflecting a disproportionate increase in the market value of certain assets, a point which Rowthorn uses Piketty's own data to demonstrate. A more plausible explanation for the increased income share of wealth owners is an unduly low rate of investment in real capital. Projecting ahead into the rest of the twenty-first century, Piketty assumes that capitalism will only deliver low rates of growth. One does not have to believe in the perfect functioning of markets to imagine that, if this were to be the case, at some point there would be a large downward revision of asset values, and that so much of the inequality generated in recent decades would disappear.

Economics is inevitably a discipline in which political values are invariably present, no matter how hard mainstream economists have tried to establish claims that it is value-free. Rowthorn certainly has his political views, but his writings are marked by their scientific candour and honesty. In the face of Piketty's book, many Cambridge-trained (or inspired) economists tend to be blinded by their political sympathies. Rowthorn approached the book as

objectively as possible and found it flawed. He applied his deep knowledge of both neoclassical and Marxist theory to arrive at his conclusion.

Rowthorn's passion for scientific truth is shown clearly in a 1975 article, a thorough empirical demolition of what was then a fashionable and influential concept known as Kaldor's Law. Kaldor was then one of the dominant figures if not the dominant figure in Cambridge economics. In addition, he had exercised substantial, direct influence over the 1964–1970 Labour government. One of its most controversial measures, the Selective Employment Tax, was almost entirely due to the content of his Inaugural Professorial Lecture in Cambridge, in which he set out his so-called law. Rowthorn was by then reasonably well established in Cambridge, although still very much junior to Kaldor in terms of status and influence. To add piquancy to the setting, both were Fellows of King's.

Kaldor claimed that a strong positive relationship existed between productivity growth and output growth in the manufacturing sector. From this, he drew the conclusion that the UK's poor manufacturing performance by international standards required labour to be shifted from the services sector to manufacturing, hence his tax on employment in the services sector. Kaldor's empirical findings were criticised, and two young members of the Department of Applied Economics, Francis Cripps, one of the anointed princes of Cambridge economics, and Roger Tarling defended him.

Rowthorn's short paper on this, published in the *Economic Journal*, another favourite Rowthorn outlet, is a model of careful applied econometric analysis. One key point, in an object lesson to many young econometricians today, is that he takes the trouble to understand the data. The econometric technique he uses is simple, but the paper is nonetheless devastating. Essentially, Rowthorn showed that, in the international data sets used in the analysis, Japan was clearly an outlier. Not just that, but this single country exercised a decisive influence on the results which were obtained by Kaldor to support his thesis. Nowadays, of course, measures based upon the hat matrix of a regression can be used to formalise this, but Rowthorn's results speak for themselves.

Entirely typically, when Rowthorn revisited this area, in a 1979 paper also in the *Economic Journal*, in his final generous remark, he notes, 'My own views on this matter have changed since I criticised Kaldor for his emphasis on dynamic economies, and I now think they are of great practical importance' (ibid.: 133, fn. 1). This article is not empirical but theoretical and offers a strong critique of Verdoorn's Law, derived from a mathematical model developed by Verdoorn in 1949 and which underpinned much of the kind of empirical analysis carried out by, for example, Kaldor. The gem of Rowthorn's

paper is its opening paragraph, which contains the marvellous sentence, '[Verdoorn's] article is frequently cited in British and American literature, yet, as far as I am aware, has never been published in English, and does not seem to have been read very carefully by those who cite it' (ibid.: 131). Again, this is entirely typical of Rowthorn. He took the trouble to read the paper in its original Italian. Rowthorn, it should be said, has a talent for languages and can read several European ones with fluency. For example, he has recently established a small study group which reads Wittgenstein. Rather than rely upon English translations, Rowthorn reads the original German edition of *Tractatus Logico-Philosophicus*, published in 1921.

3 The Dynamics of Capitalism: Unemployment and Wage Inequality

A constant theme in Rowthorn's work has been his focus, both theoretical and applied, on the major aspects of the dynamics of capitalism. He returned to this again in his 1995 paper in the *Oxford Review of Economic Policy*. Here, Rowthorn tackled two issues. First, the sharp rise in unemployment which had taken place in the OECD economies since the 1970s. Second, the increase in wage inequality, which had reversed a long-run trend towards greater equality. Rowthorn returns to the framework of his 1977 macro model discussed above. He argues that both these phenomena can be explained by a shortage of capital stock.

The prevailing orthodoxy at the time was that unemployment was primarily generated by imperfections in the labour market. For example, the quality and skill of the workforce and the level of benefits relative to wages. The work of Richard Layard and Steve Nickell was particularly influential (Layard and Nickell 1986). Rowthorn takes the Layard–Nickell theoretical model and shows that their result that the equilibrium rate of unemployment is independent of the capital stock depends upon the assumption that the production function is Cobb–Douglas, specifically, that the elasticity of substitution between capital and labour is one. Rowthorn points out that, as he did nearly two decades later with Piketty, the empirical evidence is that it is less than one. With this assumption, he shows that in the Layard–Nickell model itself, unemployment depends upon the capital stock.

In Rowthorn's own model, the capital stock is a determinant of both unemployment and wage inequality. An inadequate level of capital stock, whether brought about by premature scrapping or investment being too low, leads

to unemployment in economies where the level of flexibility in the labour market is low. Unemployed workers find it difficult to reduce their wages so as to price themselves back into work. Further, the level of benefits in such economies will typically be reasonably high relative to the wage, reducing their incentive to return to work. With flexible labour markets, of course, workers are more willing to reduce wages, and the effect is to create a situation with lower unemployment and higher wage inequality. Again, Rowthorn looks for empirical evidence with which to confront his theory. He finds positive evidence across the OECD in the case of manufacturing but not the services sector.

The 1995 paper is a useful focus for a number of additional themes. In terms of economics, Rowthorn himself rationalises the lack of evidence on the influence of the capital stock on unemployment in the services sector as follows: ‘[It is] a sector where information technology is so important. [The result] may also be due to the fact that substitution between labour and capital is on average easier in services than manufacturing, and that many services use very little capital at all’ (Rowthorn 1995: 34). The insights on the importance of IT and the low level of capital per worker in the services sector are even more important today, two decades after the publication of the article. The services sector is not only by far the dominant sector in the Western economies but a rapidly rising share of services is delivered through the technology of the Internet. Concepts such as the output gap remain of great significance in policy circles. But these developments render it virtually useless. The marginal cost of delivering many services via the web is close to zero, and in such situations output can be expanded virtually without limit. More generally within services, measured output is itself positively related to the level of demand. The price which a restaurant or a consultant, say, can charge at different points in time for an identical service depends upon the state of demand. The higher the demand the higher the price of output, for the same level of inputs and physical embodiment of the output.

The paper also, rather tantalisingly, develops briefly three other themes. The first of these is the importance of the long-term real rate of interest. This was stressed by Keynes but has been almost entirely ignored by many so-called Keynesians in the ongoing debates over austerity. Rowthorn also regards high real rates of interest as a deterrent to capital accumulation and hence creating higher unemployment. He notes that, by the mid-1990s, many OECD economies had run structural fiscal deficits for some considerable time. These, combined with both a relatively low level of personal savings and rising demand for investment funds in developing countries, lead, in this classical model, to high real interest rates. The situation is rather different at present, but the experience of the Mediterranean economies shows how easy it is to

generate high rates on government bonds if the markets lose confidence in the fiscal probity of a government. Rowthorn made the point at least as early as his 1995 paper that structural government deficits were not necessarily a good thing. High long-term interest rates will either deter borrowing to invest by depressing animal spirits or encourage companies holding cash to lend it to the government, or return it to shareholders. Either approach reduces the use of available funds for investment. Further, there is a reduction in net wealth of the private sector following the revaluation of the stock of government debt which it holds. All this shows how government debt can reduce economic activity.

The second point, which is a consistent theme of Rowthorn's work, is the importance of institutions. Economies do not operate in abstract settings in which the Walrasian equations are ground out. The particular institutional structure is often of decisive importance to the outcome, or potential range of outcomes. Mainstream economics, to be fair, implicitly assumes a set of institutions such as the rule of law and the enforceability of contracts. But Rowthorn goes much further than this. So in his 1995 paper, for example, the impact of a shortage of capital stock will be seen as a mix of higher unemployment and increased wage inequality, the relative weights of the two depending upon the extent to which any given economy is what Rowthorn describes as 'regulated' (ibid.: 31).

The third theme is the role of profit share in the determination of capital investment. This serves as a potential link to the growth cycle model of Richard Goodwin, also a Cambridge economist, based upon a Lotka–Volterra system of non-linear differential equations, in which the dynamics cast the workers in the role not of prey but of predator on the profit share of the capitalists. Meghnad Desai and I wrote a short appreciation of Goodwin's work in 1998 in the *Economic Journal*, shortly after his death, stressing in particular the power and originality of this model. In terms of what might have been perhaps a Rowthorn–Goodwin collaboration could have synthesised a macro model which would have been scientifically far superior to the real business cycle, and subsequently dynamic stochastic general equilibrium (DSGE) models which came to dominate economics.

Of course, this hypothesised collaboration is mere speculation, but the fact is that Rowthorn did write purely by himself for much of the time. He is the sole author of a clear majority of his published articles. It is certainly not the case that he was secretive or found it difficult to relate to others. Far from it. As already noted, he is a very generous person, willing to share ideas, assist others in developing theirs, and always trying to see good points, even in arguments which he thought were incorrect. For these reasons, generations of King's undergraduates have regarded him as

an outstanding teacher. One problem for Rowthorn has been that his ideas, his ways of analysing issues, are often strikingly original, at odds with the conventional thinking of the time. So the supply of people, as it were, with whom he could collaborate was limited.

Andrew Glyn was by far his closest collaborator. It is Glyn who stressed the role of profit share in the dynamics of capitalism, and it is to Glyn that Rowthorn refers several times in the 1995 paper. Glyn's premature death in 2007 was a great loss to Rowthorn. Glyn was also an original thinker, though perhaps rather more discursive in his work with not quite the same degree of mathematical rigour with which Rowthorn has always been capable of bringing to bear on a problem. But they saw eye to eye on many issues, not least on the great theme of Rowthorn's work, namely the dynamics of capitalism.

4 Grand Issues in Political Economy

Rowthorn has never been afraid of tackling grand issues in political economy. For a conference in Japan in 2014, for example, he set out scenarios into the mid-twenty-first century in a paper entitled 'The Emergence of China and India as Great Powers'. The paper is far-reaching in its scope. Rowthorn explores the long-term implications for international trade and investment, not least the relative importance of the rest of the EU to the UK, and goes on to consider the impact of China and India on the economies of sub-Saharan Africa. This is followed by a discussion of the changing balance of power in the world, including a discussion of the military balance.

The importance of institutional responses to the eventual outcome is emphasised, in particular those of the current imperial power, the USA. Intriguingly, Rowthorn sets his arguments in the context of Lenin's classic work *Imperialism, the Highest Stage of Capitalism*. Lenin identified imperialism as a special stage of capitalism, with five key features. Rowthorn makes it clear that the world has not evolved exactly as Lenin expected, but some of his predictions have come true. Large swathes of the world economy are now dominated by giant global firms that compete fiercely with each other, and the export of capital, as distinguished from the export of commodities, has acquired exceptional importance. This sets the scene for a detailed discussion of whether the leading individual capitalist states support 'their' firms in the global struggle, or whether, as is fashionably thought, giant firms are losing their national identity. Citing sources such as the *Harvard Business Review*, Rowthorn comes down rather firmly on the side of Lenin on this point. Overall, the paper is one which most academic economists would never have even dreamed of writing.

Rowthorn has a long-standing interest in regional questions within developed economies, in particular, but not exclusively, the UK. A somewhat unusual paper, for Rowthorn, is one which he wrote with Andrew Glyn in 2006 on the convergence and stability of regional employment in the USA. Rowthorn's applied analysis is usually based upon rather straightforward, classical econometrics, and it is his close understanding of the data and ability to form hypotheses which are the distinguishing and powerful features of his work. In the article with Glyn, the same very thorough appreciation of the data is present, and a substantial part is taken up with problems of measurement error in the data. The unusual aspect here is that Rowthorn uses modern developments in econometric time series analysis such as augmented Dickey–Fuller tests and split trends of the Perron type. Typically, the conclusions run contrary to conventional wisdom. Migration and other forces behind regional adjustment to shocks are now quite weak in the USA, and they cannot therefore explain the modern success of monetary union in that country.

The typical approach which Rowthorn has taken to regional economic issues is very much in the tradition of the Department of Applied Economics at Cambridge. A model is developed whose initial base is that of various identities in the national accounts. A thorough knowledge of national accounting principles can of itself generate powerful implications. Behavioural content is then added in the form of further aggregate equations, and the properties of the model are explored.

Rowthorn's 2010 paper in *Spatial Economic Analysis* is a good illustration of his work in this area. The article is concerned with the geography of structural change in Great Britain since 1971. It divides the country into two broad areas: the 'North' comprising Northern England, the West Midlands, Wales, and Scotland, and the 'South' comprising the rest of mainland Britain. The particular application is to the economy of the UK, but this two-sector model of economic development has much more general applicability. The paper documents the uneven regional impact of industrial decline and the rise of the new service economy. Rowthorn constructs what he calls a 'simple' mathematical model to link together regional competitiveness, employment, fiscal transfers, population, and migration. True, the maths is simple, but the economic insights are powerful. Rowthorn shows the crucial importance of the 'export base'. In the long run, the performance of a region depends upon the private sector, which in turn depends on the ability of this sector to produce tradable goods and services. The implications for policy makers are not particularly welcome. There are no quick fixes, because of the sheer scale of the imbalances which Rowthorn identifies, and the problem can only be partly mitigated by increased fiscal transfers between

the North and the South. Drastic measures are needed. One set put forward by Rowthorn involves, somewhat paradoxically, boosting the successful South by massive investment in the transport infrastructure and ending zoning restrictions on housing and business development. This would permit migration on a sufficient scale from the North.

5 A Cambridge Approach to Marriage, Immigration, and Trust

During the past two decades or so, Rowthorn's interests have widened even further to address major social topics of the day, such as family structure and immigration. His 1999 paper in the *Cambridge Journal of Economics*—once again!—is a detailed theoretical analysis of marriage, supported by extensive citations of empirical evidence. The concept that economics can say useful things about social issues such as family structure was, of course, the innovation of the Chicago economist Gary Becker. Becker obtained due recognition for this with his subsequent award of the Nobel Prize. But his work is set in the context of individual utility maximisation and essentially takes as a background the institutional structure of marriage and the family as it existed in the Midwest of the USA in the 1950s. Essentially, through marriage, agents realise the gains from comparative advantage, a concept introduced by Ricardo. Women specialise in rearing children and doing the housework, men specialise by going out to work. Of course, this is something of a caricature of Becker's work, but, like most caricatures, it contains substantial elements of truth.

Rowthorn's approach, in keeping with the Cambridge tradition, eschews marginal analysis and the concept of utility maximisation. His 1999 paper examines the role of marriage as an institution for providing couples with the confidence to make long-term investments in their relationship. The basic theme is that marriage should be seen as an institution for creating trust between individuals in the sphere of family life and that legal and social policy should be fashioned so as to allow this function to be effectively performed. However, many of the legal and social reforms which have been implemented in modern times have undermined the ability of marriage to perform its basic role as a trust-creating institution.

We can usefully think of the decision to marry as an example of decision-making under uncertainty, where the word 'uncertainty' is used in the Knightian sense of not knowing the probability distribution of outcomes.

In many circumstances, the idea that agents lack the capacity to gather and process information in ways which enable them to optimise was stressed by Herbert Simon in his brilliant 1955 paper already mentioned above. Keynes's *General Theory* is of course replete with examples of his view that agents are often unable to foresee the consequences of their decisions, other than in the very short term. His concept of 'animal spirits' is essentially a psychologically based theory of how agents attempt to deal with fundamental uncertainty and are hence able to take decisions in the face of it, rather than being paralysed into inactivity.

Confronted by inherent uncertainty, Simon argued that agents use heuristics, and the concept of optimisation makes little or no sense. Rowthorn sets out a range of ways in which agents can be assisted in the process of decision-making in such circumstances. His paper is therefore not merely considerably more general than that of Becker in the specific context of marriage. It is an important contribution to the much wider and fundamental issue of decision-making under uncertainty, which mainstream economics does not really address at all.

Rowthorn was not content merely to write academic papers on the topics of family structure. He convened an active study group involving think-tankers at the forefront of the interface between research and policy on this matter. Rowthorn and I authored an article in the magazine *Prospect* arguing that marriage is the best family structure and public policy should recognise this fact (Ormerod and Rowthorn 2001). In doing so, we incurred the wrath of members of the metropolitan liberal elite, for whom it was a matter of faith rather than theory and evidence that all family structures were of equal merit.

The outrage has been even greater over Rowthorn's work on immigration, and in particular his two articles in *Prospect* in 2003 and 2006 (Rowthorn 2003, 2006), and his recent monograph for the respected think tank Civitas (Rowthorn 2014b). Again, however, Rowthorn's more popular, policy-oriented work is based upon sound scientific foundations, both theoretical and empirical. For example, his paper with one of the UK's leading demographers, David Coleman of Oxford, in *Population and Development Review* (Coleman and Rowthorn 2004), and his own subsequent papers in the *Oxford Review of Economic Policy* (Rowthorn 2008a) and *Spatial Economic Analysis* (Rowthorn 2008b). Rowthorn has also drawn on his more theoretical work on the emergence of trust and altruism, concepts directly related to the integration of minorities, in the *Economic Journal* and in *Evolution and Human Behavior*.

6 Conclusion

Standing back and surveying Rowthorn's work, the striking features are the breadth and depth of his contributions. Here is a scholar, completely familiar with both economic and econometric theory, addressing major social and economic issues in powerful and often original ways. The grand theme of his writings is the dynamics of capitalism, the central issue in political economy. As the material above makes clear, Rowthorn has also made important contributions on a wide range of topics.

Rowthorn has always imposed upon himself the essential scientific discipline of looking at empirical evidence when developing a theoretical argument. This is a characteristic which he has in common with great American macro-economists such as Robert Barro and Milton Friedman. Whatever the devotees of the Cambridge tradition may think of their work, they, too, have combined theory with applied analysis and evidence. Overall, Rowthorn's work stands comparison with that of any living economist.

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Ajit Singh (1940–2015)

F.M. Scherer

1 Introduction

Ajit Singh was a highly productive member of the Cambridge Faculty of Economics, with extensive publications spanning many areas, but with an emphasis on the linkages among capitalistic industrial enterprise, financial markets, and economic development.

Born in 1940 in what became a part of Pakistan under the partition of 1947, he took his undergraduate degree at India's Punjab University. After establishing new roots and receiving a master's degree at Cambridge, he completed his PhD studies at the University of California, Berkeley, in 1970. Meanwhile, he became a University Lecturer at Cambridge in 1968 and was awarded a full Professorship Chair in 1995, entering mandatory retirement (at least officially) in 2007 until his death in 2015. He issued a stream of publications despite health problems that could stall the careers of less energetic scholars. This survey, emphasizing works that are widely cited, according to Google Scholar, can only cover the highlights. The reader is referred to his curriculum vitae for a comprehensive articulation. My review of his writings

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for this chapter revealed that we have been ploughing parallel furrows on many occasions, so I will refer to my own work with immodest frequency.

2 *Take-overs*

One of Singh's major research themes, recognized as seminal throughout the world, stemmed from his PhD dissertation. Singh investigated the effects of inter-firm consolidations, or more precisely, take-overs, on both the prospective and post-merger behaviour of modern business corporations. An impetus for Singh's research was a provocative set of behavioural hypotheses crystallized in the UK by Robin Marris (1964) and in the USA by scholars such as Henry Manne (1965). They advanced two main points. First, the top managers of large corporations, often owning only a small fraction of the common stock shares of the companies they manage, have substantial discretion to build empires and otherwise deviate from efficient operation and thus fall short of maximizing their company's potential profits. Second, if they stray in this way, their companies' common stock prices will be depressed, creating incentives for other entrepreneurs to initiate a take-over which, if successful, leads to operational improvements and nudges the target companies in the direction of more effective profit maximization. The looming presence of this take-over threat, it was argued, creates incentives for top managers to redouble their efforts to achieve profit maximization.

For a 1971 book and a major 1975 article (Singh 1971, 1975), Singh gathered evidence on the financial performance of corporations whose common stock shares were quoted on the London Stock Exchange. His focus in the book was companies subjected to what he called take-over (on which semantic differences will materialize shortly) during the 1955–1960 period and in the later article, on take-overs occurring during a take-over wave between 1967 and 1970. In both research efforts, he found that companies in the lowest profitability ranges were indeed more likely to be taken over than those of intermediate or high pre-merger profitability. However, this effect petered out at intermediate profit levels, and the supposed merger-inducing profit differences were small compared to a more powerful variable: absolute company size. From this, Singh concluded, there was a strong incentive for companies to increase their size through an active acquisition programme. Moreover, unless take-overs (under his nomenclature) motivated by the goal of greater size per se yielded economies of scale, such take-over avoidance actions could be counterproductive and fail to drive profits towards potential maxima. Whether this happened could be tested by determining whether

profits of the combined entities did indeed, taking into account other relevant variables, rise relative to their pre-take-over values. Singh's analysis of his own limited data on this point and his review of studies done by others supported the inference that take-overs were not profit increasing on average, weakening the Marris–Manne case for take-overs as an efficiency-increasing mechanism. An analysis of much richer data by Singh's Cambridge colleague, Geoffrey Meeks (1977), supported Singh's inference that the profit results of mergers were in fact disappointing on average.

My own research on mergers was in part stimulated by Singh's work. Two important clarifying insights emerged. First was a semantic clarification. Singh included as 'take-overs' all combinations of stock market-listed companies other than those that led to a new name for the combined enterprise, that is, $A + B = A'$.¹ My book with Ravenscraft (1987) distinguished among other things corporate fusions that were accomplished by more or less friendly negotiation between corporate leaders (in American parlance, mergers) and those in which the acquiring company appealed through a stock market-based tender offer, which is how the term 'take-over' was generally used in America. Singh recognized the difference between friendly and hostile combinations but could not secure the data to implement it. We found that the targets of hostile take-overs had pre-merger profit returns as a percentage of assets 2.28 points below the all-sample mean of 11.5%, but the differences among cohort means were not statistically significant.² Tests of *post-merger* performance showed, however, that the tender offer targets and especially the hostile take-over targets reported statistically significant inferior profits. Thus, our research results are consistent with Singh's inference that the take-over mechanism is at best a weak disciplinary force. Moreover, hostile take-overs appear to have been attempted much less frequently in the USA during more recent decades.

3 The Size and Growth of Firms

Even before his *Take-overs* book appeared in 1971, Singh joined a related research project with Geoffrey Whittington examining statistically the size and growth rates of UK-listed corporations. Conventional wisdom during

¹ For Singh, a merger with name change meant $A + B = C$.

² Ravenscraft and Scherer (1987: 70). Our sample covered approximately 6,000 corporate control transactions, of which 25 were consummated hostile take-overs, 50 were tender offers unopposed by target company management, and 20 were tender offers in which a usually unopposed 'white knight' rather than the original tenderer succeeded in effecting the take-over.

the 1950s was that the growth of business firms was more or less deterministic. This view was challenged, invoking what has become known as Gibrat's Law (1931), in seminal papers by Hart and Prais (1956) in England and by Simon and Bonini (1958) in the USA. With a Gibrat-type process, firms grow (or decline) at rates that fluctuate randomly over time, and no firm is assumed in the simplest models to have an advantage or disadvantage sampling from alternative growth possibilities. Naive intuition might suggest that if firms begin a Gibrat process at relatively equal sizes, their relative sizes will be preserved as they grow at randomly determined rates. But this is not true. Some firms will be lucky and draw a string of high growth rates, and some will be similarly unlucky. Also, once a firm through good luck grows to lead its former peers, it has (ignoring scale economies and the like) an equal chance on average to sustain its relative position, and a few firms will continue to be lucky, advancing still more in size. The long-run result is a highly skewed distribution of firm sizes—*asymptotically*, a lognormal distribution—with a few firms towering over the rest. Since most real-world industries in fact have highly skewed size distributions, it was a matter of keen interest whether they had arrived at that state through growth processes of the Gibrat type.

To explore the Gibrat conjectures, Singh and Whittington (1968, 1975) compiled what for the time was an unusually rich database on size measures and growth rates (somewhat surprisingly, emphasizing asset account rather than sales values) for companies listed on British stock exchanges over the years 1948–1960, first for firms specializing in four industry groups and then for a much broader sample. It is amusing in hindsight, especially for one who faced the same 'big data' problems at the same time, to observe that the difficulties of data processing were such in those early days of computerization that Singh and Whittington chose to list their programmer on the title page of their book. Some of their findings confirmed the crude Gibrat hypothesis, for example, that *average* growth rates did not vary much as a function of firm size. But they found other important violations of the Gibrat assumptions, notably, the *variance* of growth rates was lower for larger firms, meaning that those firms enjoyed greater positional stability. Moreover, at least over relatively short intervals, the growth rates for a given firm were not truly random from year to year, but serially correlated (i.e. persistent), so that a run of what seemed to be good luck continued, increasing the relative disparity of firm sizes. In the authors' analysis of profit along with raw firm size data, it was found that similar phenomena appeared—for example, the variance of profit rates was lower for larger firms, high profit rates showed some tendency to persist over time, and (combining the two foci) firms with higher profits tended at least

in some industries to grow more rapidly. Suggesting a link to Singh's work on take-overs, such changes of ownership (which we in the USA were calling mergers) proved to be an important cause of growth rate differences.

In short, the work by Singh and Whittington showed clearly that there were important random influences in corporate growth processes and that these seemingly random growth rate differences led over time to strikingly similar cross-sectional firm size distributions. But much remained to be explored by other investigators on the underlying reasons for observed growth rate differences and how they illuminated theories of firm and industry behaviour.

4 Developing Nations' Financial Structures and World Crises

As an economist raised to maturity in Pakistan and India and then trained on three continents, Singh maintained a keen interest in the international facets of industrial finance and in investigating how financial institutions and economic development have interacted in low-income nations.

During the 1990s, he was commissioned by the World Bank (Singh 1995, 1997, and Singh and Weisse 1998) to investigate a striking phenomenon. Achieving economic development had become a conscious goal of many nations that were subsisting at low average levels of income per capita as the world began recovering from the Second World War. Some embraced state socialism as a path to development. Studying how these strategies functioned and malfunctioned has been a specialized field of economics. Singh, on the other hand, focused his attention on the nations that had chosen a more or less capitalistic, market-oriented path to development. In an early effort (Singh 1995), he found a noteworthy pattern. Like many other developing nations, India had created public stock exchanges. Expanding his coverage in Singh and Weisse (1998), he observed that there were 7,985 companies listed on Indian stock exchanges, compared to 7,671 in the more highly developed USA, 2,078 in the UK, and 678 in the industrial powerhouse Germany. The creation of stock markets, he noted, had been encouraged by less-developed nations as a part of their economic development strategy. But how did the issuing and trading of common stock affect actual development? How did the processes differ in relation to those in wealthier nations?

In early studies (e.g. Singh 1995) for the World Bank, Singh discovered a surprising propensity after accumulating data on the 50 (later 100) largest listed corporations' finances in nine (later ten) less-developed nations. Those

corporations, he found, tended to rely on external finance—that is, issuing stocks and long-term bonds to finance their growth—much more than did listed companies in the USA and the UK (compare Corbett and Jenkinson 1994). Firms in highly developed economies, in contrast, were said to follow a ‘pecking order’ strategy,³ relying first upon internal cash flow from retained earnings and (recognized only in a later Singh paper) depreciation and only then on new bond and stock issues. But in his sample of ten developing nations, the median companies financed more than 40% of their growth from new common stock issues. Why were external sources emphasized more in developing nations? Were there consequences for the success of their economic development efforts?

Singh’s principal explanation for the developing nations’ reliance on external finance issues, in addition to the encouragement that stock markets in those countries received from their national governments, was that at the time of his studies economic development was enjoying a period of unusually buoyant expectations. Money surged into newly established stock markets, including (recognized in later papers) money from first-world investment fund portfolios. This inflow of funds drove stock prices up and hence reduced the implicit cost of capital to the recipient firms, tilting resident firms’ financing strategies towards stock issuance. But he argues (Singh 1997) that it also risked destabilization, in part because Keynes’s ‘animal spirits’ could reverse, shutting off fund inflows abruptly, and also because inward foreign investment decisions were influenced by exchange rate fluctuations. In his Singh (1997) and Singh and Weisse (1998) papers, he opines that the countries he studied would have been better off relying less on domestic and especially foreign stock and bond investments and more, as did Germany and Japan, on fostering powerful domestic banking institutions with detailed knowledge of what was happening in local enterprises and on the patient, stable provision of finance.

Indeed, his fears proved justified. Many developing nations experienced financial crises during the 1990s. Plagued by falling commodity prices, stock prices, and exchange rates, they found themselves unable to meet foreign investors’ demands for cash repatriation. There was intense debate over the right solutions. The prevailing ‘Washington Consensus’ associated with the World Bank and the International Monetary Fund (IMF) (with dissent from economists such as Joseph Stiglitz) was that developing nations should liberalize their financial policies even more, reduce barriers to over-the-border monetary flows, avoid intervention in interest rate setting, and, more generally, curb both government and private expenditures. Indeed, those powerful international finance institutions tried to make support for troubled nations

³ In Singh and Weisse (1998: 611), the authors attribute the ‘pecking order’ hypothesis to a 1994 paper by Corbett and Jenkinson. In fact, it dates back at least to Duesenberry (1958: 91–97), who adds appropriate caveats recognizing the opportunity cost of retained cash flow.

conditional upon the free-market and austerity measures they advocated. Singh adopted a contrary position. He argued (Singh 2002) that developing nations should place restraints upon cross-border financial flows, discourage speculative investment, receive from institutions such as the World Bank and the IMF help in restructuring their debts, and stimulate a return to the strong economic growth that would make them better able to manage their finances. This debate, in which Singh was an incisive participant, was a forerunner of the one that exploded, especially in Europe, following the world financial crisis that peaked in 2008.

Nor was this Singh's first entry into such debates. When oil prices spiked from \$13 per barrel to roughly \$34 per barrel in the wake of the 1969 Iranian Revolution, and when Federal Reserve Chairman Paul Volker's inflation-fighting measures propelled US banks' prime lending interest rate from 9% in 1978 to 19% in 1982, among other things sharply raising the exchange rate of the benchmark US dollar, developing nations, and especially those in Latin America, were hit by crisis. That time too, the World Bank and IMF preached 'austerity', that is, the reduction of public sector borrowing, increased interest rates, and money supply cutbacks. Singh (1986) argued instead for wealthy nations' forgiveness of international debts, foreign exchange controls to curb capital flight, and Keynesian measures to restore borrowing nations' domestic prosperity. Another instrument advocated by him to benefit raw material export-dependent nations was the establishment of international cartels to stabilize commodity prices—a suggestion that even today remains seductive though immensely controversial. (For a more sceptical theoretical and empirical precursor, see Newbery and Stiglitz 1981.)

5 Broader Industrialized Nation Problems

With three co-authors (Glyn et al. 1990), Singh refocused attention on problems emerging for some of the most highly developed nations—notably, the UK, the USA, France, Germany, Italy, and Japan. No dissection of authorship contributions was made, and the authors are listed alphabetically, but it seems clear from the focus and methodology that Singh was a leading contributor. The basic problem was this: after nations emerged from the chaos of the Second World War, they enjoyed very rapid economic growth up to the early 1970s—what the authors and the editor called 'a golden age'. But then followed 'a decade and a half of stagnation' in GDP per capita and productivity growth, among other things. To explain what happened, the authors offer an

analytic tour de force combining historical analysis, an abundance of quantitative data, and insight into the underlying institutions, both micro (e.g. trade union activities) and international (e.g. the role of international monetary authorities).

The authors begin with the kind of rich model one might expect from economists inheriting the tradition of Maynard Keynes. Put far too simply, fiscal and monetary policies determine output, which affects profitability, which motivates investment, which drives productivity, which (along with trade union strength) affects wages, with strong feedback effects from productivity and wage-setting to profits. They then move to what is best described as an 'open economy' analysis, taking into account inter alia the abandonment of tight world currency linkages to the US dollar, floating exchange rates, the oil price shocks of 1973 and 1979, and vanishing trade surpluses and then rising trade deficits for many key nations. The concatenation of these events in the late 1960s and early 1970s is emphasized by the authors as instigators to the profit, productivity, and real wage stagnation that followed, although they point out that there were advance clues of ebbing productivity rates even before 1973.

In this broad framework, productivity plays a central role. Herein lies my principal cavil at what is otherwise a compelling work. There is no mention in the paper of Robert Solow's pioneering (1957) article, which showed that productivity growth was only weakly dependent upon capital-labour changes and was instead largely (i.e. 81–88%) associated with a residual that Solow called 'technical change'. That bombshell gave rise, at least in the USA, to a landmark 1960 conference (Nelson 1962) and then to an explosion of studies by economists on how research, development, and innovation generate technical change, none of which is cited in the Glyn et al. chapter. I was a participant in the 1960 conference and the inter-university committee that emerged from it. A decade later, with Singh et al. and many other economists, I was baffled by the productivity growth decline that became so evident in the 1970s. There were visible precursors. Constant-dollar industrial basic research spending in the USA peaked in 1966 and then fell, resuming its growth only in 1976. US patenting peaked in 1971 and then declined 27%. Moreover, what had been vigorous growth of inflation-adjusted corporate R&D paused in 1969 and then grew only slowly and erratically until 1976. What I learned from several years of research (Scherer 1984: Section IV) was that productivity growth was indeed strongly dependent upon R&D and that the stagnation of the 1970s was significantly traceable to R&D cutbacks. Consistent with the inferences by Glyn et al. (1990), I found that the reductions in corporate R&D were in turn associated with an identifiable fall in profitability. My research findings were inconsistent with the authors' suggestion (*ibid.*: 99) that other nations

were simply ‘duplicating’ American technological innovations more quickly. To the contrary, other nations surged ahead of the USA technologically in many industrial fields, generating what was in effect an oligopolistic R&D race (see also Scherer 1992).

The authors focus their stagnation analysis mainly on the years 1973–1981 and conclude (publishing their contribution in 1990) with a gloomy prospectus for years that follow. In fact, at least in the USA, productivity growth and economic growth more generally rebounded in the late 1980s after the Federal Reserve Board eased its monetary policy, the dollar declined after a surge induced by high interest rates, the OPEC price front collapsed, and importantly, technological opportunities were restocked by advances *inter alia* in microelectronics, fibre optics, and molecular biology. A new ‘golden age’ seemed to be dawning. But it came to an abrupt end with the crash of 2008–2009 and spread persistently across much of the industrialized world. Where we go from there remains as much a mystery as the growth slump of the 1970s was in its heyday.

6 Economic Development Strategy

Singh wrote diverse papers on agriculture, and in particular, its role in economic development for low-income nations. In a 1992 contribution, for example, he and Hamid Tabatabai criticize the ‘enormous prejudice among economic policy makers in the North and orthodox economists’ (Singh and Tabatabai 1992: 417) against active intervention in agricultural markets and in particular against international commodity price agreements. Reviewing the US experience during the Great Depression of the 1930s, the authors note that farm product prices dropped by more than half while, controlling for weather conditions, output rose. Manufacturing output and prices exhibited opposite tendencies. The authors could have strengthened their case by reviewing the record of actual government interventions in US agricultural markets during the 1930s, including destruction of surplus crops, mandated price floors, acreage limits, and purchasing surplus output for distribution to unemployed urban workers. In many of the prime US agricultural commodity markets, powerful interventions continue even now (see, for example, Scherer 1996: Chapter 2). The remarkable fact is that the US interventions (and also their European counterparts) work rather well, despite palpable inefficiencies, among other things preserving a prosperous agricultural workforce and keeping citizens well fed.

An earlier (Singh 1982) chapter addressed the specific problems of agriculture versus industry in Africa. Singh criticizes the view among Washington-based policy makers that stress merely ensuring that basic food needs are met, and only after that, fostering the shift of resources into industrial development. Singh highlights the interdependence between the agricultural and industrial sectors. For one, advanced industrial technology (along with better education) is essential for increasing the productivity of farmers. Such productivity gains in turn release resources that can be used to build the industrial sector. Advancing a theme that recurs in later works, Singh argues (*ibid.*: 31) that ‘in view of the far greater degree of...backwardness of the African countries today...the state, rather than the market, will necessarily have to play the central role in industrial development in these countries’.

In 1994, Singh addressed the central key of economic development even more squarely (Singh 1994). He criticized the earlier blindness (since then partly remedied) of World Bank economists to the role that state intervention played in the economic development of Japan, South Korea, and Taiwan—for example, through picking and supporting industrial ‘winners’, protecting them from import competition during their vulnerable early development stages, encouraging cross-border technology transfer, building educational systems that yield workers able to bring their employers up to the frontiers of technology, and structuring incentive mechanisms to stimulate vigorous and efficient industrial performance. My own opinion is that in these views, Singh was much more correct than those following the Washington consensus (see, for example, Freeman 1987; Amsden 1989).

My principal criticism of Singh’s analysis is that it fails to recognize important differences in diverse Asian nations’ successful development strategies. Japan and South Korea succeeded in joining the ranks of world leaders industrially (for Japan, at least into the early 1990s, after which problems emerged) through the kinds of policies Singh emphasizes, among other things actively fostering the growth of domestic enterprises and discouraging foreign direct investment (FDI). But other Asian tigers pursued strategies that differed in important ways, most notably, with respect to FDI. This is shown by Istiaq Mahmood’s analysis (1999: Table 2) of US patents granted to organizations located in diverse Asian tiger nations. Rapid growth in technological proficiency is shown by the increase in the total number of such patents received by organizations located in South Korea, Taiwan, Singapore, and Hong Kong: from 1,896 in the 1981–1986 period to 12,283 in 1987–1996. The fraction of US patents over the two periods assigned to indigenous, as compared to foreign transplant, organizations was 98% for South Korea and

99% for Taiwan, as compared to 5% for Singapore and 17% for Hong Kong. Similarly, for India, Indonesia, Malaysia, and Thailand, the fraction of patents going to indigenous concerns ranged between 6% and 13%. Clearly, FDI played a much less important role in the technological progress of South Korea and Taiwan (as well as Japan, not included in Mahmood's tabulation) than it did for other Asian tigers.

To Singh's important 1994 paper on economic development, I add only one minor quibble. He credits the German economist Friedrich List (1837) [1983] with conceiving the kinds of protectionist policies Japan adopted in supporting its great leap forward following the Second World War. Yes, the policies were Listian. But List was stimulated to his analysis when he resided in the USA in exile from his native Württemberg, Germany, between 1825 and 1837. He chanced then to read Alexander Hamilton's *Report on the Subject of Manufactures* (1791), written when Hamilton was US Secretary of the Treasury. It advocated a set of policies that, successfully pursued, helped the USA advance from being the specialist in agriculture that Adam Smith advocated in Book II, Chapter V, of *The Wealth of Nations*, to becoming a diversified industrial powerhouse. In addition to restrictions on manufactured goods imports, Hamilton advocated vigorous technology transfer from abroad, and to enhance the one-way emphasis of that policy, the USA granted no patents on inventions by foreign applicants until 1836. It is largely true, as Singh says on page 1,814 of his 1994 paper, that more recently the USA has not had an explicit 'industrial policy'. Rather, by intervening through 'a variety of measures' (ibid.), it lacks what could be called a *coherent* industrial policy. On that, I believe we agree fully.

7 De-industrialization

A recurrent theme in Singh's research was the concern, echoed during the second half of the twentieth century by many other British observers, that Great Britain, once renowned as the workshop of the world, was losing its leadership position and the employment it sustained in manufacturing industries. In a word, it was experiencing 'de-industrialisation'. This theme was addressed *inter alia* in Singh (1977, 1987, 2000).

In assessing these phenomena, Singh stresses two quantitative indicators. One is the share of total civilian employment located in British manufacturing, the other the net balance of foreign trade on the manufacturing account. He dates the peak UK manufacturing employment share at 36.1% to 1955, after

which a marked and accelerating decline set in. He observes with alarm (Singh 1987: 304) that '[i]n 1982, for the first time in a century of its industrial history, Britain...recorded a deficit on its manufacturing trade'. Providing perspective, he observes that the USA has experienced similar vicissitudes. My own more detailed statistics yield an analogous conclusion for the USA (Scherer 1999, 2012). The share of US employment recorded in manufacturing plus mining (the latter with similar export potential, and often vertically integrated with manufacturing) rose fairly steadily from roughly 10% in 1820 and reached a peak (abstracting from a sharp decline during the Great Depression and a spurt for Second World War production) at 30.1% in 1953. It then declined more or less steadily to 11.1% in 2010. Also paralleling Singh's data for Great Britain, the USA experienced a sharp turn to negative manufacturing trade balances during the 1980s (Scherer 1992: 4). The near simultaneity of these changes in Great Britain and the USA suggests, as Singh postulates, that fundamental economic forces were at work.

Considering why manufacturing, agriculture, and services have exhibited such vastly differing employment share trends, Singh stresses two variables (Singh *ibid.*: 303): the income elasticity of sectoral demand and the rate of labour productivity growth. Productivity growth has tended over time to be much higher in agriculture and manufacturing than in services. The income elasticity of demand for agricultural products in prosperous economies has been on the order of 0.2, but much higher for manufactured goods and especially for services. With low-income elasticity and high productivity growth, the share of US employment devoted to agriculture has declined sharply (from 72% in 1820 to 2.5% in 1999). Eventually, productivity growth outraced the income elasticity effect for manufactures, whereas in services, relatively high-income elasticities exhibited no sign of compensating for low productivity growth. In preparing this chapter, I was astonished to find Singh's correct analysis of the countervailing forces, since I struggled to work them out on my own (Scherer 1999: 20–21, 131) and believed, erroneously, that my derivation was original.

Why should policy makers worry about a declining share of manufactures in national employment and a deteriorating balance of trade in manufactured goods? In a frictionless, static Walras–Heckscher–Ohlin world, there might be scant cause for concern. But the real world is not frictionless, Singh argues: exchange rates and wages are sticky and governments may struggle to maintain something approximating full employment. He warns (Singh 1977: 133) that the balance-of-payments difficulties stemming from a weak competitive position can force governments to restrain aggregate demand, which leads dynamically to lower investment and hence lower growth of

productivity and continuing balance-of-payments difficulties. In that seminal paper (*ibid.*: 128), Singh generalizes:

Given the normal levels of the other components of the balance of payments, we may define an efficient manufacturing sector as one which...not only satisfies the demands of consumers at home, but is also able to sell enough of its products abroad to pay for the nation's import requirements. This is, however, subject to the important restriction that an 'efficient' manufacturing sector must be able to achieve these objectives at socially acceptable levels of output, employment, and the exchange rate.

He also worries appropriately about the dynamics. Quoting Nicholas Kaldor's characterization of the manufacturing sector as an 'engine of economic growth', he observes that manufacturing generates technological innovations which advance productivity in other sectors and enhance the quality of exported goods, allowing exporters to capture significant market shares at favourable prices and hence permitting the payment of higher real domestic wages, all else being equal. This too, I believe, is an important truth. During the 1970s, the manufacturing sector conducted roughly 95% of company-financed research and development in the USA, generating a rich flow of products conferring externalities on other sectors of the economy (see Scherer 1984). That share has fallen to roughly 70% more recently with the growth of software and biotech industries,⁴ but manufacturing remains the predominant engine of growth in the USA. Contrary to the glib assertion to the contrary by Michael Boskin, at the time chairman of President Ronald Reagan's Council of Economic Advisers, it matters considerably whether companies are producing 'semiconductor chips or potato chips'.

Challenging the view of some economists that the debate over whether UK and USA firms are losing their 'competitiveness' as a 'dangerous obsession', Singh laments the inferior quality, design, and performance of British manufacturers' products relative to those of international rivals. Moreover, on the other side of the pond, he argues (Howes and Singh 2000: 17) that, 'both from the short-term and long-term perspectives of the United States as well as the world economy, the United States needs to improve its national competitiveness in order to achieve sustained faster growth of output and productivity'.

To improve their competitiveness in this context, Singh warns (1977: 134) that the UK and in a later work (Singh 2000) the USA might for a considerable

⁴This is in part a statistical illusion, because biotech start-up firms with no products to sell are assigned by the US Census Bureau to the service sector, but when they begin selling products, they are reassigned to manufacturing.

period of time have to abandon the regime of free trade and free convertibility of currency. I agree that a problem exists and that solutions are needed. I am sceptical, however, whether protectionist tariffs necessarily improve matters. In a statistical analysis of cross-border technological rivalry (Scherer 1992: 152–170), I found that 308 US companies' R&D/sales ratios grew more slowly between 1971 and 1978, the greater was the fraction of their sales protected by import barriers, taking into account variables proxying inter alia for the richness of technological opportunities. Year-to-year changes in company R&D/sales ratios were also negatively related to the strength of import barriers, *ceteris paribus*. The relationships are weak and fall short of the 95% statistical significance point, but the results are at least cautionary. A regression analysis of aggregate investment by the import-plagued US steel industry (Scherer 1996: 193–195) showed, again weakly, that steelmakers *reduced* their investment in plant and equipment, on average by a half billion dollars per year, when import barriers were in place, taking into account also steelmakers' output, cash flow, and interest rates. For infant industries in emerging nations protectionism may well support technological dynamism, but in mature economies like the USA and the UK, barriers may act more as a narcotic than a stimulant. Industrial policies emphasizing wider and better education for engineers and scientists, vigorous support of basic science, and R&D tax credits would seem a more promising route.

In his 2000 book with Howes on international competitiveness, Singh returns to his early research, suggesting that the myopic time horizons companies adopt to keep current profits high and thereby discourage take-over attempts concurrently impede incentives for innovative investment and the cultivation of firm-specific human capital. The authors suggest (*ibid.*: 19) that 'throwing sand in the gears of the market for corporate control' might help encourage longer-term investment perspectives.

8 The New Threat: China and India

Singh (2007) revisits an international facet of the de-industrialization question, focusing on challenges that highly developed economies face from the rapid industrialization of India and especially China. Early impacts include the widening of competitively interlinked world labour markets, the outsourcing of skilled jobs, and dramatic increases in the number of newly trained engineering graduates competing for technology jobs. Whether workers in the earlier industrialized nations have been harmed or benefited by this new competition has been vigorously debated. Singh asks whether the expansion of Asian industries has been complementary to, or competitive

with, advanced nations' economic activity. He argues it has been expansionary on the whole, but does not pin down the locus of spillover gains—for example, to what extent Australian iron-ore miners, Middle Eastern oil producers, American electronic component makers, and German precision machinery makers have benefited from the increased demand, as compared to the job displacement caused by increased Chinese and other newly industrializing nations' finished manufactured goods exports. Singh notes that exports of cheap Chinese and Indian consumer goods have reduced inflationary pressures and raised standards of living (what some characterize as the Wal-Mart effect) for less affluent American consumers, but the overall balance of benefits is left in doubt. Singh cites work by Baily and Lawrence (2004) that manufacturing's falling share of the US workforce was attributable mainly to productivity gains outracing income elasticity effects, and that increased import competition had considerably weaker share-reducing effects. Since the time of his paper, the Chinese renminbi has risen in value relative to other major world currencies, reducing export price pressures, but on the broader implications of this most important development, the final verdict remains to be written.⁵

9 Conclusion

To summarize, Ajit Singh added glory to the University of Cambridge with a sustained outpouring of economic research on the central institutions of modern capitalism. He has focused his efforts on some of the most important issues—how financial market activity affects the incentives and performance of firms cranking out goods and services, how finance institutions and the rise of previously underdeveloped nations interact, the conditions affecting output and employment in industrialized sectors, how the rate of economic development is affected by nations' consciously implemented industrial policies, and how international trade affects workers' prosperity in a world of product differentiation, oligopolistic rivalry, and sticky prices. Despite his official retirement in 2007, Singh continued to conduct research and to write—among other things, publishing in 2013 a fascinating analysis of the similarities and differences between Western and Islamic finance (the latter prohibiting the payment of interest) (Sheng and Singh 2013). The world will miss a highly productive scholar.

⁵ For a recent and more pessimistic analysis, see Autor et al. (2013).

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David Michael Garrood Newbery (1943–)

Michael G. Pollitt

1 Introduction

Starting with Alfred Marshall, Cambridge Economics has distinguished itself in the field of microeconomics at least as much as it has in macroeconomics or econometrics. David Newbery is one of the very best microeconomists that Cambridge has produced in recent decades. As recently as July 2014, he was the highest ranked of any Cambridge economist in the rankings of top UK economists (on RePEc),¹ even though he had retired (formally) from the Faculty in 2010.

David has made many contributions to economics over the years, in development economics, public economics, industrial organisation, economic regulation, transport, and energy economics. He has published over one hundred academic papers and been co-author or co-editor of eight books.² This makes summarising his work rather challenging!³ I am however grateful to the

The author acknowledges the very helpful comments of Robert Cord, Richard Green, and David Newbery.

¹<http://ideas.repec.org/top/top.uk.html>.

²See <http://ideas.repec.org/e/pne15.html>.

³I draw on the CVs available at: <http://www.econ.cam.ac.uk/people/crsid.html?crsid=dmg&group=emeritus> and <http://www.fondation.dauphine.fr/la-fondation/lequipe/personne/prof-david-newbery/>.

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guidance given by Richard Gilbert in his short (but very informative) tribute to David in the special issue of *The Energy Journal* that Richard Green and I edited in 2008 to celebrate David's 65th birthday (Gilbert 2008). In this issue, Stephen Littlechild also makes the point that as Professor of *Applied Economics*, David listed *economic theory* at the beginning of his (long) list of interests (Littlechild 2008: 43), emphasising that David's micro-foundations are at the root of all his work.

David is a Cambridge economist through and through having studied economics at Cambridge and spent all of his working life as a member of the Faculty of Economics between 1966 and his formal retirement in 2010. Since then he has continued to serve as Director of the University's Energy Policy Research Group and continues to have his main office in the Faculty of Economics. His intellectual inspiration at Cambridge was undoubtedly the great economic theorist Frank Hahn—they were both Fellows of Churchill College—of whom he wrote an affectionate obituary (Newbery 2013). However, Frank remained somewhat surprised by David's development from a theorist into an applied economist!

David came up to Trinity College, Cambridge, in 1960 and studied mathematics for two years (having skipped Part I and moved straight to Part II). Unsure of whether to continue to Part III in maths, he had a brief conversation with Jim Mirrlees, who fortunately convinced him to do an additional two-year Part II in economics. He graduated with a First in 1965. He subsequently received his PhD from Cambridge in 1976 (on the basis of published work). David was elected to a Teaching Fellowship at Churchill on graduation in 1965, but took up a one-year post in Tanzania, working in the Treasury on an Overseas Development Institute (ODI) Nuffield Fellowship. This was to prove a formative experience in his sustained interest in the application of sound economics to developing countries throughout his career. On his return to Cambridge, he became a Lecturer in the Faculty of Economics, rising to become Professor of Applied Economics in 1988. David served as the final Director of the Department of Applied Economics (DAE) from 1988 to 2003. Although continuously employed at Cambridge throughout his career, David had a highly significant two-year period at the World Bank between 1981 and 1983 as Chief of Public Economics. He has also had sabbaticals at Stanford, Yale, Princeton, the International Monetary Fund (IMF), and Berkeley.

David has worked with many great economists who passed through Cambridge at some point in their career. He is a co-author with two Nobel Laureates: Joe Stiglitz (2001 for analysis of markets under asymmetric information) and Eric Maskin (2007 for mechanism design theory). He

was supervised by a third, Jim Mirrlees (1996 for theory of incentives under asymmetric information) and with Mirrlees had a fourth, Richard Stone (1984 for the development of systems of national accounts) as his PhD examiner. In addition, he has co-authored with Antony Atkinson, Richard Gilbert, Larry Karp, and Nicholas Stern. Joe Stiglitz refers to several of his joint papers with David in his Nobel Prize Lecture (Stiglitz 2002). David has held international honours as President of the European Economic Association (for 1996) and President of the International Association of Energy Economics (IAEE) (for 2013). In the Queen's Birthday Honours of June 2012, he was awarded a CBE (Commander of the British Empire) for 'services to Economics'. He was elected a Fellow of the British Academy (FBA) in 1991 and received his higher doctorate, a Doctor of Science (ScD), from the University of Cambridge in 2001. In 2002, he was the recipient of the IAEE's Outstanding Contributions to the Profession Award.

He has helped develop generations of Churchill undergraduates, including Richard Smith who has been chair of the Faculty of Economics at Cambridge. His doctoral students have included Richard Green, Professor of Sustainable Energy Business at Imperial College, and Karsten Neuhoff, Professor of Climate Policy at DIW Berlin. He has played a formative role in the careers of many others who have worked with him at the early stages of their careers (including myself). David was a popular University Lecturer, known for his stimulating but challenging lectures on topics in applied welfare economics, where he brought the joys of the Treasury's Green Book on public project appraisal (and other topics which we will get to shortly) to successive cohorts of economics undergraduates.

David is an example, par excellence, of someone who has combined funded academic research and the production of high-quality papers in social science. In the 1980s, he was part of the highly successful Economic and Social Research Council (ESRC) research grant looking at risk, information, and quantity signals (the so-called Risk Project, led for 15 years by Frank Hahn up until 1991). From 1989 to 2010, he led ESRC-funded projects on utility and then electricity market reform, culminating in the award of £2.38 million (in 2005) for the creation of the Electricity Policy Research Group (EPRG). In addition, David won several other competitive research grants, including from the Engineering and Physical Sciences Research Council (EPSRC) and the European Union. David has also contributed as an advisor to many government agencies and departments (most notably the regulatory agencies for energy, water, and railways, Department of the Environment, and the Department of Energy and Climate Change) and acted as a consultant on numerous projects. He served

as a member of the Monopolies and Mergers Commission (the UK's competition authority) from 1996 to 2002. In 2001, he helped establish a consultancy firm—Cambridge Economic Policy Associates (CEPA)—where he is currently acting Chairman of the Board of Directors and Vice President. David has also travelled to many countries to speak and advise: notably Hungary, Poland, the Czech Republic, Bulgaria, Russia, Kenya, Tanzania, South Africa, Brazil, Argentina, Bangladesh, and India. David's capacity for economic work—theoretical, applied, and practical—is prodigious.

David has also made more than his fair share of contributions to the intellectual life of Cambridge, being a regular in the coffee room at the Faculty and at the lunch table at Churchill College. I have been in many a meeting which either began in or repaired to the coffee room on the fourth floor of the Austin Robinson Building because it was 11am or 4pm (i.e. coffee/tea time!), or whose beginning or end was defined by the need for David to cycle off to Churchill to be at lunch between 1pm and 2pm. David's delight in the discussions which arise in those settings is and always has been obvious. His commitment to Churchill culminated in him being elected President of the Senior Common Room in 2010. My own experience as a fresh-faced Lecturer in the Faculty of Economics was David's boundless enthusiasm about what he *just* learnt, and therefore *had* to share. It was David who taught me that the key to being a happy (and productive) academic was to find continuing joy in the *latest* factoid that one could glean about one's topics of interest!

It is difficult to categorise David's contributions to the literature, but I have divided this review into three parts.⁴ The first focuses on his early work, partly arising out of his ODI Fellowship in Tanzania and culminating with his period in Washington at the World Bank. This includes his work with Joe Stiglitz on commodity price stabilisation, with Richard Gilbert on patenting, and with Nicholas Stern on taxation in developing countries. The second looks at his work on the pricing of transport and energy, particularly with respect to efficient road pricing and optimal energy taxation. This includes work with Eric Maskin and Larry Karp. The final part reviews his work on electricity market design, in terms of both the operation of wholesale electricity markets and the regulation of network monopolies. This includes significant work with Richard Gilbert and work arising from his leadership of research projects at the DAE. I do not aim to be comprehensive (I ignore David's work on Hungarian transition and his recent interest in the economics of wind farms!). I do aim to give a flavour of the significance of David's thinking in these areas highlighted by his citation counts.

⁴ I draw on the citation counts in Google Scholar (<http://scholar.google.co.uk/>) to identify David's most significant contributions to the literature.

2 Earlier Theoretical Work, Mostly on Developing Economies

David's early work shows his commitment to the application of economic theory to problems of practical interest, the problems being largely those faced by developing countries. The genius of much of this work is that it starts from the fundamentals of microeconomic analysis and sees how far we can get in the face of the particular problem being addressed. David's work demonstrates that neoclassical economics in practice is not—as it is so often characterised by its critics—about the blind application of textbook models in spite of real-world complexity. It is about providing a place to start in getting a conceptually tractable handle on what might otherwise be too easily (and lazily) characterised as a unique problem by those who dismiss microeconomic theory as being too abstract to have practical value.

David's early theorising drew on his experiences in Tanzania combined with his interest in the advanced theory being produced by his colleagues, notably Tony Atkinson, Jim Mirrlees, and Ken Arrow. Thus in Newbery (1970), David offers an elegant two-page mathematical proof of a point made by Atkinson (1970) that the empirical measures of inequality (such as the Gini coefficient) are unreliable at the national level. David's paper proves that '[t]here exists no additive utility function which ranks income distributions in the same order as the Gini coefficient' (Newbery 1970: 264).

David's lifelong interest in project appraisal is well demonstrated in his 1976 book (with Scott and MacArthur) on *Project Appraisal in Practice: The Little-Mirrlees Method Applied in Kenya*. Little and Mirrlees' (1968) book on social cost–benefit analysis in developing countries made the argument that when conducting a project appraisal of costs and benefits in an underdeveloped economy, world (or border) market prices should be used for traded goods, not local market prices. This correctly reflects the true opportunity cost of goods traded in international markets, whatever their local valuation. However, the shadow price of labour, important for non-traded goods and services in a developing country, that should be used in social cost–benefit analysis is usually below the market wage. As a result, the shadow price that should be used for non-traded goods and services is somewhere between the local market price (evaluated at the official exchange rate) and the world market price. These values need to be estimated, and this is what the authors' work does for Kenya. They argue that for developing countries, shadow prices are usually less than market prices at the official exchange rate because the official exchange rate is overvalued.

The book's empirical calculations include an evaluation of whether it is NPV (net present value) positive to import grain to fatten cattle for export. The argument is that the world price of grain is low (due to agricultural subsidies), while the world price of beef is relatively high. Thus, moving up the value chain (to produce beef rather than grain) can be beneficial for developing countries, even though the opportunity cost of grain *is* its world market price. One can see the value of sound microeconomics to the practical problems of developing countries shining through this sort of argument. As Schafer (1979: 395) quotes in his excellent book review, the authors point out that social cost–benefit analysis is 'an art which can only be learned by practising it'.

The influence of his Tanzanian experience is evident in another significant theoretical paper from David on 'Risk Sharing, Sharecropping and Uncertain Labour Markets' (from 1977). Sharecropping describes a contractual situation where a tenant farmer pays the landlord a fixed share of the crop. Traditionally, this arrangement was thought to be economically inefficient (by no less than Adam Smith himself) because it reduced the marginal incentives to add inputs on the part of the farmer. David's paper sought to explain the circumstances under which sharecropping was actually an efficient arrangement. Like so much of good microeconomics, this paper is in the tradition of trying to explain why something so widespread—that looks like an example of inefficient underdevelopment—has actually a clear efficiency rationale. David's argument looks at sharecropping as a means to spread production risk and as a means to spread input price risk. What David shows is that if wages are correlated with output, then sharecropping can be an efficient way of spreading labour (input) cost-related risk. Here we see a forerunner of David's later work with Joe Stiglitz, in that he models a developing country world where formal risk markets for commodity producers do not exist and where other means have to be found by them to mitigate risk.

David's most cited work is his 1981 book with Stiglitz on *The Theory of Commodity Price Stabilization: A Study in the Economics of Risk*. This is a quite brilliant attempt to extend general equilibrium thinking to incorporate risk, in situations where risk markets are incomplete. This book was written in the context of international (i.e. World Bank) concern about the volatility of commodity prices and the impact of this on developing countries. This had led to calls for commodity price stabilisation (through the holding of international buffer stocks) to stabilise the incomes of developing countries. However, Newbery and Stiglitz point out that it is not that simple and carefully attempt to analyse who gains and who loses and by how much. Rather cheekily, Dowie's

review (1983) suggests the book might have been better titled ‘The Economics of Risk: A Study of Commodity Price Stabilization’ (ibid.: 230).

I can still recall the intellectual thrill of learning (as an undergraduate at Cambridge) the following example which lies at the heart of their insights. Imagine there are two countries producing a single agricultural commodity. There are no insurance markets. Output is perfectly negatively correlated between the two countries. There is unit elasticity of demand in each country. Should we act to stabilise the price of the commodity? No, because if we do that, then we will stabilise the price but not the income of farmers in the country. It is income, not price, which enters utility functions. Indeed, the best thing for the farmers in the countries is that we leave the countries in agricultural autarky because in that case the prices will rise in inverse proportion to national agricultural output and hence agricultural incomes will be stable. This latter point is clearly made by Newbery and Stiglitz in their 1984 *Review of Economic Studies* paper.

The idea that price stabilisation will not be effective at stabilising income or consumption is at the heart of the book. The authors say it is written with three different audiences in mind—policy, agricultural, and general economists (whose various messages are nicely reviewed in Behrman 1985). As is characteristic of David, it starts with the general equilibrium model and relaxes its assumptions in important ways that reflect reality and sees where this goes. In this case, it provides an argument that a favoured intervention—price stabilisation through buffer stocks—is unlikely to work because the net gains are small, while the costs of speculative attacks if the operators of buffer stocks miscalculate are high.

David’s second most cited work is his 1982 *American Economic Review* paper with Richard Gilbert on ‘Preemptive Patenting and the Persistence of Monopoly’. This is another theoretical paper motivated by a real-world example—that of the alleged patent thicket of unused patents created by Xerox to prevent its rivals from competing with its products. In the paper, Gilbert and Newbery show that an incumbent monopolist has more incentive to come up with an unused blocking innovation than a rival firm. The paper characteristically starts with some classical theory and shows the circumstances in which it does not apply.

Arrow (1962) basically argues that incumbent monopolies have less incentive to innovate than a rival firm seeking to gain the monopoly. This is because for a cost-reducing innovation, the monopolist only gets the difference between the profit after the new innovation (π_2) and its initial positive profit ($\pi_1 > 0$; $\pi_1 < \pi_2$). This is less than a rival, who if they can get the monopoly after an innovation gets the same profit (π_2) but a higher increase in profit than the incumbent (who only gets $\pi_2 - \pi_1$). What Gilbert and Newbery brilliantly show (another warm glow comes over me as I recall reading this argument for

the first time as a graduate student!) is that in a differentiated product market, the incentives to innovate are different. If the rival innovates, it gains less than half of the initial monopoly profit of the incumbent, because the market is now a duopoly. On the other hand, if the incumbent prevents the loss of its monopoly by innovating, its incentive to innovate is strictly greater than half of the monopoly profit, because it has stopped the market becoming a duopoly. QED: this suggests that there is a clear incentive for incumbents to create patent thickets if they can. However, in typical David style, the paper is careful to state that it is very difficult to decide in practice whether a particular R&D investment is pre-emptive.

While David was working at the World Bank, he was involved with a project which led to his 1987 co-edited book *The Theory of Taxation for Developing Countries*, with Nicholas Stern. This book focuses on the application of optimal tax theory to developing countries. As Toye (1988) points out in his review, this book (at 694 pages) is actually two books in one: the first co-authored between Stern and Newbery, and the second an edited conference volume. The idea behind the book is to apply Diamond and Mirrlees (1971a, b) thinking on optimal taxation to developing countries. The book consistently suggests that developing countries have very inefficient tax systems (when evaluated from a Diamond–Mirrlees viewpoint) and addresses the important issue of how their tax systems should be reformed. As is characteristic of David's work, appeals to high theory when carefully applied rarely lead to definitive conclusions, though they can give clear guidance in particular cases. As Hines (1989) points out in his review, the editors manage to bring out a consistent message that optimal tax theory is an important reference point for tax reform. This is something very much reflected in David's undergraduate lectures on applied welfare economics and links into the next batch of his work we review.

3 Optimal Taxes and Charges for Transport and Energy

David's Directorship of the DAE beginning in 1988 coincides with the flourishing of his applied work, albeit strongly rooted in the micro-foundations of his earlier research. This work was very much focused on transport and energy. Although his energy work is the better known of the two, I particularly appreciate his research on transport and start with this.

Following his work on optimal taxes in a developing country context, David became very interested in the pricing of goods in developed countries

that were often mispriced by the standards of optimal tax theory. One such target of his writing was the pricing of road use, where David became a leading public advocate of the use of road pricing in Britain in the 1990s. Indeed, it was this work which attracted the most media attention in his career. I still remember—in the early days of my own collaboration with David—waking up to hear him being interviewed about road pricing at a busy junction on BBC Radio 4's flagship morning news programme.

David's public views on road pricing arose directly from his writing. The central problem to be addressed was how to appropriately recover the costs of the road system from its users. This was addressed in his 1988 paper 'Road User Charges in Britain' (Newbery 1988a). This paper discussed the theoretical basis for road charging, calculated the likely amounts raised from optimal charging—for congestion, road damage, and accidents—and compared this to the actual charges paid by road users. Among the themes that emerged were the fact that passenger cars should be heavily taxed because of their contribution to congestion and accidents, not because of their contribution to road damage (which is negligible), while optimal charging would only collect 40% of the total damage costs imposed by heavy goods vehicles (HGVs). Overall, road-related taxes only recovered 70% of total cost, indicating significant room for improvement in tax policy. David's conclusions suggested that the failure to properly account for congestion and accidents in charging meant that decisions on future road investment were unlikely to be sensible. Indeed, a central implication of his work emerges here: new roads, which reduce congestion and accidents, while raising additional fuel taxes, were likely to be highly socially beneficial and self-financing for the Treasury.

David continued his interest in road damage costs in his 1988 *Econometrica* paper: 'Road Damage Externalities and Road User Charges' (Newbery 1988b). This paper won the Frisch Medal of The Econometric Society in 1990, awarded every two years. It theoretically explored optimal road damage charging. What the paper shows is that under certain circumstances 'the externality caused by vehicles damaging roads, which raises the operating cost of subsequent vehicles, exactly cancels out when averaging over roads of different ages' (ibid.: 313). This has the implication that if vehicles are charged per mile in proportion to the direct damage they cause, that would be optimal. This paper further shows that optimal charging of maintenance costs is likely to under-recover such costs while optimal charging for congestion will over-recover marginal capacity costs. Thus, considering both costs together might yield optimal charges which both provide optimal price signals and recover total road network costs. This holds out the possibility that rebalancing road user charges on an optimal tax basis might allow the road system to be self-financing.

This last idea was further explored in Newbery (1989). Here, David shows theoretically and empirically how an optimal road user charge for maintenance and congestion will cover capital and maintenance costs for the road network in the UK. This is interesting because the road user charge should be levied on an equivalent standard axle (ESA) factor basis (which measures the damaging power of each vehicle axle), while congestion should be measured on a passenger car unit (PCU) basis (i.e. relative to the congestion of a representative car). On an ESA basis, an HGV is in orders of magnitude more damaging than a car but in terms of congestion only represents 2–3 PCUs. David's empirical results suggest that while current aggregate road charges may be in line with costs, the misalignment between actual charges and optimal charges means that road investment decisions are unlikely to be optimal. He thus advocates road pricing—well ahead of the Electronic Road Pricing in Singapore (which began in 1998) and the London Congestion Charge (from 2003).

David's most comprehensive paper on road pricing is his *Oxford Review of Economic Policy* paper from 1990. This paper is familiar to generations of Cambridge undergraduates because it includes all of the key diagrams in David's lectures on road pricing to final year students. In this paper, a number of economically correct (but often unpopular with politicians and environmentalists) arguments are strongly made. These include the facts that efficient road pricing would probably justify further road expansion (by correctly showing the positive NPV and self-financing nature of such investments) and reduce the quality-adjusted costs of public transport (a positive externality!). This point about public transport would be especially true if the extra revenues generated by substitution away from using correctly priced urban roads were spent on improving quality. This last point was amply justified by the subsequent positive experience of exactly this happening in the case of the London Congestion Charge.

Another key idea advanced by David (in Newbery 1994) was the idea that a public road authority should be created, drawing on the experience of private regulated network monopolies. The idea was that a commercial entity should be created to own and operate the road network in Great Britain. This entity (which would not necessarily have to be privatised) would have a balance sheet and could consider investment decisions on a commercial basis. The main point of doing this would be to allow proper financial decisions to be made about road investment, following the arguments in David's earlier papers about road charging. It would also allow a more sensible debate about the costs and benefits of road investment and allow the roads authority to borrow to invest where there were socially profitable (and often financially profitable) road projects. This is an idea whose time has not quite come, but it remains a powerful suggestion, which would free road

investment from the political business cycle and represent a considerable supply-side benefit to UK plc.

A favourite paper of mine on road transport is David's 1995 piece discussing the Royal Commission on Transport and the Environment (Newbery 1995a). This Commission reported in 1994, recommending a substantial rise in fuel duty as part of the effort to avoid the forecast doubling of road traffic in the UK out to 2025. David's economic hackles are rather wonderfully raised by this presumption that restraining road transport would be good for society. A particular focus of the paper is the ignoring, by the Commission, of the congestion externality and its overestimation of the emissions impacts of road transport. David sharply points out that doubling fuel duty, as the Commission recommended, would massively distort the relative taxes on emissions between sectors (by raising it by £600/tonne of carbon) and could not be justified on optimal tax grounds. He also makes the point, with which I still delight my own supervision students, that the problem in the UK—given the huge cost of road congestion to the economy—is *too few* roads, not too many!

Like his work on transport, David's initial interest in energy was linked to optimal pricing. David's early work in this area examined the dynamic consistency problem of government policy (similar to Kydland and Prescott 1977), applied to energy taxation. Fundamental to David's approach was the analysis of the pricing problem faced by oil importing governments. David's papers in this area took as a given that the producers had market power, but sought to model the impact of including the fact that consumers (large oil importing countries) *also* had market power. He did this in the context of the theory of exhaustible resources, where the producer price today had to be arbitrated against the price tomorrow, and hence a dynamic schedule of producer prices and taxes had to be calculated.

Newbery (1981) focuses on oil producers, the oil market being characterised as having a Stackelberg leader (cartel) facing a competitive fringe. This paper shows the extent to which the presence of the competitive fringe undermines the power of the cartel and provides incentives to renege on agreements among producers and with consumers.

The problem examined (with Eric Maskin) in 'Disadvantageous Oil Tariffs and Dynamic Consistency' (1990) was that the oil importing country with market power optimally wanted to define a time series of import taxes (known as optimal open loop taxes) which maximised its social welfare. It would likely be optimal to commit to higher taxes tomorrow in order to drive down prices today. However, this would be subject to a time inconsistency problem, in that when tomorrow came, it would be optimal to renege and reduce taxes—from their pre-announced level—in order to reduce consumer prices and increase

oil consumption. This paper also suggests that the presence of low-cost storage might act as a strategic commitment to solve the time inconsistency problem.

The theoretical modelling of the market power of oil importers is further explored with Larry Karp in their 1991 paper, 'OPEC and the U.S. Oil Import Tariff'. This paper notes the similar concentration of oil consumers and oil producers and models OPEC's position in the oil market as being that of a symmetric duopolist with a competitive fringe, the authors noting 'it is somewhat surprising that no-one has proposed this solution concept before' (ibid.: 305). The importing countries in this case should impose optimal import tariffs. The overall impact of the interaction between the three players in the market is that the initial price falls. The optimal modelled US oil import tariff is initially around half the final US consumer price.

This paper was followed by another with Larry Karp in 1992: 'Dynamically Consistent Oil Import Tariffs'. This won the Harry Johnson Prize from the Canadian Economics Association in June 1993, for the best article published in the *Canadian Journal of Economics* in 1992. The paper continued David's investigation of the dynamic inconsistency problem of oil taxation. It explores the differences between buyers and sellers of oil with market power. The results show that buyers with market power are more likely to be dynamically inconsistent while sellers are less likely to be so. The intertemporal price arbitrage of an exhaustible resource turns out to be a dominant effect, which means that complicated intertemporal oil import tax variations, for large importers, have relatively small effects on social welfare. Once again, the USA could benefit from the imposition of significant oil import tariffs. This paper is a good example of David showing how sophisticated theory does not necessarily support complicated (i.e. time-varying) government pricing, but does support obvious pricing of externalities (in this case the consumption externality of oil imports into a large country).

In his 1992 paper, 'Should Carbon Taxes Be Additional to Other Transport Fuel Taxes?', David combines his interests in energy and transport. The title question arises because transport fuel is already heavily taxed—relative to other sources of carbon emissions—in the UK and many other countries. The answer, according to David, is that if anything, the transport fuel tax should go up by at least the carbon tax content equivalent amount. This requires the maximisation of utility less the cost of gasoline, carbon emissions, road use, and congestion, subject to the presence of one pricing instrument—fuel tax. This theoretical paper contains a rather brilliant tax argument to explain why:

A carbon tax will lead to a reduction in the fuel used per km driven because it will encourage greater fuel efficiency. This in turn will reduce the tax base on which the congestion charge per km is to be levied and raise the required road user charge per litre (ibid.: 54).

David's most comprehensive paper on energy taxation is his 2005 'Why Tax Energy? Towards a More Rational Policy' (Newbery 2005a). This paper brings together the insights from his work on both transport and energy. A central theme of the paper is that the differentials in energy taxes between countries and within countries on different fuels cannot be justified on optimal tax grounds and should be harmonised further. These differentials substantially distort trade, especially within the EU Single Market. The paper contains a good summary of David's work on optimal oil import taxes. This shows that the oil taxes in the EU were roughly optimal under certain assumptions, but that by implication, natural gas and coal (given its relatively high carbon content) taxes were far too low, being close to zero in many countries.

'Why Tax Energy?' is an important question because as an intermediate good, it is not clear why it should be taxed in a Diamond and Mirrlees (1971a, b) world. However, as David clearly argues, energy taxes can serve as optimal import tariffs, environmental externality prices, and road user charges. But he expresses doubts about the 'double dividend' argument for energy taxation, which suggests that energy taxes increase overall welfare by allowing the reduction of other distortionary taxation. This is because tax rates relative to other goods are likely to be close to being welfare optimal already, especially when taking distributional arguments into account, while higher energy taxes worsen income distribution. As ever, David wants to take us beyond the simple theory to theory which has taken the empirical realities into account.

4 Electricity Market Design

From 1989 to his formal retirement, David led UK Research Council-funded energy market research, and this has been the main focus of his research since then. His work in this area was substantially driven by real-world events in the energy market in the UK, which made 'the British Electricity Experiment' a world-famous case study in market liberalisation.

The British electricity industry was substantially restructured and privatised in 1990. A key feature of the process was the break-up of the monopoly Central Electricity Generating Board (CEGB). This involved separating the ownership of power plants from the transmission grid and the creation of a competitive wholesale power 'pool' with half-hourly price bidding by individual power plants to meet demand (i.e. a vertical and horizontal unbundling of the incumbent monopoly). David's third most cited paper, published in 1992 with his then DAE colleague Richard Green, looked at 'Competition in the British Electricity Spot Market'. The CEGB was broken up, but the wholesale

market was effectively a duopoly, with National Power and PowerGen controlling 90% of the price-setting power generation plants in the pool.

Green and Newbery analyse the optimal bidding strategies of duopolists in such a power market using supply function equilibria (following Klemperer and Meyer 1989). In a supply function equilibrium setting, the firms maximise profits by choosing a function that relates their output to the market price. Green and Newbery analyse this both theoretically and using reasonable parameter values for the British market. They show the difference between having two firms, rather than five in the market, in terms of company profits and deadweight losses. This paper provided early formal analysis of the failure of the government to break the CEGB up into enough firms to create a truly competitive market, a process that was eventually achieved by a combination of new entry and forced divestitures by 2001.

If the creation of a competitive wholesale market was one central plank of electricity liberalisation championed by David, a second general plank—also a favourite point of David's—was the creation of an effective regulatory environment, where newly privatised network monopolies could invest in the knowledge that they would be allowed to recover a fair return via user charges subsequently. This was one of the central ideas behind the creation of independent regulatory agencies for utility industries, including electricity. Returning to his theoretical roots, David provided a convincing analysis of the economics of regulatory commitment, in a paper with Richard Gilbert in 1994 entitled 'The Dynamic Efficiency of Regulatory Constitutions'. Newbery and Gilbert model the dynamic game played between the regulator and the private monopoly to show the circumstances under which appropriation of private returns was more or less likely. The nice point about this paper is that it models the fact that private companies are more likely to be present when they can provide the network service at lower cost (than if they were nationalised), investment requirements are growing, and where the social discount rate is lower. All of these elements would make appropriation by the regulator less likely.

David spent much of the 1990s arguing for the introduction of more competition into the wholesale power market in Great Britain. Two influential papers which discussed this clearly were his 1995 'Power Markets and Market Power' and his 1998 'Competition, Contracts, and Entry in the Electricity Spot Market'. In Newbery (1995b), he focuses on the need to break up the incumbent duopoly (something which was to effectively happen later). In Newbery (1998), he analysed the role of fixed-price contracts between new entrant generators and incumbent suppliers in promoting competition in the face of large incumbent market shares in the wholesale pool. This pointed out how the ability of entrants to sign these contracts moderated the ability of incumbents to raise prices in the pool, hence

mitigating market power. This theoretical proof was supported by the empirical observation of significant new entry into the power pool in England and Wales in the early 1990s, by new generators signing long-term contracts with suppliers.

David was very interested in the measurement of the benefit of electricity liberalisation and encouraged considerable work on this at the DAE. The earliest example of this was his paper with me, published in 1997 on ‘The Restructuring and Privatisation of the CEGB—Was It Worth It?’ This was an attempt at a comprehensive social cost–benefit analysis of the CEGB privatisation, drawing on earlier work for the World Bank (Jones et al. 1990). It was based on putting the constituent parts of the CEGB back together after privatisation to construct a time series through privatisation of revenues and costs. This was then compared to a counterfactual of what might have been expected to happen in the absence of privatisation (based on trend costs and returns in the public sector). The differences between the actual and the counterfactual, including operating cost, investment, and emissions effects, were then discounted to calculate an NPV of the privatisation, which we allocated between consumers, producers, and the government. The overall result was a small but positive benefit from the privatisation (equivalent to a permanent reduction in costs of around 5%), which went to the government and the producers (with consumers losing out). It was a result which was echoed in subsequent studies of electricity privatisations.

Much of David’s later research was to draw on this initial work as he sought to communicate the lessons of the British experience to an international audience. Among the most notable of his writings was his European Economics Association Presidential Address, ‘Privatisation and Liberalisation of Network Utilities’, published in 1997. In this, he highlights the role of economists in analysing the restructuring of network utilities, noting that ‘the gap between rather abstract theoretical models and the important specific features of each utility is narrowing’ (ibid.: 381) and no doubt with a glint in his eye, ‘the variety of experiments under way in an increasing number of countries...offers a tempting menu of problems to keep economists intellectually stimulated, financially rewarded, and socially productive’ (ibid.).

David wrote ‘Problems of Liberalising the Electricity Industry’ in the *European Economic Review* in 2002 following the botched reform of the wholesale electricity market in California (supposedly based on the British model), which halted the progress of electricity market liberalisation in many countries. In this paper, he highlights a favourite theme of his later work: the importance of adequate transmission capacity to effectively increase the size of the wholesale power market. David has long had an interest in the role of transmission grids in liberalised power markets. A significant theoretical contribution on this is his 2004 paper with Richard Gilbert and Karsten Neuhoff on ‘Allocating

Transmission to Mitigate Market Power in Electricity Networks'. The authors explore the conditions under which generators with market power should be allowed to buy capacity on transmission grids. In general, transmission capacity could be (mis-)used to reinforce incumbent generators' market power (via foreclosure) and hence David has been an advocate of the position that generators must use their transmission capacity or 'lose it'.

David's clearest summary of the lessons from the British experience with electricity liberalisation is in his 2005 paper, 'Electricity Liberalisation in Britain: The Quest for a Satisfactory Wholesale Market Design' (Newbery 2005b). This paper details the importance of the unbundling of transmission ownership from the ownership of power plants; the need to properly price access to transmission grids; the need to address market power in power markets; how transparent power pools can promote new entry; the need to understand whether the subsequent integration of generation and retail markets is beneficial for competition; and the superiority of the competitive wholesale markets and independently regulated natural monopoly networks over the fully vertically integrated incumbents observed in the USA. David also discusses a favourite theme of his, whether the reform of the British wholesale market in 2001, which saw a compulsory power pool replaced by bilateral contracts and a balancing market, was beneficial. David argues, as he has done consistently, that this was a costly mistake, which did nothing to promote competition.

Much of David's wisdom on liberalised electricity markets, and reflections on network utility reforms in general, is brilliantly distilled into his 2000 book, based on lectures he gave in 1995, *Privatization, Restructuring, and Regulation of Network Utilities*. The book remains one of the best introductions to both the theoretical and empirical economics of network industry liberalisation. David carefully distinguishes between the impacts of privatisation, competition, and regulation. He contrasts successful liberalisation with both public ownership (as seen in pre-privatisation Europe) and traditional rate-of-return regulation (as practised in the USA). David argues how each element can contribute to a successful liberalisation: privatisation makes it possible, competition handles the potentially competitive segments, and incentive regulation can regulate the remaining monopoly networks. He draws extensively on his theoretical work on regulation and his empirical studies of electricity privatisation.

5 Conclusion

Summing up David's contributions to the literature is difficult. However, Vogelsang (2001) puts it rather nicely in his book review: 'Newbery is a profound thinker. Without being overly formal, [his work] is therefore intense

reading that requires pause from time to time, in order to absorb the material' (ibid.: 484). The many fans of David's combination of a rigorous neoclassical thinking applied to the detail of real-world microeconomic problems would recognise and appreciate this in all of his work.

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Anthony B. Atkinson (1944–)

Stephen P. Jenkins

1 Introduction

It is an honour to write about Anthony (Tony) Atkinson's contributions to economics and his links with the University of Cambridge where he was both student and Faculty member. From a personal point of view, I owe Tony a huge amount for his exemplary supervision of my doctoral thesis, his friendly but incisive comments on later research, and his ever-present general encouragement, support, and inspiration. For more than 35 years, answering the 'What would Tony think?' question has been a means by which I and many of my contemporaries have assessed our own research. (Our salutes to him are collected in Jenkins and Micklewright 2007.) These personal benefits are of course separate from the immense contributions to economics that Tony has made, many of which had their gestation and nurturing in a Cambridge context. It is these public dimensions that I focus on in this chapter.

I owe Andrea Brandolini and John Micklewright many heartfelt thanks for their substantial contributions. The remarks attributed to Tony Atkinson derive from an interview that Andrea, John, and I did with Tony in October 2014. We thank Tony and Judith Atkinson for their generous hospitality and provision of information about the Cambridge years. The research for this chapter was partially supported by core funding of the Research Centre on Micro-Social Change at the Institute for Social and Economic Research by the University of Essex and the UK Economic and Social Research Council (award ES/L0009153).

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Tony Atkinson has made fundamental and original theoretical contributions to economics in general, and to public economics and the analysis of economic inequality in particular. He has also undertaken original and innovative empirical analysis of economic inequalities, and of their relationship to economic institutions such as the welfare state. His work has brought the analysis of distributional issues back to a central position in economics, arguably a position that it has not had since the period of the classical economists of the late eighteenth and early nineteenth centuries. It is no overstatement to say that the modern analysis of economic inequality started with Tony's 1970 paper in the *Journal of Economic Theory*.

The rest of this chapter substantiates these remarks. (I do not talk about Tony's considerable influence on other social science disciplines such as social policy and social statistics.) First, I provide some Cambridge context. Second, I describe Tony's theoretical contributions to the analysis of economic inequality and then broaden the scope to highlight his contributions more widely to public economics and to the understanding of contemporary society and public policy. Tony's integrative role is a topic I return to in the concluding section.

2 Tony Atkinson and Cambridge, England

Tony went up to Churchill College, Cambridge, in 1963 intending to study mathematics, and he completed Part I before transferring to economics. His interest and engagement in social problems was long standing, however. He had spent 11 months working for IBM (thinking he might go into computing), and then six months in a hospital in a deprived area of Hamburg, taking along reading matter such as John Hicks's *Social Framework* and Joan Robinson's *Economic Philosophy*. It was on an early outing of the Student Community Action volunteer society that Tony met his future wife and fellow social conscience, Judith (they married in Tony's third year).

Tony has remarked on how lucky he feels to have been at Cambridge in the mid-1960s, citing several contributing factors. Compared to today, it felt as if there was relatively little to learn—one was directly at the frontiers of the subject. The reading was not of textbooks but recent journal articles and surveys such as Hahn and Matthews's (1964) review of economic growth theory in the *Economic Journal*, Chipman's (1965a, 1965b, 1966) three-part review of international trade theory in *Econometrica*, and Mishan's (1960) review of welfare economics in the *Economic Journal*. There was a general air of excitement concerning the role that economics could play in public policy; it was integral to thinking about social as well as economic issues. Many Cambridge economists were actively involved

in advising the then new Labour government. James Meade was writing not only academic articles and books (e.g. Meade 1964), but also the Institute of Economic Affairs' Hobart Papers on topics such as Britain's role in the Commonwealth and European Union (EU) (Meade 1962).

Economics was taught as an integrated subject with less of a clear-cut divide between theory and applied work than in today's mainstream core (a distinguishing feature of Tony's own research, as I explain below). The Tripos courses were Economic Principles and Economic Problems, for instance, not Microeconomics and Macroeconomics. Teaching included economic statistics in general and national income accounting in particular (the *Blue Book* was a required purchase). However, econometrics was not well developed in the curriculum, and Tony has remarked how he found his time at the Massachusetts Institute of Technology (MIT) (see below) an eye-opener in this respect.

There were influential teachers such as Jan Graaf and Frank Hahn (Tony's college Tutors), Richard Goodwin, Michael Farrell, Brian Reddaway, and especially James Meade. Visitors included Peter Diamond, Jacques Drèze, Edmund Malinvaud, Joe Stiglitz, and Christian von Weizsäcker. There were colleagues such as Partha Dasgupta, Geoff Heal, and David Newbery and older Faculty members such as James Mirrlees. Interaction was facilitated by a strong tradition of having coffee together each morning in the Faculty Common Room.

In the summer of his third year (1967), Tony benefited from Hahn's long-standing exchange programme with MIT: Cambridge, England, led to Cambridge, Massachusetts, where Tony was a research assistant to Robert Solow and made lifelong connections with people such as Avinash Dixit and Robert Hall (with whom he shared an office) and also interacted with William Branson, Robert Gordon, Bronwyn Hall, William Nordhaus, Michael Rothschild, Karl Shell, Eytan Sheshinski, Joe Stiglitz, and Martin Weitzman.

Tony returned to Cambridge, England, to take up a Fellowship at St John's College and then also a University Lectureship (1967–1971). He subsequently moved to chairs at the University of Essex (1971–1975), University College London (1976–1979), and the London School of Economics (LSE) (1980–1992). He briefly returned to Cambridge as Professor of Political Economy and Fellow of Churchill College (1992–1994) before becoming Warden of Nuffield College, Oxford (1994–2005). Tony remains an Honorary Fellow of Nuffield College and is also a Centennial Professor at the LSE. He is also an Honorary Fellow of Churchill College and so, as he said when accepting the post, he has now come full circle in Cambridge terms.

Tony's return to Cambridge at the end of the 1960s was the period when he learnt public finance in order to teach it, he says. From such beginnings came

his magisterial textbook with Stiglitz (*Lectures on Public Economics*, 1980, republished 2015), and his founding editorship of the influential *Journal of Public Economics* (1971–1997), with the collaboration of co-editors Martin Feldstein, Leif Johansen, Joe Stiglitz, and (from 1973) Nick Stern.

The Cambridge integration of economic principles and empirical analysis of a major public policy issue emerged in Tony's first book, *Poverty in Britain and the Reform of Social Security* (Atkinson 1969a). Brian Reddaway was very supportive of this work, and James Meade was interested in it as well, though Tony has remarked that poverty was not generally seen at that time as a subject for economics.

Tony's Fellowship period was also when his first journal articles appeared—in both economic growth and economic inequality. The first two published articles fully reflect the spirit of the time, and have connections to the two Cambridges, the places of his formation. In his 1969 *Review of Economic Studies* paper, Tony uncovers an overlooked property of the standard model of economic growth as well as two other contemporary models, that with heterogeneous capital goods due to Shell and Stiglitz (1967) and the Marxian–Keynesian growth-cycle model developed by Goodwin (1967), one of his Cambridge (England) teachers. The motivation of Tony's article is clearly spelled out at the outset:

[T]he speed of change is a prediction of the model, and by examining this we have a further test of the model's properties ... If we throw away information about the time dimension, we are reducing still further our limited understanding of the relationship between these models and the real world (Atkinson 1969b: 137).

Tony then goes on to argue that the 'stylized fact' that factor shares are broadly constant over time cannot be a reason to reject the plausibility of capital-augmenting technical progress if the convergence to the long-run equilibrium takes a very long time.

In Tony's *Economic Journal* paper with Stiglitz (Atkinson and Stiglitz 1969), technical progress is suggested to be local, that is, confined to some techniques, so that the production function does not change smoothly: 'The view of technical progress presented in this note contrasts sharply with the dominant mathematical theories of growth, which are essentially ahistorical in character. Where technical progress is localised, history is very important' (ibid.: 577). According to Daron Acemoglu, this paper 'was ahead of its time in...challenging the orthodoxy in the modelling of technological change' (Acemoglu 2015: 456).

In addition to their importance for economics, these first articles already reveal distinctive characteristics of Tony's lifelong approach to research: the need to work out the full implications of theoretical models, an open-minded attitude to alternative modelling choices matched by an awareness of the limits of these models, careful alignment of data and theory, and attention to historical developments.

Tony has also said that he benefited throughout his career from having wonderful collaborators—and that is what they say about him too! According to Tony, his generation was perhaps the first to be routinely engaged in joint authorship, often internationally, and he has said that it has meant a great deal to him personally. These interactions are perhaps a modern equivalent of those that Tony first experienced in the Common Room of the Cambridge Faculty of Economics.

3 Inequality Measurement and Atkinson (1970)

The state of the analysis of income distribution prior to the 1970s was well summarised by Tony himself: '[D]espite the wide use of [various inequality] measures, relatively little attention has been given to the conceptual problems involved in the measurement of inequality and there have been few contributions to the theoretical foundations of the subject' (Atkinson 1970: 244). His own analysis remedied this. It is no exaggeration to say that Tony's path-breaking 1970 *Journal of Economic Theory* article has totally changed the way in which economists, and more generally social scientists, think about these issues by providing the foundations of the modern theory of inequality measurement.

The 1970 article made three crucial contributions. The first was theoretical—a conceptual breakthrough relative to the tradition. By emphasising the welfare economic foundations of inequality measurement, Tony brought income distribution analysis back into the realm of economic analysis. Tony made his own the observation by Dalton (1920) that some concept of social welfare underlies all measures of inequality. With his arguments for making the normative features of different inequality indices explicit, Tony destroyed any idea that choice of a specific inequality index was a value-free (or irrelevant) 'statistical' choice.

Tony proposed evaluation of an income distribution y using a 'social welfare function' that is an additively separable and symmetric function of each person's income:

$$W(\mathbf{y}) = \left(\frac{1}{n} \right) \sum_{i=1}^n U(y_i)$$

where $U(y_i)$ is an increasing and concave function of each person's income. Thus, social welfare increases if the income of any one person increases, other things being equal (the Pareto Principle). A social preference for equality is also incorporated by the concavity assumption: it means that a rank-preserving income transfer from a richer person to a poorer person increases social welfare, other things being equal (equality preference). When $U(y_i)$ is taken to represent utility, Tony's formulation is consistent with the utilitarian social evaluation function that has long been at the heart of most welfare economics, theoretical and applied. But the utilitarian reading is only one possible interpretation of a formulation that is far more general. As emphasised by Tony in later reflections on developments sparked by his 1970 paper, 'the analysis is quite consistent with a variety of views about principles of economic justice' (Atkinson 1983: 5).

Tony showed how commonly used inequality indices were special cases of this general specification and how they each quantified assumptions about equality preference in different ways. In particular, some indices are relatively sensitive to income differences at the bottom of the distribution, some are middle-income sensitive, and some are top-income sensitive. Some indices are not consistent with the evaluation function at all.

Tony's second contribution was also conceptual, but with decisive practical consequences. He devised a straightforward means of checking whether two distributions could be ordered in terms of their inequality according to *all* standard inequality indices (those consistent with the social welfare function above). In other words, he provided a means of drawing unanimous conclusions about inequality differences even if there was not unanimity about the precise form of the social welfare function—a so-called 'dominance' result. Tony proved that unanimous inequality orderings were equivalent to non-intersection of Lorenz curves for each distribution. (The curves introduced by Lorenz (1905) plot cumulative income shares against cumulative population shares, where the population is ordered from poorest to richest and the cumulation is from the bottom upwards.) As a corollary, intersecting Lorenz curves are a warning about the consequences of drawing conclusions from a single inequality index.

The power of Tony's argument was to show how straightforward graphical devices such as the Lorenz curve and its higher-order derivations have important implications from both a normative standpoint (illustrating the extent of the restrictions of the social preferences that are necessary to order two distributions) and a statistical standpoint (showing why different inequality

indices may lead to contradicting results). Although the Lorenz curve had been a well-known tool since its introduction, its normative interpretation had been missed by researchers.

Tony's third contribution was the one with the most explicit practical implications. He provided a new class of inequality measures in which, for the first time, different attitudes to equality preference were incorporated *explicitly* via a single parameter which could be straightforwardly interpreted as the 'degree of inequality aversion': the greater the aversion, the more weight that is given to differences in income at the bottom of the distribution relative to those at the top. To do this, Tony introduced the concept of the equally distributed equivalent level of income (y_{ede}) which is 'the level of income per head which if equally distributed would give the same level of social welfare as the present distribution' (Atkinson 1970: 250). He proposed measuring the degree of inequality I as the proportionate shortfall of the 'equally distributed equivalent' income from the mean income μ (i.e. the per capita income consistent with complete equality of income):

$$I = 1 - \left(\frac{y_{ede}}{\mu} \right)$$

Tony's class of inequality measures was derived by combining this specification with a single parameter functional form for the $U(\cdot)$ exhibiting constant relative inequality aversion.

Tony's article has had a huge influence on subsequent theoretical and empirical analysis. It has stimulated a whole new field of enquiry in which authors have sought to develop his ideas (more about these developments shortly). Empirical researchers now routinely use Lorenz dominance checks and calculate 'Atkinson' inequality indices as part of their standard 'tool box', whether looking at inequalities of income, wealth, or consumption expenditure. For example, the US Bureau of the Census publishes estimates of three Atkinson indices annually in the US official income distribution statistics (the 'P-60' series).

Tony's application of the most recent advances in the probability distribution theory literature about measurement of risk to income distribution analysis was an exemplar of cross-fertilisation, a remarkable case of how lateral thinking can lead to innovative and original results. His derivation of the link between unanimous inequality orderings and Lorenz curve configurations was new. Some of Tony's results were derived independently by Serge Kolm (1969) at much the same time, and Tony himself acknowledges this in his article. However, as Lambert (1993: viii) puts it, 'it was Tony Atkinson who (in 1970) captured the imagination of the profession and showed the possibilities for implementation of the new measurement techniques'.

Before discussing how the profession, and Tony himself, developed the 1970 article's contributions, it is important to state that they have cross-fertilised other fields in public economics. Analysts often use Tony's conceptualisation of the social welfare function incorporating different social attitudes parametrically when assessing outcomes in their models. For example, Tony's discussion of the value judgements implicit in income distribution comparisons resurfaces in contemporary analysis of economics of climate change. (See, for example, Stern's (2008) Ely Lecture to the American Economic Association which draws on the article subsequently published as Atkinson and Brandolini (2010).)

Tony's 1970 article has also had a very large impact in economics more generally, as evidenced by frequency of citation. When assessing 'what has mattered to Economics since 1970?', Kim et al. (2006: Table 2) report that Atkinson (1970) was the 36th most cited article among the 146 articles published in 41 prominent economics journals between 1970 and 2002 that had received more than 500 citations up to June 2006 (citations recorded in ISI Web of Science/SSCI). Atkinson (1970) is the most cited welfare economics article in the list.

4 Standing on Atkinson's Shoulders: Subsequent Research

Tony's 1970 article stimulated much research by others, keen to build on his key insights and to extend them. Tony continued to develop his work as well of course. In this section, I classify developments under four main headings: the nature of the social welfare function, social welfare comparisons, poverty comparisons, and multidimensional comparisons.

Variations in the Social Welfare Function

Other works have generalised Tony's 1970 results or taken his ideas in a different direction. Early works addressed issues relating to the social welfare function. For example, Dasgupta et al. (1973) generalised Tony's Lorenz dominance result to all Schur-concave evaluation functions, not only concave ones. Blackorby and Donaldson (1978) showed how to uncover the social judgement hidden in any equality index, while Newbery (1970) suggested rejecting the Gini coefficient because of its incompatibility with an additively separable function, causing Sheshinski (1972) to respond that additive separability has no particular appeal. Others have explored different concepts of inequality—for instance, when inequality refers to absolute differences in income rather than

relative differences as in the Atkinson (1970) approach. It turns out that there is a family of absolute inequality indices (with a parameter encapsulating the degree of absolute rather than relative inequality aversion) that is analogous to Tony's class of relative inequality indices (Kolm 1976a, b). Moyes (1987) introduced the concept of an absolute Lorenz curve and showed that unanimous orderings by all standard absolute inequality indices were equivalent to orderings by non-intersecting absolute Lorenz curves—a dominance result analogous to Tony's 1970 Lorenz dominance result for relative income differences.

In the Atkinson tradition, the properties of inequality indices are not derived directly but are related to the properties which are placed on the social welfare function. An important sub-branch of the inequality literature has taken a different approach, characterising general classes of inequality measures by specifying properties on the measures directly *ab initio*. The four basic axioms placed on inequality indices in this tradition are labelled symmetry, scale invariance, replication invariance, and the principle of transfers. Fields and Fei (1978) and Foster (1985) show that orderings according to all inequality indices satisfying these axioms are equivalent to orderings by non-intersecting Lorenz curves, which is of course the Atkinson 1970 result. Moreover, if one adds one more axiom, that of decomposability by population subgroup, then one gets the so-called single-parameter generalised entropy class of inequality indices (see, for example, Bourguignon 1979; Cowell 1980; Shorrocks 1980, 1984). But it turns out that this class includes the entire class of Atkinson inequality indices (monotonically transformed). Thus, Atkinson indices not only link directly to fundamental welfare economic foundations in terms of social welfare functions, but they are also decomposable—a property which has turned out to be remarkably useful in empirical applications (Cowell and Jenkins 1995). (See Atkinson (1993) and Jenkins (1995) for examples that exploit this property to help explain why UK inequality rose so much during the 1980s.)

Social Welfare Comparisons

One of the most fruitful extensions of Atkinson (1970) has been the development of methods for comparisons of income distributions in terms of their *social welfare*, as well as in terms of their inequality. As in Tony's original article, the contributions refer to both dominance (partial orderings) and scalar indices (complete orderings) for social welfare comparisons.

Social welfare comparisons are sensitive to differences in real income levels between two distributions as well as inequality differences and allow for some trade-off between the two (Tony's inequality comparisons ignore differences in mean income by construction). For example, it may be the case that increases

in real income levels are judged sufficient to compensate for some increase in income inequality. Shorrocks (1983) showed that unanimous social welfare orderings according to all measures consistent with essentially the same social welfare function as Tony's were equivalent to orderings by non-intersecting *generalised* Lorenz curves. A generalised Lorenz curve is simply a standard Lorenz curve scaled up vertically at each point by the mean income of the distribution. (See also Kakwani 1984.) A dominance result was also proved by Saposnik (1981), who showed that unanimous orderings of all symmetric increasing (but not necessarily concave) social welfare functions were equivalent to orderings by non-intersecting cumulative distribution functions of income. These second- and first-order social welfare dominance results have proved particularly useful because they also have applications in poverty comparisons (see below) and to examinations of trends in social welfare over time and across countries. Scalar indices of social welfare have also been used for these purposes, and they are often simply Tony's 1970 equally distributed equivalent income or monotonic transformations of it: see, for example, Jenkins (1997) or Gruen and Klasen (2008). A feature of these 'real income' indices, as Sen (1976a) labelled them, is that choosing different values for the inequality aversion parameter allows checking of the sensitivity of results to differences in social judgements.

When Lorenz or generalised Lorenz curves cross, we do not have unanimous inequality and social welfare orderings. Several authors proved how orderings can be derived, nonetheless, in such cases if more assumptions are made about the social welfare function: in particular if it is assumed that not only does a rank-preserving transfer from a richer person to a poorer person reduce inequality, but also that the inequality reducing impact of this is larger the poorer the recipient of the transfer (transfer sensitivity). Dardanoni and Lambert (1988) and Shorrocks and Foster (1987) show that where generalised Lorenz curves cross just once, unanimous orderings of distributions consistent with this revised class of evaluation functions depend on the variances of the distributions. Davies and Hoy (1995) develop results for the case where Lorenz curves intersect potentially several times. As it happens, the key result for the case of a single crossing of Lorenz curves was already in an unpublished paper circulated by Tony in 1973, subsequently published as a 'Rediscovered Classic' in 2008 by the *Journal of Economic Inequality* (Atkinson 2008a).

Poverty Comparisons

Another substantial impact of Atkinson (1970) has been its influence on how researchers think about *poverty* measurement. There is now much cross-fertilisation between approaches to analysing inequality and poverty.

For example, there are now well-developed dominance results concerning poverty as well as inequality and social welfare, and scalar poverty indices are constructed using the approaches that Tony pioneered for inequality measurement. The additional complication in the poverty context is that there is also scope for differences in social values concerning the generosity of the poverty line. Foster and Shorrocks (1988) consider criteria for poverty orderings which are robust to variations in the poverty line. Focusing on the Foster et al. (1984) class of poverty indices, they show that orderings by the first member of this class (the headcount ratio) and for all poverty lines were equivalent to unanimous orderings by symmetric increasing social evaluation functions (Saposnick's first-order welfare dominance result cited earlier). Orderings by the second member of the class and for all poverty lines are shown to be equivalent to generalised Lorenz dominance (Shorrocks's second-order welfare dominance result cited earlier).

Tony took these results further in his Presidential Lecture to the Econometric Society (Atkinson 1987), presenting dominance results for whole classes of distributionally sensitive poverty indices rather than only the Foster et al. (1984) class. The essential insight was that these poverty indices could be seen as special cases of social evaluation functions. In particular, the negative of virtually all distributionally sensitive poverty indices could be written as increasing Schur-concave functions of the incomes of the poor—types of functions for which dominance results were available (see above). Alternative but related devices for checking poverty dominance conditions (Three 'I's of Poverty curves) are developed by Jenkins and Lambert (1997).

The social welfare function approach to the derivation of inequality indices pioneered by Atkinson (1970) has also influenced the development of poverty indices. Sen (1976b) emphasised the Incidence, Intensity, and Inequality features of poverty, and, thereafter, researchers have employed a range of methods to incorporate these features into scalar indices. Several poverty measures incorporate inequality of income among the poor, summarised using Atkinson's 1970 inequality indices rather than the Gini coefficient (used by Sen 1976a). (See, for example, Blackorby and Donaldson 1980; Takayama 1979; Clark et al. 1981.)

Multidimensional Comparisons

Another major development of the Atkinson (1970) results is their extension to comparisons of multidimensional distributions of economic status. This has proved important because of increasing recognition that peoples' material well-being depends not only on their money income (or consumption expenditure), but also on other factors.

The key theoretical results were derived by Atkinson and Bourguignon (1982), who prove a series of dominance results concerning multidimensional comparisons of social welfare in a general setting. The nature of the correlations between the various dimensions is shown to play an important role. The results have been applied to a number of areas.

First, there is the treatment of differences in ‘needs’ in income distribution comparisons. The conventional approach to distributional comparisons (as in Atkinson 1970) supposes that incomes have been adjusted in advance to take account of differences in household size and composition using an equivalence scale. But this approach presupposes social agreement about how much greater the economies of size or ‘needs’ are of larger families compared to smaller families, or of adults compared to children, that is, about equivalence scale relativities across different households. The great insight of Atkinson and Bourguignon (1987) was that one could treat income and needs jointly as a bivariate distribution, and seek dominance conditions for unanimous social welfare rankings with weaker assumptions about equivalence scale relativities than is made using a particular equivalence scale. The checks for dominance are shown to require a sequence of generalised Lorenz curve comparisons. Tony’s 1992 article extended the same idea to comparisons of distributions of poverty and needs. Jenkins and Lambert (1993) relaxed the constraint that the marginal distribution of needs is the same in the two bivariate distributions being compared. (For other developments in this area, see the review by Aaberge and Brandolini (2015: 193).)

Longitudinal income mobility is a second important area of application: the multiple dimensions now refer to incomes at different points in time for these same individuals. As with inequality measurement, there is a large number of ‘statistical’ or ‘descriptive’ indices used to summarise mobility, but their welfare foundations are unclear (Jäntti and Jenkins 2015). Tony provided these foundations with his framework for evaluation together with a set of conditions for checking mobility dominance (Atkinson 1981a).

The third and currently most active area of application is to the development of multidimensional measures of poverty—the multiple dimensions refer to not only income but also deprivations in other domains of people’s life. This interest coincided with widespread recognition that ‘poverty’, especially in developing countries, is not only about low income but also about other factors such as poor health or educational attainment. In the measurement of multidimensional poverty, Tony’s distinctive bridging of different strands of research comes to the fore again. He examines the relationship between, on the one hand, the social welfare approach pioneered by Tsui (2002) and Bourguignon and Chakravarty (2003) and, on the other hand, what he labelled the ‘counting approach’, ‘widely used in applied studies, which concentrate on counting the number of dimensions in which people suffer deprivation’ (Atkinson 2003: 51). The counting approach

was subsequently developed in the influential paper by Alkire and Foster (2011), while Duclos et al. (2006a, b) are two illustrations of the first approach. (For a collection of papers on the topic, see Kakwani and Silber (2008), and for a comprehensive recent survey, see Aaberge and Brandolini (2015).)

5 Atkinson and Economic Inequality: Understanding Contemporary Society

Discussion of Tony Atkinson's influence cannot be confined to his fundamental theoretical research on income distribution analysis. Through many seminal contributions, he has greatly enhanced our understanding of the distribution of labour earnings, income, and wealth in modern societies.

Although this work has constantly been informed by rigorous theoretical modelling—a typical example being provided by Atkinson (2008b, especially Part II)—this line of research is mainly empirical. It is distinguished by scrupulous attention to the quality of data that serves as a model for social science inquiry. Data quality is an aspect of scholarship that is not currently fashionable in mainstream economics. Yet, throughout his work, Tony has painstakingly tried to dig into the details and nuances of the available statistical information, often developing original ways to employ it. His ability to match conceptual theorising with an understanding of statistical production has gained him the respect of professional statisticians, as testified by the 'Atkinson Review' or his appointment to the European Statistical Governance Advisory Board (see below). Tony's effort has provided a salutary warning against unwary use of statistical material, the understanding of which is a precondition for avoiding misleading conclusions, and arriving at robust results that are of value (e.g. Atkinson and Brandolini 2001). A second distinctive feature of this line of work is the constant attention to the international dimension.

Tony has been one of the pioneers, with Piketty and Saez, of the burgeoning literature on long-run trends in the distribution of income summarised in terms of *top income shares* (the share of total income held by the richest 5% or the richest 1%, etc.). The innovation is to use previously unexploited data from income tax administrative records, suitably adjusted, to derive series back to the beginning of the twentieth century. This enhances understanding of trends and cross-national differences in the light of historical trends to a much greater extent than is typically possible in many analyses of economic inequality (income survey data for most OECD countries typically go back only about 40–50 years at most). (See, inter alia, the cross-national studies collected in the volumes edited by Atkinson and Piketty (2007, 2010), and the magisterial review and synthesis by Atkinson et al. (2011) and Alvaredo

et al. (2013).) The research shows that the experience of English-speaking countries differs significantly from those of others. (On this also see Atkinson and Leigh (2013). For Tony's research on top incomes in the UK, see, for example, Atkinson (2005a) and Atkinson and Voitchovsky (2011).) In current research, Tony is extending the top incomes analysis to former colonial countries.

The rise in earnings and income inequality from the mid- to late 1970s onwards that occurred in the USA and the UK is often assumed by many economists to be a universal phenomenon. Per contra, Atkinson (2008b) showed that the rich industrialised countries of the OECD exhibited great heterogeneity in the patterns and timing of changes in earnings and income inequality, echoing the lesson of the top incomes research. This built on Tony's pioneering and comprehensive documentation of *income distribution differences across OECD countries* 15 years earlier (Atkinson et al. 1995). More recently, Tony was one of the first economists to examine the relationship between financial crises and the distribution of income, again taking a cross-national and long-run historical perspective (Atkinson and Morelli 2011).

Tony Atkinson's research has been ahead of its time, anticipating a number of topics that have recently become of substantial contemporary interest, and his contributions remain key reference points. Examples include his work on the *distribution of wealth* which made innovative use of inheritance tax data for the UK (Atkinson and Harrison 1978), and on *earnings mobility* (Atkinson et al. 1992) which reviewed the analytic possibilities provided by longitudinal data on earnings and income which were then just becoming more widely available. Tony's research on *inequality and poverty in Eastern Europe* seized on a topic that became of fundamental importance thereafter (Atkinson and Micklewright 1992).

An even better example of prescience is Tony's work on *intergenerational mobility in income* (Atkinson et al. 1983), based on an innovative follow-up survey in the late 1970s of children of the respondents to a 1950 survey, producing some of the first ever estimates of the degree of inheritance of earnings and income to match estimates of social and occupational class mobility that were more commonly available. These were pioneering steps: 30 years later, and with more data available, estimates of intergenerational income mobility now abound for many countries (see Jäntti and Jenkins 2015). This is also a topic to which Tony has added analytical insights alongside empirical innovation. For example, it is now 'well-known' that summarising a father's income using data for a single year can lead to a downward bias in the estimate of the degree of intergenerational immobility in income. Although the result is commonly attributed to Solon (1992), it appeared a decade earlier in Atkinson (1981b).

6 Atkinson and Public Economics: High Theory Contributing to Concrete Policy Analysis

Tony has greatly contributed to important issues in public finance. His contributions are not only of a theoretical nature, but also his work has been typified by a constant effort to *engage with and inform public policy*, something that he recognises as a fundamental role for economics and economists. Over many years, Tony's research has addressed many different aspects of policy on taxation and the welfare state, and has often led and informed emerging debates. His work has influenced national governments, international organisations, and the policies pursued by supranational institutions, notably the EU.

Some of Tony's early research made fundamental contributions to the *normative theory of public finance*—how best to design taxes to raise a given amount of revenue (Atkinson and Stiglitz 1972, 1976). (See also his definitive graduate textbook, *Lectures on Public Economics*, with Stiglitz (1980, republished 2015).) But the contributions to analysis of policy design for which Tony is probably best known are those about *the welfare state*, and, indeed, this was the subject of his first book (Atkinson 1969a). This research has had an enormous impact, and Tony is recognised as the world-leading scholar in this area.

Tony's work has ranged across *many topics in public economics*, including pensions, unemployment benefits, flat taxes and citizen's incomes, family benefits and means-tested benefits for low-income households, and wealth and wealth taxes. Some of this research is collected in a series of books (Atkinson 1972, 1983, 1989, 1995). Tony was one of the pioneers of tax-benefit microsimulation models that use nationally representative household survey data to calculate tax liabilities and benefit entitlements, and thereby examine the consequences of introducing different combinations of policies (Atkinson and Sutherland 1988). These models are now routinely used in academia and government agencies worldwide. Tony also played an important influential 'godfather' role in the development of the pan-European tax-benefit microsimulation model, EUROMOD, that simulates taxes and benefits for every EU member state (Sutherland 2007).

One of the themes of Tony's research is that welfare states have a potentially positive economic role to play. This is in contrast to most research by economists, which has focused on the potentially adverse effects that welfare states may have on incentives to work and to save. Tony acknowledges these issues, but he also draws attention to the positive roles played by welfare states through income replacement (social insurance) and income maintenance and poverty alleviation. (See, for example, Atkinson and Micklewright 1991;

Atkinson 1999.) The role that political and social institutions can play in countering the impacts of growing inequality, and reducing economic inequalities, is the central topic of Tony's latest book, *Inequality: What Can Be Done?* (2015).

This concern for 'what can be done' has been a constant theme of Tony's: it is precisely what motivated his first book, *Poverty in Britain and the Reform of Social Security* (Atkinson 1969a). Tony has related how, just after Christmas 1965, the *Daily Telegraph* newspaper published an article about Brian Abel-Smith and Peter Townsend's (1965) *The Poor and the Poorest*. He got a copy of their book, and it was one of the few he took with him to MIT in 1967. Tony says that this was the book that inspired him to study poverty. Reading it, he formed the view that economists' analysis did not address sufficiently what to do about it, and he set out to rectify that imbalance in his own work.

Tony has often put his economic understanding into *public service*, both nationally and internationally. For instance, he was a member of the UK's Royal Commission on the Distribution of Income and Wealth 1978–1979, and the Retail Prices Advisory Committee, 1984–1990. He chaired the 'Atkinson Review' on the *Measurement of Government Output and Productivity for the National Accounts* (Atkinson 2005b), widely regarded both as a classic piece of analysis and as providing a model for governments and national and international statistical agencies to follow. He was a member of the Conseil d'Analyse Économique, advising the French Prime Minister, 1997–2001, and of the Commission on the Measurement of Economic Performance and Social Progress created by President Sarkozy of France (chaired by Stiglitz and Sen), 2008–2009. Tony was also a member of the European Statistical Governance Advisory Board, 2009–2012.

Without doubt, Tony has significantly influenced the debate on *measurement of inequality and poverty within the EU*, and he has made major contributions to the development of European thinking about *anti-poverty policy*. Tony was possibly the first scholar to estimate income inequality in the EU as if it was a single country (Atkinson 1996), whereas in *Poverty in Europe*, he observed that an EU-wide poverty line would represent 'a significant move towards viewing the European Union as a social entity' (Atkinson 1998: 29). He then suggested considering a weighted geometric average of national and EU poverty lines as the poverty cut-off, with the weight assigned to each one being a crucial policy parameter. His report with co-authors on social indicators proposed the so-called Laeken Indicators that were adopted by the EU in 2001—a core set of indicators of poverty and social exclusion against which member states monitor their progress towards agreed EU objectives. (See Atkinson et al. 2002, 2006.) His books, co-edited with Eric Marlier (2010, 2016), are directed at improving EU-SILC, the data that are the sources of the EU social indicators.

The *interactions between macroeconomic policy and social inclusion and inequality*, with a particular focus on times of financial crisis and austerity, are the subject of advice to the European Commission, written up in two papers (Atkinson 2013a, b). In the Graz Schumpeter Lectures presented in 2012, Tony turned to demonstrate the application of public economics to analysis of public finance problems in this age of austerity, emphasising that not all austerity packages are the same and there is considerable leeway in their design (Atkinson 2014). More importantly, he restated his lifetime concern that economists often tend to forget the extent to which the conclusions reached from their analysis depends critically on specific modelling assumptions: ‘[E]conomists are too often prisoners within the theoretical walls they have erected and fail to see that important considerations are missing’ (ibid.: 84). He followed the remarks with a long list of the elements missing in contemporary debates about austerity.

Tony’s commitment to the view that a primary role of economics is to illuminate possible solutions of urgent social problems is neatly exemplified by his coordination of a research project, sponsored by the United Nations University World Institute for Development Economics Research and the United Nations (UN) Department of Economic and Social Affairs, aiming to respond to the UN General Assembly’s call for a rigorous study on ways to increase external *finance for developing countries*, particularly to meet the Millennium Development Goals (Atkinson 2004). His subsequent work in this area has advanced both the theory and the empirical analysis of one form of finance—charitable giving of development aid and its links with governments’ official development assistance (Atkinson 2009; Atkinson et al. 2012). This picks up a thread from earlier work on the optimal tax treatment of charitable donations (Atkinson 1976).

7 Conclusions: Tony Atkinson, Economics, and the Economic Analysis of Inequality

Tony Atkinson recently commented about the need for economists in general ‘to be more explicit about the relation between welfare criteria and the objectives of governments, policymakers and individual citizens’ (Atkinson 2011: 157). The importance of recognising and evaluating the consequences of different social judgements is of course one of the main lessons of Tony’s path-breaking 1970 article, and it provided tools for doing so that have been widely used ever since.

Although much of Tony’s research has been in the area of income distribution and economic inequality, he has long emphasised the central importance of integrating the analysis of income distribution into the mainstream of economic

analysis, and he practises what he preaches. This is illustrated inter alia by his deft employment of different explanatory models of the income distribution (e.g. Atkinson 1997, 2008b; Atkinson et al. 2011), his modelling of life-cycle savings earlier in his career (Atkinson 1971a, b), as well as the large body of research on taxes, benefits, and the welfare state reviewed here. Thomas Piketty has also been recently arguing for the importance of ‘placing the study of distribution and of the long-run back at the center of economic thinking’ (Piketty 2015: 68), and he cites Tony as one of the exemplars of this tradition:

[S]ince the mid-20th century, a number of economists, most notably Simon Kuznets and Anthony Atkinson, have been developing the possibility of an approach that blends theory with more data-intensive and historical approaches. This historical data collection project on which my book is based follows directly in the tradition of the pioneering works by Kuznets (1953) and Atkinson and Harrison (1978) (ibid.).

The integrated approach to economics that Tony espouses and practises is of course exactly what he grew up with and was so influenced by at Cambridge. Yet, he was not entirely moulded by the Cambridge atmosphere. As Tony has said:

As a student in Cambridge, England, in the early 1960s, I was taught by a number of people who held strong views, views that they expressed with great fervour and combative style. Perhaps as a reaction, perhaps as a matter of personal temperament, I have always regarded my views about economics—in contrast to my moral principles—as tentative and open to revision as I learned more and acquired more evidence (Atkinson 2014: x).

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