

THE PALGRAVE COMPANION TO LSE ECONOMICS

Edited by Robert A. Cord

LSE

Old Building



The Palgrave Companion to LSE Economics

Robert A. Cord
Editor

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For Helen and David

Introduction

This is a volume about the economics and economists associated with the London School of Economics (LSE). It is the second in a series to be published by Palgrave examining 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 illuminate 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 while at the same time trying to establish external hegemony.

Each volume in the series will consist of two parts. The first will contain a set of chapters which will consider the contributions made by a centre where these contributions are considered to be especially important, and this subjects to a mixture of personal preferences 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

¹Articles are of course another matter.

are the more difficult case. One way forward in these instances is to place an economist in the institution where they 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 has 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? Issues of leadership style, discipleship, loyalty, access to publication outlets and to financing also enter the frame. Finally, there are issues of practicality, including space constraints and unavailability of contributors, among others. 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.

The next volume in the series will examine the University of Oxford.

Robert A. Cord

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Part I

Themes in LSE Economics



1

LSE and Econometrics

Jim Thomas

1 Early Developments

Lionel Robbins, the head of the Economics Department at LSE from 1929 to 1961, was a sceptic when it came to the value of statistical estimation in economics. In his *Nature and Significance of Economic Science* (Robbins 1935: 108–109), he satirised the efforts of ‘the wretched Blank’, who, in 1907–1908, estimated the elasticity of demand for the common herring (*Clupea harengus*) to be 1.3:

Rough computations of this sort are not really very difficult and may have considerable utility for certain purposes. But what reason is there to suppose that he was unearthing a constant law? No doubt the herring meets certain physiological needs which are capable of fairly accurate description, although it is by no means the only food capable of meeting these needs. The demand for herring, however, is not a simple derivative of needs. It is, as it were, a function of a great many apparently independent variables. It is a function of fashion, and by fashion is meant something more than the ephemeral results

I am grateful to Olav Bjerkholt, Sue Donnelly, Charles Goodhart, Vassilis Hajivassiliou, Javier Hidalgo, David Hendry, Sue Howson, Richard Layard, Peter Phillips, Steve Pischke, Peter Robinson, Nigel Rogers and Marcia Schafgans for helpful comments. Since I did not follow all of their suggestions, I remain responsible for all sins of commission and omission.

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of an Eat British Herrings campaign, the demand for herrings might be substantially changed by a change in the theological views of the economic subjects entering the market. It is a function of the availability of other foods. It is a function of the quantity and quality of the population. It is a function of the distribution of income within the community and changes in the volume of money. Transport changes will alter the area of demand for herrings. Discoveries in the art of cooking may change their relative desirability. Is it possible reasonably to suppose that coefficients derived from the observation of a particular herring market at a particular time and place have any *permanent* significance – save as Economic History? (ibid.; italics in original).¹

Robbins's answer was negative and it is perhaps not surprising that there was little interest shown in econometrics by members of the Economics Department at LSE for some considerable time. The early econometric developments at the School came from two members of the Statistics Department: A.L. Bowley and R.G.D. Allen.²

The foundation of the Econometric Society resulted largely from the efforts of Ragnar Frisch over a considerable period of time. In 1926, when he sought support for his plans, one of the four colleagues he contacted was Bowley (see Louçã and Terlica 2011: 59). When the conference to establish the society was held in December 1930, Bowley was one of the ten men elected to serve on its Council (see ibid.: 63). When the first Econometric Society meeting was held in Lausanne in September 1931, Bowley was scheduled to present a paper, though he was forced to cancel it and did not attend the meeting (see Bjerkholt 2015a: 1160). However, Bowley was responsible for organising some of the early European meetings of the society. A further link came from Bowley's collaboration with Allen in a study of family expenditure, in which the Preface states that of the three converging sources of the investigation, 'One is an article in *Econometrica*, 1935, "The Action of Economic Forces in Producing Frequency Distribution, etc." followed by an unpublished communication to the Econometric Society in 1934 on "The Variation of Expenditure"' (Allen and Bowley 1935: v).

Allen's links with the Econometric Society were also developing: in 1934, he published two articles in *Econometrica* (Allen 1934a, b), and in 1935, he was elected a Fellow of the society. Things progressed further and in 1937,

¹The extent of Robbins's knowledge of statistical theory is discussed in Thomas (2009: 411–412), where the evidence suggests that he did not take the theoretical section of the compulsory course in statistics which he should have attended as an undergraduate when studying at LSE.

²See Chapter 8 in this volume for an evaluation of Bowley and Chapter 20 for an assessment of Allen.

when the Cowles Commission was looking for a new Director of Research, Allen was proposed as one of the candidates:

The names considered during or after the [1937 Cowes Commission Research] conference were in addition to Tinbergen, Marschak and Yntema, also R.G.D. Allen, Oskar N. Anderson, Ernest H. Phelps Brown, and Costantino Bresciani-Turroni; other names may also have been mentioned ... It can be noted there seemed to be a premium on Europeans for the position as CC research director (Bjerkholt 2013, 2015b: 15).

Furthermore,

Frisch held...Allen in very high regard. In October he informed Cowles that Allen had got a one-year Rockefeller fellowship and advised Cowles to get in touch with Allen and persuade him to spend time at the Commission, adding “the more I have thought the matter over the more I have come to the conclusion that Allen is the man to consider as a candidate for directorship taken everything into account”.

Frisch’s overall record for later years shows perhaps that he did not always hit the mark in personal assessments but he held sound opinions in 1937–1938 ... He tentatively concluded to Cowles that he placed Allen, Marschak, and Tinbergen above the others. He also threw in the name of Phelps Brown but the proposal was not pursued (ibid.: 16).

In July 1938, Allen and his wife attended the Cowles Commission Annual Research Conference on Economics and Statistics in Colorado Springs, where Allen presented two papers, the first entitled ‘Some Statistical Measures of Labour Mobility in England’ and the second ‘Patterns of Family Expenditure: The Effect of Social Level and Family Composition’. Among those present were Harold Davis, Elmer Working, Abraham Wald, Gerhard Tintner, Henry Schultz and Alfred Cowles.³

Meanwhile, in the LSE *Calendar 1935/1936*, Allen offered a course of 15 lectures under the title ‘Some Problems in Econometrics’. The details were:

SYLLABUS—The first part of the course consists of an account of the main statistical methods used in the description and analysis of economic phenomena. The treatment is largely non-mathematical, and the essential mathematical notions are put as simply as possible.

³See Cowles Commission for Research in Economics (1939). I am grateful to Sue Howson and Olav Bjerkholt for bringing Allen’s connections with the Cowles Commission to my attention.

The second part is concerned with some particular problems in econometrics, with the testing of theoretical constructions and the evaluation of fundamental economic concepts. The topics considered include the deduction of elasticities of demand and supply from market data, the analysis of family budget collections and the measurement of the cost of living (LSE 1935: 112).

In 1936–1937, the same course was on offer, but now consisting of ten lectures, whereas it was back to 15 lectures in 1937–1938. In 1938–1939, it was listed, but now as ten lectures to be given in 1939–1940. Finally, in 1939–1940, it had become nine lectures to be given in 1940–1941 and there was a note that it was to be given in alternate years. As the war took Allen off to official duties elsewhere, the course disappeared. However, the *Calendar 1939/1940* listed a new course to be taught by Allen:

The Econometric Approach to Business Cycle Problems, consisting of nine lectures. The details were:

SYLLABUS—The course will be concerned with an exposition of recent work by Tinbergen, Frisch and others on econometric business cycle research. The emphasis will be laid as much on the statistical methods used and the nature of the ‘dynamic’ economic relations involved as on the conclusions reached in the testing of theories of cyclical fluctuations.

BOOKS RECOMMENDED—Tinbergen, *Econometric Approach to Business Cycle Problems*, ‘Einige Grundfragen der mathematischen Konjunkturtheorie’ (*Archiv für mathematische Wirtschafts- und Socialforschung*, 1937), ‘On the Theory of Business Cycle Control’ (*Econometrica*, 1938), *A Statistical Testing of Business Cycle Theories, Business Cycles in the U.S.A., 1919–1937*; Frisch, ‘Propagation Problems and Impulse Problems in Dynamic Economics’ (*Economic Essays in Honour of Gustav Cassel*) (Given in alternate years.)

The lectures were scheduled for October 1939.⁴

At the onset of the Second World War, LSE was evacuated to be accommodated at Peterhouse College, Cambridge, and many academics, including, as noted, Allen, left to undertake war service.⁵

⁴Despite the close links reported above between the statisticians at the Cowles Commission and the Econometric Society, it seems that the first LSE academic to publish an article in *Econometrica* with the word ‘econometric’ in the title was an economist, Victor Edelberg (Edelberg 1936). A second article with ‘econometric’ in the title was Edelberg (1940). Edelberg was appointed as an Assistant (what would today be called a Teaching Assistant) in the Economics Department in 1935, but in the early 1940s he had a severe mental breakdown from which he never really recovered (see Howson 2011: 254 and Thomas, forthcoming).

⁵The outflow of academics from both institutions meant that teaching duties fell to a relatively small number of teachers, and as a result, students from LSE and Cambridge shared courses. According to the *Calendar 1942/1943*, one of the shared courses offered in the summer term by Rothbarth was a course of ten lectures under the title ‘Introduction to Econometrics’. The course was offered for a second time

Allen returned to LSE after the war, and in the 1946–1947 session, he taught a ten-lecture course on ‘Problems of Econometrics’ that was essentially his 1939–1940 business cycle course with the addition of Leontief’s input–output analysis. The course was repeated in the 1947–1948 session, but then discontinued and econometrics disappeared from the LSE syllabus for a number of years.⁶

In the 1951–1952 session, a 10-lecture course appeared taught by Geoffrey Penrice of the Statistics Department and entitled ‘Introduction to Econometrics’. This course was provided as an option for students specialising in Statistics in the BSc(Econ), with the following syllabus: ‘Scope of Econometrics. Derivation of Supply and Demand curves by regression analysis and simultaneous probability equations. Production and Consumption functions. Problems of identification and aggregation. Connection between micro-economic theory and macro-economic models. Problems of obtaining suitable statistical data’ (LSE 1951: 353).

By the following session, the course had expanded to 20 lectures and was being taught by Mr. Booker and Dr. Morton.⁷ The following session, it expanded again to 24 lectures and Booker and Morton were joined by W.J. Corlett, an econometrician from University College London (UCL). The course now settled down and continued in this format until the 1959–1960 session, though with game theory and linear programming appearing in the syllabus.⁸ The following year, the teachers were listed as ‘Mr. Corlett and others’ and the syllabus reverted to its original 1951 format, with game theory and linear programming being dropped.⁹

in the summer term of the following session (LSE 1942, 1943). Erwin Rothbarth came to LSE with one of the first bursaries for students who had been displaced from Germany, graduated in 1936 and carried out research at the school until 1938, when he moved to Cambridge as a research assistant to Keynes working on national income statistics. He returned to Cambridge from internment to teach but volunteered for the British Army and was killed in Holland in November 1944. So far, I have not been able to obtain any information on what was taught on the Rothbarth course. However, perhaps one may speculate from the fact that he reviewed the second volume of Tinbergen’s *League of Nations study* (Rothbarth 1941) and, in an obituary of Rothbarth, the authors wrote: ‘But his most striking achievement was perhaps that he made himself a master of the new mathematical technique of writers of econometrics, such as Slutsky, Frisch, Koopmans and Tinbergen, and adapted their methods for his own work’ (Champernowne and Kaldor 1945: 131). See also Cuyvers (1983, 1983–1984) and Toporowski (2013: 122–124).

⁶For an account of Allen’s important contributions to economic theory, see Chapter 20 in this volume.

⁷In 1952, Penrice left LSE for a distinguished career in the UK Civil Service, the OECD and the IMF.

⁸Dr. George Morton was a mathematician who taught game theory and linear programming. It is interesting to note that the proportion of the reading list devoted to those two topics expanded during the period when he was involved in teaching the course.

⁹See Gilbert (1989) for a further discussion of the teaching of econometrics during this period.

Another important contribution from the Statistics Department was made under the direction of Professor Maurice Kendal who was interested in the analysis of time series and encouraged James Durbin to come to LSE.¹⁰ Durbin provided a number of courses that were important to the development of time series econometrics.

2 The Contribution of A.W.H. ‘Bill’ Phillips

Bill Phillips played an important role in getting the Economics Department to focus on the need to increase the mathematical and statistical content in its provision of teaching and research. Coming from an engineering background, he also brought a different approach to the interpretation of macroeconomic models of the economy, by seeing the need to treat them as dynamic systems, rather than analysing them through the conventional comparative statistics. The Phillips Machine provided a dynamic simulation of a macroeconomy that could trace out over time the actual paths of adjustment of key variables to changes in parameters, such as changes in interest rates, the money supply or government expenditure.¹¹

Phillips saw that the feedback mechanisms that engineers used to control dynamic systems (proportional control—the action taken is proportional in magnitude and opposite in sign to the error to be corrected; integral control—the action taken is proportional in magnitude and opposite in sign to the cumulated error up to that time; and derivative control—the action taken is proportional in magnitude and opposite in sign to the rate of change of the variable to be controlled) had potential similarities to the economic policies used by the government and/or other agencies in an attempt to control the economy. In two theoretical articles (Phillips 1954, 1957), he explored the theory of optimal control in dynamic models. The innovations

¹⁰James Durbin and Denis Sargan were both undergraduates at St. John’s College, Cambridge, in the 1940s. Durbin was invited back to Cambridge in 1948 by Richard Stone to work in the Department of Applied Economics. At the time, there was a good deal of research going on there on time series problems, with Guy Orcutt and Donald Cochrane working on their test and transformation for dealing with first-order (AR1) autocorrelated errors. Durbin worked on the problem with Geoffrey Watson, and this led to the development of the Durbin–Watson test. His interest in time series problems led to Durbin being appointed as an Assistant Lecturer in the Statistics Department at LSE and his involvement with econometrics teaching and research. For a full account of Durbin’s work at LSE, see Chapter 25 in this volume.

¹¹See Chapter 23 for more information on Phillips and the Phillips Machine. For an excellent biography of Phillips and a non-technical account of his research and importance, see Bollard (2016). See also Phillips (2000).

were: (i) that policy should not be thought of in a static mode but rather in a dynamic mode; (ii) policy is best thought of in terms of rules; (iii) it is very hard to assess the interaction of policy and system dynamics; and (iv) some useful observations about the nature of policy were presented as a result of the simulations performed (see Pagan 2000: 130–131).

At the time, most econometricians who were interested in macroeconomic modelling followed the Cowles Commission's methodology and were estimating the parameters in a set of simultaneous equations using data collected discretely. Phillips's approach faced a new problem: Theoretical models were formulated in continuous time, but the data were collected at discrete points of time. This problem was addressed in a number of publications (see Phillips 1956, 1959; Phillips and Quenouille 1960), with this work having important implications for later econometric developments at the School.¹²

In addition to the gradual increase in the teaching of econometric theory and mathematical economics at LSE, there was one interesting development that took place with respect to applied econometrics in the form of the establishment of a seminar to formulate models and submit them to statistical testing.

3 The Methodology, Measurement and Testing (M²T) Seminar

The early development of applied econometrics at LSE resulted from a negative reaction by a number of the younger members of the Economics Department to the arguments put forward in Robbins's *Nature and Significance*. The reaction has been well summarised by the leader of the group, Richard Lipsey (2009: 845).¹³

The first stage in the development of the group was methodological. Group members spent time in discussions with Dr. Joseph Agassi, a junior member of the Philosophy Department, concerning Karl Popper's philosophy of science (see De Marchi 1988). The methodology that emerged from these discussions was, in simple terms, that, to be taken seriously, models

¹²While continuous time modelling was largely ignored elsewhere, Rex Bergstrom was influenced by Phillips in his work in this area and both Peter Phillips and Clifford Wymer worked on continuous time modelling as PhD students at LSE (see Mizon 1995).

¹³Other members of the group were Chris Archibald, Bernard Corry, Kurt Klappholz, Kelvin Lancaster, Maurice Peston and (later) Max Steuer.

should make testable predictions and that econometrics should be used to test these predictions. This methodology led on to the two other components of the Seminar's title: the need for relevant economic data to enable testing (measurement) and then the testing itself. The group's methodology was basic not only in their own research but also in their reaction to the research of other economists: a number of visitors to the Seminar who came to present a model based on 'plausible' assumptions were nonplussed on being given an M²T grilling and being told that the model being discussed did not seem to predict anything that could be tested.

Not all members of the group were concerned with all three of these components. For example, Kurt Klappholz was mainly interested in methodology (see Klappholz and Agassi 1959; Klappholz and Mishan 1962), while Chris Archibald concentrated his research on investigating whether a selection of mainstream models actually yielded testable predictions (see Archibald 1960, 1961). Others carried out empirical analyses, notable examples being Dick Lipsey's re-estimation of the Phillips curve (Lipsey 1960, 2000) and Lipsey and Max Steuer's attempt to test a model of inflation proposed by Kaldor (see Kaldor 1959; Lipsey and Steuer 1961).¹⁴

The M²T Seminar had a gradual impact on the teaching of applied economics: whereas in the 1950s courses in applied economics had involved plotting economic data against time and telling plausible stories about their movements, courses now began to present the results of empirical testing and discuss statistical significance and goodness of fit.

In its purest form, the M²T Seminar began to change in the mid-1960s with the departure from LSE of a number of the original members: Archibald and Lipsey moved to the new University of Essex, Bernard Corry and Maurice Peston went to Queen Mary College to meet an expansion in the teaching of economics within the University of London, and Kelvin Lancaster was a visitor at Columbia University in New York before deciding to stay there.¹⁵ The Seminar continued as a general meeting for the presentation of empirical research for a number of years under the chairmanship of Max Steuer, but lost its relevance and was discontinued after the development of alternative seminars within the specialised branches of economics.

¹⁴Kelvin Lancaster was an economic theorist who explored the possibility of making testable predictions in qualitative economics (see Lancaster 1962), while Bernard Corry published empirical studies of the labour market (see Corry 1961).

¹⁵The departure of this group of younger members of the staff led to a sense of disappointment on the part of some of the MSc students and was reflected satirically in one of a number of similar items performed in a cabaret at one of the Staff-Student Weekend Schools held in the 1960s: 'Where have all the Great Men gone?', was the lament, with Robbins being blamed for driving them away.

4 The Coming of the Econometricians

The 1960s saw dramatic changes in the composition of the Economics Department. Robbins's attempt to become Chairman of the *Financial Times* while continuing as an academic was rejected, and this, together with his appointment to chair the Commission on Higher Education, led to his retirement from LSE in 1961. A number of the long-serving senior members of the Department also retired in the years that followed (dates in parentheses show the period the professor spent in the Economics Department): Sir Arnold Plant (1930–1965), Frank Paish (1932–1965), Sir Henry Phelps Brown (1947–1968) and Richard Sayers (1947–1968).¹⁶

Following Robbins's retirement, there was a short period of confusion during which it was unclear who the head of the department was,¹⁷ but the situation was swiftly clarified by a reorganisation of the School's administration that led to the appointment of departmental Convenors. Convenors were to be heads of departments, but on a rotating three-year basis rather than for indefinite periods. The first Convenor of the Economics Department was Ely Devons, and, having consulted widely among the staff in the Department, he set out to attract new staff and was remarkably successful, so that by the late 1960s the Department had been joined by Rex Bergstrom,¹⁸ Denis Sargan,¹⁹ Harry Johnson, Frank Hahn and Terence

¹⁶See Backhouse (1997) for a discussion of changes that took place at LSE in the context of developments at other UK universities. He notes (p. 44) that Birmingham University introduced an MSocSc degree in 1952, which predated the developments at LSE. The driving forces at Birmingham were Terence Gorman (there from 1949 to 1962), Frank Hahn (1948–1960) and Alan Walters (1952–1968). They had already transformed undergraduate teaching at Birmingham, with courses in 'Mathematical Economics and Econometrics' and a compulsory individual 'Quantitative Economics Project' for third-year students. All three later moved to LSE.

¹⁷My irreverent comment at the time was that it reminded me of the confusion in the Politburo after the death of Stalin.

¹⁸Rex Bergstrom was an econometrician from New Zealand who came to LSE as a Reader in Economics in 1962. He returned to Auckland University in 1964 as Professor of Economics, and, when he returned to the UK in 1970, it was to the University of Essex, where he remained until 2005. While at LSE he began his research on continuous time econometrics, but as most of his research in this area was carried out after he left the School, it will not figure in this discussion of LSE's contribution to econometrics. See Phillips (1988a, 2010) for a full discussion of Bergstrom's research and publications.

¹⁹Durbin played an important part in these appointments. In his *ET* Interview, he states that: 'Bill Phillips and I cooperated in getting two new posts at the Readership level at the school: one in the economics department and one in the statistics department. Rex Bergstrom took the post in the economics department for a time and we persuaded Denis Sargan to come from Leeds to the post in the statistics department. Soon afterwards Bergstrom left and Denis migrated to the economics department as a Professor of Econometrics' (see Phillips 1988b: 135). For an evaluation of the influence of Bill Phillips on econometrics, see Hendry and Mizon (2000).

Gorman.²⁰ Sargan, who arrived at LSE in 1963, summarised the importance of these appointments:

In the econometrics field, Gorman and Hahn were unique in moving to LSE from Oxford and Cambridge, where they were already well established. Gorman was already professor at Nuffield College, Oxford, in 1966, and Hahn a leading member of Churchill College, Cambridge. They were friends of long standing; they, Alan Walters and John Wise were sometimes referred to as the “Birmingham mafia” as they had all been teachers or students at Birmingham in the early 1950s. Undoubtedly, Hahn and Gorman only moved to LSE to be reunited together in the hope of founding a pioneering school of econometrics and mathematical economics, at a time when both Oxford and Cambridge were uninterested in these fields. They, together with Bill Phillips and myself, provided the critical mass sufficient to attract good students and faculty from all over the world to take and teach our courses, particularly in our new MSc in Econometrics (Sargan 2003: 433).

Further impetus for change came through changes in degrees at both the undergraduate and graduate levels. In the process of revising the BSc(Econ) degree, which had provided a limited number of courses in mathematical economics and econometrics, a new degree, the BSc(Econometrics and Mathematical Economics), was introduced that offered much more specialisation in those subjects. At the level of the MSc, LSE introduced new one-year taught degrees in 1964, an MSc(Econ) and an MSc(Econometrics and Mathematical Economics). There were conversion courses to enable undergraduates in other disciplines to enter these new degrees, with the result that there was soon a flow of students from science backgrounds with advanced mathematics coming through into the MSc(Econometrics and Mathematical Economics). The final change of interest was in the structure of the PhD, where the Economics Department first persuaded the rest of the School and then the University of London that, as an alternative to the traditional doctorate in which the degree was awarded on the basis of a single magnum opus of 75,000 words, it was possible to submit a number of shorter (and not necessarily connected) pieces of work that demonstrated the required level of originality. These changes produced a flow of outstanding

²⁰Gorman made an important contribution to the development of the BSc(Econometrics and Mathematical Economics) degree by arguing for the inclusion of a compulsory individual ‘Quantitative Economics Project’ for third-year students, as at Birmingham. He insisted on ‘Quantitative’ rather than ‘Econometric’, this allowing for a much wider range of statistical techniques to be used than simply conventional econometric methods.

PhD students in econometrics in the late 1960s. The excitement of the period is described by David Hendry, the leading member of this group of Sargan's students:

The student rebellion at LSE was at its height in 1968–1969, and most of Denis's students worked on the computer at UCL, an ocean of calm. It was a wonderful group to be with. Grayham Mizon wrote code for optimization applied to investment equations, Pravin Trivedi for efficient Monte Carlo methods and modelling inventories, Mike Feiner for “ratchet” models for imports, and Ross Williams for nonlinear estimation of durables expenditure. Also, Cliff Wymer was working on continuous-time simultaneous systems, Ray Byron on systems of demand equations, and William Mikhail on finite-sample approximation. We shared ideas and code, and Denis met with us regularly in a workshop where each student presented his or her research. Most theses involved econometric theory, computing, an empirical application, and perhaps a simulation study (Hendry 2003: 750–751).²¹

5 Sargan, Hendry and the LSE Tradition in Econometrics²²

To understand the importance of the econometric methodology developed at LSE, it is helpful to remember the poor quality of many of the applied econometric studies undertaken in the 1960s. Most of the models being investigated were single equations, to be estimated from time series data. There were some data constraints imposed by short time series, but more important were the severe computational constraints of a pre-computer era in which computation involved mastery of the Doolittle method of matrix inversion using desktop electric calculating machines.²³ A typical article might involve Section 1: the discussion of an economic theory that leads to Eq. (1):

$$Y_t = \beta_0 + \beta_1 X_{1t} \quad (1)$$

with the prediction that $0 < \beta_1 < 1$. Section 2 involves the testing of the theory, so an error term is added to (1) to yield (2) and it is assumed that the

²¹The recollections of another LSE econometrics student from this time are presented in Spanos (2014).

²²For technical details, see Chapter 27 below, Gilbert (1986) and Hendry (2009). See also Hendry (1980), Phillips (1985), Pagan (1987), and Hansen (1996).

²³During the 1950s and early 1960s, LSE economists were fortunate to have available the skills of June Wickens in this capacity, but when she married and moved to Bristol there was a considerable fall in the productivity of the Economics Department.

u_t is independently and identically normally distributed with a mean of zero and a constant variance:

$$Y_t = \beta_0 + \beta_1 X_{1t} + u_t \quad (2)$$

Some time series data are now introduced with a perfunctory discussion of their construction and relevance and (2) is estimated by ordinary least squares.

The main emphasis in evaluating the results was on the goodness of fit (high value of R^2), the correct signs and the statistical significance of the estimated coefficients. It was also a requirement that the researcher checked the Durbin–Watson (DW) statistic to test for the presence of first-order (AR1) autocorrelated errors. If the test suggested the presence of AR1 errors, then the Cochrane–Orcutt transformation would sometimes deal with the problem, but in those cases where this was not possible, there was no available alternative methodology to provide a solution. What tended to happen was that the researcher would add more variables to the equation in the hope that with enough ‘data mining’ an acceptable result might emerge that got rid of the autocorrelation problem:

$$Y_t = \beta_0 + \beta_1 X_{1t} + \beta_2 X_{2t} + \dots + \beta_k X_{kt} + u_t \quad (3)$$

A non-technical description of the LSE econometric methodology is as follows:

- (1) In contrast to the implicit assumption in Eqs. (1) and (2) that the economic processes adjust rapidly and are complete within a single time period, the LSE alternative assumed that adjustment processes might be slow and lagged values of the variables (both dependent and independent) might be necessary. In addition, autocorrelation was seen not as a purely statistical problem but rather the effect of misspecification when lagged values of the variables were omitted.
- (2) A second important difference is a respect for the economic time series being used: rather than merely providing data for a ‘test’ of a theory, the series contain important information on the process of economic adjustment. Hence, the objective is to develop a model that could have generated the data being observed.²⁴

²⁴This is the idea underlying the so-called data generating process (DGP). See Gilbert (1986) for the technical details.

- (3) A third difference is the assumption that while an economic model such as that in Eq. (2) is too simple to explain the data being analysed, the variables in the model may be useful in explaining the data, but they will be used in a more general, dynamic model. For example:

$$Y_t = \beta_0 + \beta_1 X_{1t} + \beta_{11} X_{1(t-1)} + \beta_2 X_{2t} + \beta_{21} X_{2(t-1)} + \gamma Y_{(t-1)} + u_t \quad (4)$$

The search for a suitable model involves the use of a battery of tests, including tests for the form of the model (linear versus log-linear), normality, heteroscedasticity, autocorrelation and the stability of the model outside the period used in the fitting.²⁵

- (4) One advantage of starting with a general model that could have generated the data is that it is then possible to simplify the model by imposing restrictions on the parameters (e.g. testing for zero values of the parameters of variables that are statistically insignificant and looking for possible common factors among the parameters) and test the significance of the restrictions in a systematic way, as the simplified models are *nested* in the general model.²⁶

Instead of ignoring the models of other economists that attempt to explain a particular economic phenomenon, the objective is to *encompass* them: ‘(W)e consider it an essential (if minimal) requirement that any new model should be related to existing “explanations” in a constructive research strategy such that previous models are only supplanted if new proposals account (so far as possible) for previously understood results, and also explain some new phenomena’ (Davidson et al. 1978: 662).²⁷

²⁵To some extent, these developments were helped by the enormous increase in computing capacity and the development of econometric software packages that greatly simplified the testing process. The improved computing facilities encouraged the use of Monte Carlo methods to study the properties of alternative estimators and showed that desirable asymptotic properties were often present in relatively small samples (see, for example, Hendry 1973).

²⁶This procedure has been labelled the ‘general to specific’ process and is in marked contrast to the ‘specific to general’ process involved in going from Eqs. (1) or (2) to Eq. (3) above. For an interesting example of this process, see Hendry and Mizon (1978), which presents a critique of a demand for money model developed at the Bank of England.

²⁷The Davidson et al. (1978) article illustrates the process of encompassing by reconciling a number of apparently very different consumption functions.

While most of Sargan's students mentioned in the Hendry quotation at the end of Section 4 above left LSE after obtaining their PhDs to spread the message at other institutions, Hendry himself stayed on and advanced rapidly from lecturer in 1969 to Reader in 1973 and Professor in 1977. Many of the basic ideas summarised in this section arose as a result of the work of Phillips and Sargan, but Hendry made major contributions both in developing theory and in presenting practical applications which illustrated the various strengths of the methodology. Without his efforts, the impact of the methodology would certainly have been less profound.²⁸

Perhaps it is appropriate that Aris Spanos, who was a student at LSE, completing a BSc, MSc and PhD between 1973 and 1982, should sum up the balance (Spanos 2014: 371–372):

Denis Sargan is undoubtedly the “father” of the LSE tradition, but the protagonist who brought out the revolutionary nature of the LSE perspective and unflaggingly endeavoured to change empirical modelling in economics was David Hendry. Their different personalities complemented each other in a way that contributed significantly to the success of that tradition. Sargan was a reluctant revolutionary because he saw himself as pursuing the agenda set out by the Cowles Commission in the early 1950s. He was a lot more comfortable discussing instrumental variables, Edgeworth expansions and Gram-Charlier approximations than methodological issues pertaining to empirical modelling. In contrast, Hendry relished the opportunity to compare different approaches to modelling and break new ground by introducing alternative inference procedures and modelling strategies that improve learning from data.

The Sargan–Hendry era came to an end in the 1980s with Hendry's move from LSE to Nuffield College, Oxford, in 1982 and the retirement of Sargan in 1984.²⁹

²⁸See Ericsson (2004) for an evaluation of Hendry's work and a detailed bibliography.

²⁹Hendry continued to publish research after his move to Oxford, but this work lies outside the scope of this chapter. For a full evaluation of his contribution to econometrics, the reader must await the relevant chapter in the next volume in this Palgrave series, *The Palgrave Companion to Oxford Economics*.

6 Peter Robinson and the Econometric Programme Based in STICERD³⁰

There were further developments in econometric theory after Peter Robinson was approached to see if he would be interested in the Chair in Econometrics, this resulting in him returning to LSE.³¹ One partial result has been the ‘Econometrics Programme’ based in STICERD, which currently focuses on areas such as long memory time series, nonparametric and semiparametric methods, Edgeworth approximations, adaptive learning, diffusion, bootstrapping, simulation methods, sample selection, identification and spatial econometrics.³²

The development of the programme has reflected evolving research interests and changes in the availability of data. Over time, researchers interested in microeconomic problems worked with cross-sectional data, such as information on families, firms or the labour market gathered through large-scale surveys. In addition, there were data obtained from panels of individuals or firms to provide time series of cross-sectional data. While the national income data required for macroeconomic econometrics continues to grow slowly, financial markets generate vast quantities of time series data that are available for econometric analysis. In some applications, the relationships between the relevant variables require new techniques of analysis.³³

The earlier interest in cointegration and unit roots has already been discussed. However, in some cases, the autoregressive damping in the time series being analysed was very gradual and this led to an interest in long memory time series (see Robinson 2003) and fractional cointegration.³⁴

³⁰STICERD is an acronym for the Suntory and Toyota International Centres for Economics and Related Disciplines. See <http://www.sticerd.lse.ac.uk> for further details.

³¹Robinson was an undergraduate at UCL, where he graduated with a BSc in Statistics in 1968. He came to LSE, completed an MSc in Statistics in 1969 and was a Lecturer in the Statistics Department in 1969–1970. He left LSE for Australia and completed his PhD at the Australian National University in 1973. He then moved to Harvard University, where he held a joint position in both the Economics and Statistics Departments and taught there until 1979, which time included a year at Berkeley. After spending 1979 at the University of British Columbia, he returned to the UK as a Professor in the Department of Mathematics at the University of Surrey in 1980 and taught there until 1984, when he moved back to LSE as Professor of Econometrics. See Delgado and Hidalgo (2011).

³²While STICERD generally provides financial support for seminars and workshops, funding for research and IT is provided from elsewhere.

³³A convenient summary of developments in econometrics that covers much of the recent period is Geweke et al. (2008).

³⁴Marinucci and Robinson (2001) illustrated their fractional cointegration analysis using quarterly time series data on consumption and income from Q1 1947 to Q2 1981 taken from Engle and Granger (1987) and annual data on stock prices and dividends from 1871 to 1986 from Campbell and Shiller (1987).

The availability of data sets that are often very large provides sufficient information for the researcher to take a more flexible approach to specifying the relationship between variables: instead of specifying a particular parametric model (such as Eq. 3 above), it is possible to specify a nonparametric model, such as

$$Y_t = m(X_t) + u_t \quad (5)$$

and to determine the form of the relationship by smoothing the data. Extending a nonparametric model to the multivariate case requires even larger data sets, and this need can be reduced if it is possible to specify a semiparametric model that involves some parametric restrictions on the data, for example

$$Y_t = m_1(X_{1t}) + m_2(X_{2t}) + u_t \quad (6)$$

where $m_1()$ specifies a parametric relation between Y_t and X_{1t} and $m_2()$ is nonparametric.³⁵

The absence of distributional assumptions concerning the parameters rules out conventional significance testing, but it is possible to treat the large data set as a population and derive its properties from the data. It is also possible to justify the conventional normal approximation to the distribution of the parameters and improve it. (On Edgeworth approximations, see Nishiyama and Robinson 2000; Linton 2000.)

Information on the moments of the estimated distribution may be obtained by drawing repeated samples from the data with replacement ('bootstrapping') (see Nishiyama and Robinson 2005; Camponovo and Otsu 2014; and Otsu and Rai 2015). Faced with a wide range of data sources, the question of sample selection arises (see Schafgans 1997; Schafgans and Zinde-Walsh 2000; and Schafgans and Stelcner 2006). There has been an interest in spatial econometrics (see Robinson 2010a; Robinson and Thawornkaiwong 2010; and Delgado and Robinson 2013) and in particular in dynamic panel data, where the combination of time series and cross sections pose some interesting problems: see Robinson (2007, 2010a, b), Robinson and Velasco (2013), and Robinson and Rossi (2013a, b).

In many ways, the Econometrics Programme at STICERD may be seen as a natural extension of earlier econometric work at LSE while reflecting

³⁵Plotting y against x and smoothing the data using a moving average would be a simple, though not very efficient, example of nonparametric modelling. For the technical details, see Henderson and Parmeter (2015). See also Linton et al. (2004), Komarova (2012), and Adusumilli and Otsu (2015).

the changing background of research interests. For example, while the LSE tradition had concentrated on the data rather than estimating simple, static economic models, it worked with linear or log-linear models and relatively short time series, mainly macroeconomic data. There was an interest in cointegration, unit roots and differencing to achieve stationarity. The econometricians at STICERD were more focused on theoretical issues and did not have the proselytising methodological zeal of the earlier LSE researchers in econometrics.

7 Applied Econometrics and the Growth of Research Institutions

In contrast to the early econometricians at LSE who generally worked on their own or with only a few collaborators, in recent years there has been a growth in centres in which economists carry out their work within specific research programmes. The research is still individual, but now focused on aspects of a wider research programme.

7.1 The Centre for Economic Performance (CEP)

In 1964, Claus Moser, together with Richard Layard, who had been his assistant working on the Robbins Enquiry (Committee on Higher Education 1963), set up a Higher Education Research Unit at LSE. It became the Centre for the Economics of Education (CEE) in 1974 and was funded by the Esmée Fairbairn Foundation. In 1980, with funding from the Economic and Social Research Council (ESRC), the Centre became the Centre for Labour Economics (CLE), with the involvement of Richard Jackman, Richard Layard, David Metcalf, Stephen Nickell and Christopher Pissarides. Its area of research expanded in 1990, and it became the CEP.

An early focus for quantitative research in the CEE was on the links between inequality and unemployment and evidence compiled by the Centre was submitted to the Royal Commission on the Distribution of Income and Wealth, chaired by Lord Diamond (Diamond Commission 1975a, b; 1976a, b).

At the CLE, the early emphasis was on the duration of unemployment (see Nickell 1979a, b), and in the 1980s, there was a concentration on the aggregate rate of unemployment and work on the NAIRU (non-accelerating

inflation rate of unemployment) and the Phillips curve.³⁶ Another area of research was on the interaction of rates of unemployment and vacancy rates. Much of the research of this early period is presented in Layard et al. (1991, 2005).

Since the transition from the CLE to the CEP, the high rate of productivity of output has continued and the number of CEP Discussion Papers had reached over 1500 by October 2017, with considerable numbers of other types of publications. As a result, the small selection of items quoted here represents a tiny proportion of that output and are selected for their econometric content.³⁷ (See Ahlfeldt et al. (2014), Ahmadi and Ritschl (2010), Arellano and Bover (1990), Arellano et al. (1990), Burgess (1991), Coles and Petrongolo (2003), Corrado and Fingleton (2011), Gibbons and Overman (2010), Gibbons et al. (2014), Hajivassiliou and Ioannides (1995), Harvey and Scott (1994), Hensen and McMahan (2013), Jarociński and Marcet (2011), Manning (1993), Manning (2004), Redding and Rossi-Hansberg (2016), Santos Silva and Tenreyro (2009a, b), and Yashiv (2007).)

7.2 The Financial Markets Group (FMG)

The Financial Markets Group (FMG) was founded at LSE in 1987 with support from a number of financial institutions to carry out research in that area. The interests of LSE were represented by Charles Goodhart and Mervyn King, with David Webb having a major role in its organisation from 1991 to 2012. Initially, the FMG was linked to the Department of Accounting and Finance, but with increasing specialisation there was a separation into the Department of Accounting and the Department of Finance, with which the FMG is now associated.

Empirical research into the operation of financial markets is blessed with the availability of long time series, and it is therefore not surprising to find many of the theoretical techniques investigated in Peter Robinson's Econometric Programme being applied by researchers in the FMG. For example, there are a number of studies involving nonparametric and semiparametric estimation (see Altissimo and Mele 2005; Conner and Linton 2000; Linton and Shintani 2001; and Sentana and Wadhvani 1989), kernel smoothing (see Linton et al. 2004) and the bootstrap (see Sullivan et al. 1998). Other econometric studies

³⁶This continued following the transition from CLE to CEP (see Manning 1992; Ellison and Scott 1998; and Haldane and Quah 2000). See also Nickell (1982, 1985a, b).

³⁷In 2008 a Spatial Econometrics Research Centre (SERC) was set up and was incorporated into the CEP in September 2015. Its discussion papers are listed on the CEP website as 'SERCDPs'.

include Chan (2009), Danielsson (1997), Goodhart and Giugale (1988), Harvey and Shephard (1993), Linton et al. (2004), Robinson and Zaffaroni (1997), and Sentana and Wadhvani (1990).

Currently, research in the FMG is listed under four headings:

- Asset Pricing and Portfolio Management
- Corporate Finance and Governance
- Financial Regulation and Risk Management
- The Paul Woolley Centre for the Study of Capital Market Dysfunctionalities

The website lists over 30 researchers, and the range of research is very wide, but with not all studies involving econometrics. Given the range and quantity of the output of the FMG, the papers listed below are chosen to give an idea of the range of applied econometric research within the Group. Arbitrage activity: Lou and Polk (2013). Asset pricing: Buraschi et al. (2014a, b), Ghosh et al. (2017), Julliard and Parker (2005), Lou (2012), Lou et al. (2013), and Malkhozov et al. (2016). Financial markets: Cohen et al. (2017), Danielsson and Peñaranda (2011), Dasgupta et al. (2011a, b), Huang et al. (2016), Lou (2014), Patton and Verardo (2012), Cohen et al. (2009), and Verardo (2009). Portfolio choice: Cohen and Lou (2012) and Bretscher et al. (2016). Risk and uncertainty: Danielsson et al. (2012) and Danielsson et al. (2013). Semiparametric Bayesian inference: Julliard and Ghosh (2012).

8 LSE and the History of Econometrics

As discussed above, David Hendry made major contributions to theoretical and applied econometrics, but he has also developed an interest in the history of econometrics that led to contributions in this area.³⁸ He collaborated in 'A Re-Analysis of Confluence Analysis' with Mary Morgan (Hendry and Morgan 1989), whose PhD thesis he supervised and which formed the basis for Morgan (1990). In Hendry and Morgan (1995), the authors

³⁸When asked how his interest developed his answer was: 'Harry Johnson and Roy Allen sold me their old copies of *Econometrica*, which went back to the first volume in 1933. Reading early papers such as Haavelmo (1944) showed that textbooks focused on a small subset of the interesting ideas and ignored the evolution of our discipline' (Hendry in Ericsson 2004: 779).

provide an historical survey of the development of econometrics and reprint key historical articles. Further LSE contributions came from Spanos (1989a, b) and Thomas (1989, 1992).

Another of Denis Sargan's PhD students, Peter Phillips, who in New Zealand in the late 1960s began to develop an interest in the history of econometrics, made a significant contribution in a different direction when he established the journal *Econometric Theory* in 1985 and launched 'The *ET* Interview' series, which provides in-depth interviews with leading econometricians.³⁹ The history of econometrics is now a flourishing area of research, to which LSE has made a significant contribution.

9 Conclusion

Within LSE, the early contributions to econometrics came from within the Statistics Department. Arthur Bowley and Roy Allen established links with the Cowles Commission and the Econometric Society, and Bowley organised the European meetings of the latter. At LSE, Allen taught the econometrics courses 'Some Problems in Econometrics' from 1935–1936 to 1939–1940 and 'The Econometric Approach to Business Cycle Problems' in 1939–1940.⁴⁰ On his return from war service, Allen repeated his pre-war course in the 1946–1947 and 1947–1948 sessions.

When formal teaching of econometrics began as part of the BSc(Econ) degree in 1951, the teacher providing the course was in the Statistics Department, and, when he left LSE in 1952, there was nobody at the School to teach econometrics. The School had to import an econometrician, Wilfred Corlett, from UCL to teach on the course. Interest in econometrics in LSE's Statistics Department was revived through Maurice Kendal's research on the analysis of time series, and he was responsible for encouraging Jime Durbin to come to LSE from Cambridge.

There was some interest in econometrics on the part of some of the junior members in the Economics Department who set up the Methodology, Measurement and Testing (M²T) Seminar and carried out some early

³⁹There are now nearly forty interviews in the series, and those with Rex Bergstrom, Jim Durbin, David Hendry, Peter Robinson and Denis Sargan were extremely useful to the author of this chapter.

⁴⁰These courses were listed in the LSE *Calendar* to be taught in the 1939–1940 session, but with the outbreak of war and LSE's move to Cambridge, they may not have been given. While at Cambridge a course 'An Introduction to Econometrics' was taught in the 1942–1943 session by ex-LSE student, Erwin Rothbarth. This course was open to both LSE and Cambridge students.

applied econometric research and testing. However, with the rapid rise of Bill Phillips within the Economics Department, things changed dramatically. He and Durbin encouraged Rex Bergstrom and Denis Sargan to take up Readerships at LSE and under Sargan in particular econometrics flourished. The expansion of the teaching of econometrics and mathematical economics at both the BSc(Econ) and MSc levels meant that a large number of talented students went on to do research at the School. Among them was David Hendry, whose theoretical and applied research was a major contribution to what became known as the 'LSE Tradition in Econometrics'. This era came to an end in the 1980s with Hendry's move to Oxford in 1982 and Sargan's early retirement in 1984.

A new chapter in the development of econometrics at LSE began in 1984 with the return of Peter Robinson, and this continues with the current Econometrics Programme. The availability of very large data sets and greatly improved computing power has led to a theoretic rather than applied focus on nonparametric and semiparametric models among other areas of research.

There were also important changes in the organisation of research in econometrics, with researchers tending to work in research centres rather than as isolated individuals. At present, the two most important centres at LSE carrying out work in econometrics are the CEP and the FMG. These centres have been prolific in their output of research, and only a sample of their work has been presented here.

Finally, in the post-war period, an interest in the history of econometrics began to develop and Hendry became interested and contributed to its development. He encouraged Morgan's interest, and they and others published in this area.

Compared with the early days, the situation now is very different. Members of the Statistics Department at the School were pioneers in the development of econometrics. When the Economics Department finally took up mathematical economics and econometrics, LSE had a period of dominance with Sargan and Hendry in econometrics, and Robinson continues the LSE tradition by leading an outstanding research programme. However, there has also been an enormous expansion in the teaching of and research in econometrics carried out elsewhere in the UK, in some cases led by the research students taught by Sargan and Hendry. Although LSE is no longer the pioneer, the general expansion and rise in standards is to be welcomed.

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2

Economic History at the London School of Economics and Political Science: A View from the Periphery

Colin M. Lewis

1 Introduction

This chapter explores the role of economic history at the London School of Economics and Political Science (LSE) and contributions by economic historians to the intellectual and institutional development of the School. From the inception of LSE, economic history was a key component of research and teaching. Although such language would not have been used by contemporaries, when LSE was established in 1895, economic history was considered as a bridge between economics and other social sciences. A characteristic of economic historians at the School, even before the formation of the Department of Economic History, was methodological diversity and an emphasis on the applied. These traits were sustained as the group grew and the Department developed. Given this plurality in approach, which contributed and contributes to the depth and range of research and teaching at LSE, it would be a mistake to expect the formation of a distinct School

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of Aldwych Economic History, as occurred, for example, with the Vienna School of Economics, or economics at Cambridge, or the Manchester School of Economic History associated with George Unwin in the 1920s.

The chapter opens with a stylised description of the discipline and its development over time, largely slanted to perspectives prominent in the Department. This is followed by an assessment of the initial position of economic history at the School and contributions by various generations of economic historians to the subject and LSE. The fourth section offers a brief account of broader contributions by members of the Department to the organisation of LSE and scholarly community at large. The chapter closes with speculation about the future of the discipline and the Department, as implied by its long-term trajectory and recent changes.

2 Economic History: Locating the Subject and the Department

At the risk of provoking the spirit of the late Professor Donald C. Coleman, who once wrote, ‘It is not difficult to concoct brief definitions of economic history; but nor is it very rewarding’ (Coleman 1985: 35), this section provides a description of the subject. Economic history may be viewed as the application of social science concepts to the study of economic and socio-demographic change in the long run and to the analysis of processes and events that have transformed economy, society and polity. In assessing structural change, economic historians pay particular attention to the causes and consequences of growth and the impact on human well-being—that is the causes of poverty and inequality and trends in living standards and welfare. Resources and location also feature prominently: Why did some places develop before others; why did areas grow, and then ceased to grow, while growth proved to be a more enduring phenomenon in yet others? Such questions relate to the interplay between institutions and endowments and between technology and organisational factors as drivers of productivity. Growth recurring or growth sustained, had and has implications for institutionality and the human experience.¹

¹The phrase ‘growth recurring’ was coined by Eric Jones (see Jones 1988). Jones may be regarded as an early pioneer of global economic history, contributing to the subject before it was fully fledged. See below for further discussion on global economic history, and the new economic institutionalism (NIE), which influenced Jones.

Many economic historians would trace the intellectual origins of economic history to the growing body of writing in the seventeenth and eighteenth centuries about agricultural production, population, trade and public finance, that is political economy. For some, such early exercises in quantification prefigured twentieth-century national income accounting techniques. Yet, while Adam Smith's *The Wealth of Nations*, first published in 1776, may be regarded as characterising such writing, later authors like David Ricardo and John Stuart Mill offered largely theoretical works on economics; they were less inclined to historical narrative and detail than Smith. It was not until the late nineteenth and early twentieth centuries that the discipline assumed conceptual and institutional form that would be recognised today. According to Harte, the first Chair in Economic History was inaugurated at Harvard in 1893; it would be another 15 years before economic historians would be delivering inaugural lectures in the UK, though given by lecturers rather than holders of Chairs (see Harte 1971a: vii). Speaking in the early twentieth century, and noting the contributions of such thinkers as Smith and his contemporaries to the development of the discipline, Price observed that as a 'defined and systematic study economic history is comparatively new' (Price 1906: 26) and established its systematic credentials as follows: '[I]f history is to tell its full tale distinctly, facts must be grouped in orderly intelligible arrangement, and the competent study of economic principles may lend an assistance to such grouping which should neither be regarded as inopportune nor dismissed as idle' (ibid.: 22–23).

Many decades later, in a lecture given to mark the Golden Jubilee of the Economic History Society in 1976, T.C. 'Theo' Barker reflected that 'there were very few professional economic historians before 1914' (Barker 1977: 4). Yet, it enjoyed 'very widespread popularity' (ibid.). Barker explains this in terms of the evolving nature of the subject, notably a focus on industrial and labour history and on the social effects of industrialisation. Such publications as Toynbee's *Lectures on the Industrial Revolution in England*, Cunningham's *The Growth of English Industry and Commerce*, the Webbs' *The History of Trade Unionism and Industrial Democracy* and the Hammonds' *The Village Labourer, 1760–1832*, *The Town Labourer, 1760–1832* and *The Skilled Labourer, 1760–1832* were emblematic of this refocusing. These studies, which began to appear in the late nineteenth century, and around the time of the First World War, reflected concerns of the age and captured the popular imagination. Many of these books ran through several editions were widely read and widely discussed in public talks and lectures, for example extramural events organised by the 'new' red brick universities of the late Victorian period, and classes arranged by the Workers' Educational Association founded in 1903.

It was a melding of the historical and the contemporary, and of the economic and the social, that served to advance the discipline beyond aspects of political economy that had dominated much earlier writing. As argued further below, a capacity to combine historical analysis and reflections on the contemporary, resonating with sociopolitical and economic concerns of the day, may account for the institutional growth and academic presence of economic history as a discipline in the middle of the twentieth century. There was a veritable boom in the 1960s and 1970s, following pretty constant growth from the 1940s (see Harte 2001: 5–6; J. Tomlinson 1997: 232). It was not the case that newly created departments of economic history were populated by former graduates of the LSE Department and of Cambridge, trained by Ashton and Clapham respectively, though it occasionally appeared to be so according to some critics (see J. Tomlinson, *ibid.*: 235; BLPES Archives 1980–1991). Economic history was well placed to benefit from the huge expansion of higher education that took place in the UK in the 1960s because it had a firm theoretical base and methodology, a clearly defined cluster of core research questions and topics, established institutional bases represented by existing university departments, and a professional association, the Economic History Society, founded in 1926 (at LSE), and a peer-reviewed journal, the *Economic History Review* (see Stevenson 1997: 208).

To rephrase the question: What is distinct about economic history, what is specific about its methods and approaches, and how does it connect with other branches of the social sciences? As suggested above, most economic historians acknowledge a debt to late eighteenth-century political economy, with its interests in national wealth, population, trade and the funding of the state. By the middle of the nineteenth century, what would become economic history was already engaging with the study of the welfare impacts of economic change, and the role of technology—in agriculture, as well as manufacturing, business organisation, while displaying a continuing interest in well-established themes in political economy. If economics remained the dismal science, several aspects of the thematic concerns of the discipline fostered the image of it as a radical science. W.A.S. Hewins, founding Director of the School, might have described the subject as the study of historical economics, and emphasised the applied nature of the discipline, distinguishing it from the preoccupation with theory present in Marshallian economics. At his Inaugural Lecture delivered at the University of Edinburgh in 1908, George Unwin, a contemporary of Hewins, defined the subject as the study of ‘the economic condition of mankind at large through the recorded past’, acknowledging that it recognises that ‘the economic situation is more largely influenced, if not dominated, by forces and ideas which are

non-economic—the authority of the State, the power of custom and voluntary associations, the forces of nationalistic sentiment or of ethical conviction’ (Unwin 1971: 42). This implicit reference to the social sciences at large, and an emphasis on the need to temper (and develop) theory by analysing real conditions, became the hallmark of economic history and continues. On its current web page, the Department of Economic History describes the subject in the following terms: ‘Economic History uses concepts and theories from the Social Sciences as a starting point for studying the development of real economies and understanding them in their social, political and cultural contexts’ (LSE 2017).

According to Koot, at the beginning of the twentieth century, early historical economists in the UK who laid the foundations for the professional study of economic history considered themselves to be both economists and historians, sentiments that would have been shared by the founding fathers (and mothers) of LSE, not least when placing the discipline at the core of teaching (see Koot 1993: 646). Others, following Unwin’s efforts to define and locate the discipline, would have positioned it more broadly within the social sciences. For Sidney Pollard, economic history was a science of society. While other social sciences—not least economics and sociology—attempt to derive the laws of motion of society, it is to economic history that this task particularly falls (see Pollard 1965: 16). Among British practitioners, W.A. Cole, holder of the first Chair in Economic History at the University of Wales, established in University College, Swansea, was one of the earliest to place the subject firmly and formally within the social sciences, while arguing that ‘the study of economic history cannot readily be undertaken by one who has received a conventional training in either general historical studies or economic theory’ (Cole 1968: 2). The specifics of economic history, and its position in the social sciences, lie in the ‘fact that the links between history and theory are very much closer in the analysis of economic changes than they are in some other branches of historical studies’ (ibid.: 3). This, and the fact that economic history, like most sciences that derive their social justification from increasing our understanding of how man may master his world, define the discipline as a ‘genuine social science’ (ibid.: 12). Indeed, the scientific nature of the subject is due to the capability of ‘deepening our understanding of the past, to help us catch a fuller glimpse of the possibilities of the present, so that we may decide how best to shape our own future’ (ibid.: 23).

As the study of economic history became ever more firmly rooted in the social sciences, underlying tensions between those whose training derived from emphases on the primacy of documents and the place of narrative, and

those who were more inclined to begin their analytical and interpretive journey from a close engagement with theory, were never far from the surface. This is not to deny that there were others who saw not dissimilar debates about ‘frontier’ and approach between economics and economic history, as between economic history and history, or economic history and the history of economics: some were content to borrow, and others were inclined to prescribe (see Winch 1997: 1–2). Such tensions were about to become more pronounced. Around the time that Pollard and Cole were reflecting on the nature of the discipline, the methodology of economic history was being transformed by the quantitative approach of economic historians then based mainly in the USA. Various known as cliometrics, econometric history and the ‘new economic history’, applying a combination of economic theory, econometric techniques and mathematical modelling to the study of history, signal contributions of the 1960s included works by Fogel and Engerman (see Fogel 1964; Fogel and Engerman 1974). When Fogel was awarded the Nobel Prize for Economics in 1993, with Douglass C. North, international recognition of the approach and the discipline—as well as the individuals themselves—seemed assured.² By the end of the twentieth century, further developments in the fields of cliometrics and institutional economics led to the consolidation of the approach in the USA, with many economic historians associated with the National Bureau of Economic Research, Cambridge, Massachusetts, being fully paid-up members of the Clio Club.

Yet, as quantitative methods were gaining ground in the USA, a distinguished UK-based academic spelt out the dangers of the coming technique:

The very fact that the economic historian’s preoccupations are particularly susceptible to economic or sociological analysis, and to statistical handling, can easily lead him to write solely for an audience of his fellow specialists. It is sometimes desirable, and often convenient, to use a technical vocabulary and a mathematical formulation; but the taste grows insidiously and it is easy to slip into a habitual and quite unnecessary use of them (Davis 1965: 18–19).

This caution was appreciated by several contemporaries, particularly when applied to an emphasis on the counterfactual, an essential starting point for most practitioners of the ‘new’ economic history. As one sceptic, who applauded the contribution and potential of cliometrics, observed at the time:

²Fogel and North were recognised for having renewed research in economic history by applying theory and quantitative methods to the analysis of economic and institutional change.

The new approach would command wider approval if its advocates tempered their enthusiasm for statistics and economic theory with a greater respect for literary sources ... Attempts to measure precisely “what did not happen” inevitably contain a measure of quantified guesswork, and “new” economic history allows as much scope for suggestive guesswork and selection of evidence as traditional history, with the added disadvantage that subjective analysis is more easily overlooked in mathematical models (Hunt 1968: 17).

Even when valuing the findings of quantitative analyses, critics argued there was scant regard among some cliometric historians for the reliability of numerical sources and little effort was made to relate ‘reconstructed’ quantitative data to more traditional sources of evidence. Such scepticism was fairly prevalent in the Department of Economic History at LSE at the time: economic literacy and numeracy were valued while the usefulness of counterfactualism and questionable ‘data reconstruction’ was held in less regard. On balance, what Theo Barker describes as ‘an epidemic of econometric history’ (Barker 1985: 37) spreading from the USA diminished the appeal of the subject in the UK.

Another refinement largely deriving from the USA had greater impact in the UK and the Department, namely what may be described as the ‘new’ business history. More or less concurrent with the rise of cliometric history, business history was consolidating as a separate academic discipline—certainly a sub-discipline. Although regarded by T.S. Ashton as a branch of economic history, and until then largely associated with the production of glossy hagiographies of individuals or firms, by the 1980s, business history in the UK was responding to the business theory and case-study approach pioneered at the Harvard Business School, by N.S.B. Gras and, above all, Alfred Chandler. Intent on analysing the structure, scale and strategy of enterprises in order to explain the growth of firms, as well as examining the relationship between business and the State, practitioners drew on theory and historical cases. Writing on the eve of the new surge in academic writing, Arthur Cole acknowledged the contributions of economic history, and its practitioners, to business history or, as he sometimes preferred, ‘entrepreneurial history’ or ‘enterprise history’. For Cole, the subject involved an examination of the wheels of production, distribution and processes required to form and maintain enterprises, while the theory of the growth of firms required the study of ‘men working together’, using and applying (and extending) the knowledge of how to do things. Business history was not corporate history or biographies of businessmen, useful though these might be; it was considerably more and more systematic. The subject

required the scientific study of individual firms and of business in society (see Cole 1962: 98–101, 104–106). The challenges issued by Cole were taken up scholars in the USA and UK.

Drawing on economics and sociology, as well as economic history, historians of business consider the dynamic interaction of market, innovation (in production, organisation, administration and finance) and the political context in order to explore how firms grow and respond to opportunities and challenges. In the case of the Department, the impact of these new methods and approaches was reflected in the formation of the Business History Unit (BHU), largely due to the drive of Barker.³ For Tomlinson, in the UK, the ‘new’ business history was derived from the work of Charles Wilson, based at Cambridge, who imparted vigour to research and writing from the 1950s, notably in the form of a magisterial study of Unilever (see J. Tomlinson 1997: 247–248). Up to that point, apart from hagiographies, much academic writing had been confined to the study of dead firms, whose archives had become available. Another pioneer, based in the LSE Department, was Donald C. Coleman. His early work involved classic studies of the early modern English economy, followed by later contributions to business history. Coleman wrote about the paper and textiles industries, and about key individual entrepreneurs and financiers—for example Sir John Banks, the Restoration baronet and businessman. These endeavours culminated in Coleman’s magisterial history of Courtaulds. The UK Association of Business Historians awards an annual prize in honour of Coleman.⁴ Steven Tolliday observes that, by the early 1980s, the new economic history and Chandlerian business history had become largely autonomous sub-disciplines with a large degree of internal cohesion in approach and methods, achieving substantial institutional power in terms of appointments. Especially in the USA, cliometric history set the agenda ‘through its missionary zeal and its intimidating culture of expertise’ (Tolliday 2001: 389–390). As argued, the Harvard approach was equally influential in schools of business and management.

Contemporary with intellectual and methodological advances was the institutional growth of the 1960s and 1970s mentioned above. The number of separate Departments of Economic History (or Economic and Social History, as became increasingly fashionable) may have reached around 15,

³See the chapter by Hannah in this volume.

⁴Coleman read his first degree and completed his PhD in economic history at the School, was appointed Lecturer in Industrial History in the Department in 1951, promoted to Reader of Economic History in 1969 and was awarded a personal Chair in 1971, at which point he moved to Cambridge.

yet intellectual and institutional horizons narrowed thereafter. Harte asserts that the subject was established in practically every British university by 1970, when there were approximately 30 professors and almost as many departments (see Harte 1971b: xi). Whether due to the alleged negative impact of cliometric history, or the apparent appeal of business history, both provided more than a methodological challenge to economic history. By the end of the 1970s, the boom was over. After several decades of vibrant growth that featured an innovative research agenda and an expansion in the thematic frontier of the subject which included integrated subjects of historic and contemporary economic and social relevance, combined with radical thinking, the subject became dull, introverted and challenged, and was on the decline—at least according to critics (see Jones 1987: 119; J. Tomlinson 1997: 232; Harte 2001: 7, 8; Millum 2008: 5).⁵ To paraphrase Jones paraphrasing Coleman's acerbic put-down, the subject had become flabby, not unlike the annual conference organised by the Economic History Society where 'the large number of middle-aged (and largely male) members of the profession...look forward to their annual diet of hangovers and greasy breakfasts' (Jones, *ibid.*). Moreover, other social science disciplines proved more popular to students, for example sociology in the 1970s and development in the 1980s. The former may have appeared more radical, and the latter more relevant, ground hitherto commanded by economic history. There was also financial pressure on universities in the 1980s, with particular constraints experienced in precisely those institutions and subjects where the social sciences had expanded rapidly during preceding decades. When Glasgow's Department of Economic and Social History 'disappeared' into the School of Social and Political Sciences in 2010, the LSE Department of Economic History was the only stand-alone department in the UK, though many universities continued to offer economic and social history programmes, usually in departments or schools of history.

The 1970s was also a time of significant change for the Department, as well as for the subject. Institutional and methodological challenges, reinforced by personnel changes, which coincided with closures and mergers elsewhere appeared to threaten the independent existence of the LSE Department. In terms of personnel, there was a veritable changing of the guard. Jack Fisher retired in 1975. To borrow Dahrendorf's metaphor,

⁵A pithy account of the malaise encountered by the subject is offered by Coleman (1987). Such negative assessments are open to challenge, yet while some sceptics of the period may have had an axe or two to grind that does not invalidate many of their criticisms.

the retirement of Fisher marked the passing of the third generation of economic historians—or a transition from the third to the fourth. Fisher had been trained by Tawney in the 1920s, who was himself associated with those who had founded the School and who were instrumental in establishing economic history at the core of the curriculum. He had continued Tawney's study of the medieval and early modern periods in English economic history, and shared with his predecessors a reputation for provocative, stimulating teaching.⁶ Donald C. Coleman, in turn a student of Fisher, and one of the brightest minds of his generation, moved to Cambridge. Arthur H. John, who had been trained at LSE, and appointed Lecturer in Economic History in 1949 and succeeded Fisher as Convenor, died in 1978. A greater challenge was posed by the substantial reduction in the size of the Faculty as a result of the transfer of the 'BA Historians', as they were known, to the History Department. They tended to specialise in medieval and early modern economic history. The transfer reduced the size of the Department by around a third. It was a blow to morale at a time when the discipline was in apparent decline and the social sciences threatened by the intellectual climate of the moment. Given that most of the BA Historians wrote and taught on periods and subjects that had featured prominently in the syllabus of the School since its inception, including local economic history and the history of trade and empire in Continental Europe during the Norman and Angevin periods, the move signalled another break with the past.⁷

With the departure of the BA Historians, it appeared that the Department was abandoning a period and subjects that had defined it. Not until the appointment of Patrick Wallis in 2004 did the Department,

⁶See below for further comment on Fisher and Coleman.

⁷The BA Historians were Olive Coleman, A.R. 'Tony' Bridbury, Lucy Brown and John Gillingham. Their move from Economic History was determined by several factors. Some found the growing influence of quantitative methods and theory uncongenial; Bridbury was particularly hostile to 'ideological theory' and quantification, notwithstanding his work on economic growth in the late Middle Ages. (Paradoxically, in the final event, Bridbury decided to remain in the Department.) Some may have felt that the retirement of Fisher, the departure of Coleman for Cambridge and the resignation of Daniel Waley, who left LSE to take up the position of Keeper of Manuscripts at the British Museum in 1972, as likely to weaken the commitment of the Department to their period and subject. Perhaps, too, they were concerned about the position of the School regarding intercollegiate programmes. Until the transfer, the Department offered two undergraduate degree programmes: the BSc Economics (Economic History), taught exclusively to School students, and the BA History, a federal programme of the University of London in which students were encouraged to take courses in other parts of the University, in addition to their home college or school. At that point, the Department of International History at LSE only taught undergraduates registered for the federal, intercollegiate history degree offered across the University of London.

as some remarked at the time, recover its past—Tawney's Century.⁸ Wallis's appointment signalled the reinvention of contributions to subjects that had been core decades earlier, and also marked a welcomed revival of interest in social history. Wallis worked on markets, consumption and social conditions in early modern England, a subject and period that he promoted through the application of distinct approaches and imaginative writing. In the colder climate of the 1970s and 1980s, the School was also changing. The relaxed style of management—best epitomised by Fisher who saw little point in department meetings, when a word or two with colleagues would suffice—was no longer practical. In what would become a pattern for the future, the central administration expanded and the Department was subject to greater monitoring which required it to adopt more formal systems of internal organisation.

Yet all was not doom and gloom. By the early 1980s, notwithstanding institutional shrinkage as the number of separate departments declined, there were few history departments in the UK that did not include economic or social historians. The days when 'history' meant political history or constitutional history were long gone (see Harte 2001: 8). Emulating the achievements of the Economic History Society, and the endeavours of the Society for the Study of Labour History formed in 1960, the Social History Society had been set up in 1976. For the next forty years, until his death, its President was Asa Briggs (later Lord Briggs), the distinguished historian of Victorian England who was in turn an economic, social and urban historian. Social historians were determined to correct dismissive assessments of the subject deriving from Trevelyan's misquoted and misunderstood description of social history as 'history with the politics left out'; Trevelyan's view was that the social and the economic shaped political history. Social historians demonstrated that the study of popular responses to the agricultural, industrial and urban revolutions, the emergence of an industrial society, the growth of social and labour organisations, changing patterns of consumption and social attitudes, along with the development of associational life and demographic changes mattered, and could be studied systematically as new techniques and sources became available (see Stevenson 1997: 207–210). After a chequered existence for around a decade, by the mid-1970s, the Urban History Group appeared fairly resilient, planning to hold meetings and conferences to co-ordinate with those of the Economic History

⁸Tawney's research and writing about the origins of modern capitalism focused principally on the period 1540–1640, although he also published on the following 100 years (see below).

Society. Established in 1964, the Demographic History Group was more successful. In the 1970s, with financial support from the then Social Science Research Council, a series of conferences on Third World economic history were organised, continuing with partial funding by the Economic and Social Research Council. Some six such conferences had been held by the mid-1980s (see B.R. Tomlinson 1987–1988: 45). In certain quarters, the formation of such groups and specialisms signalled the fragmentation of economic history as a discipline, possibly contributing to the crisis of conscience of the 1970s; for others, they represented a new dynamic and opportunities. Professor Sir Tony Wrigley, an eminent British demographic historian and president of the Economic History Society from 1995 to 1998, remembers being drawn to economic history as an undergraduate at Cambridge, inspired by Michael ‘Munia’ Postan.⁹ Wrigley acknowledges a debt to the subject while maintaining the distinctness of demographic history and demography as a separate academic discipline.¹⁰ Several of these initiatives contributed to fruitful advances in the 1990s and beyond.

Despite the challenging climate of the period, LSE further enhanced its reputation for cutting-edge social science research and teaching, largely thanks to the efforts of the Director, Ralf Dahrendorf, who held the institution together, successfully negotiating problems generated by funding cuts and a negative political environment. In retrospect, and counter to contemporary pessimism, the 1970s and 1980s may be viewed as decades of renewal, notwithstanding institutional contraction associated with the merger/closure of economic history departments and considerable soul-searching by economic historians. Possibly absorbing new theories and research agendas from sociology and development—as well as responding to business history debates—economic historians began to turn their attention (or returned) to such topics as British decline, the position of Britain in the world economy, the international monetary system, the functioning of the global economic order, plus associated disparities and inequalities, and the ‘emergence of the Third World’. In pursuit of these subjects, economic historians borrowed and devised new approaches. Structuralism and ideas about the ‘development of underdevelopment’, comparative analyses of industrialisation, the economics of empire, the nature and structure of capitalism—its variants and alternatives—were some of the themes and

⁹Michael Moissey Postan (later Sir Michael) was known to his friends as Munia, a name that reflected his origin: he was born in Bessarabia, located in present-day Moldova and Ukraine. See below for further discussion of Postan.

¹⁰See <https://www.sms.cam.ac.uk/media/1139340>—video and transcript.

theories in the ascendant, or impacted on the collective conscious, eliciting varying degrees of enthusiasm or antagonism. Once again, pressing issues and problems of the day encouraged a re-examination of the past and revisionist thinking. Similarly, the performance of African, Asian and Latin American economies in the 1980s and 1990s saw economic historians look again at the prevailing international economic order to consider past constraints on growth, development and welfare in specific national contexts, as well the long-term implications of geography and environment, and factors inhibiting or limiting technology diffusion (see B.R. Tomlinson 2001: 394–395). Perhaps unsurprisingly, it was around this time that departments of economic history, and history departments with clusters of economic historians, began to extend teaching and research beyond a primarily British and North American (or European) focus (see Hunter 2001: 161). The Department at LSE sometimes followed these trends and tendencies and was sometimes at the forefront.

As above, the BHU was established at the School in 1978. This was a unique initiative, directed first by Leslie Hannah and subsequently Terry Gourvish. Hannah had been appointed to head the BHU in 1978, becoming Professor of Business History in 1982. The BHU was charged with promoting academic research in the field in the UK. The commitment to business history was strengthened with the appointment of Geoffrey Jones. Initially appointed to the Unit, Jones was subsequently offered a Lectureship in the Department. Later, he was appointed to the Chair at Harvard Business School held by Chandler. Barker had returned to the School, on appointment as Convenor of the Department in 1976, where he had taught between 1953 and 1964. An enthusiastic promoter of business history in Britain and overseas, Barker particularly enjoyed forging links with scholars in Eastern Europe and Japan, where he encountered special interest in reinvigorated debates about the decline of British manufacturing and the role of business and government in the economy. In 1970, Colin Lewis was appointed Lecturer in Latin American Economic History. The post was held jointly with the Institute of Latin American Studies, University of London, one of several such positions created at the School due to the Hayter and Parry initiatives¹¹ to encourage the study of Africa, Asia and Latin America in the UK. Janet Hunter, who had arrived at LSE in 1980

¹¹In 1961, the University Grants Committee (UGC) set up the Hayter Committee on Area Studies (Africa and the Orient) and the following year the Parry Committee on Latin America Area Studies to inquire into the state of research and teaching on these regions at British universities. The committees recommended the creation of area studies centres specialising in research and teaching, as well as the

to take up a Research Fellowship at the International Centre for Economics and Related Disciplines (later the Suntory and Toyota International Centres for Economics and Related Disciplines (STICERD)), was appointed Saji Research Lecturer in Japanese Economic and Social History in 1984. Gareth Austin, specialising in the economic history of Africa, arrived in 1988, followed by Kent Deng, working on the economic history of medieval China, appointed in 1995. Such 'area studies' added a further dimension to established extra-British interests in the economic history of the USA and Europe already present in the Department. William P. 'Bill' Kennedy had joined the Department in 1979; his research embraced cliometric history, particularly finance and innovation in the nineteenth century, and US economic history. Kennedy's arrival and interests complemented those of established senior figures like Charlotte Erickson and Jim Potter who worked on the USA and, along with Dudley Baines, had pioneered comparative teaching on the UK and USA, at the time the most popular non-compulsory first-year undergraduate course at LSE. Comparative teaching about the UK and the USA, the changing position of these countries in the world economy and the industrialisation of the international economy, would soon feature in undergraduate programmes at other colleges in Britain.

The European dimension of research and teaching was considerably enhanced when Alan Milward arrived as Professor of Economic History in 1986, a position that he held for ten years until being appointed official historian at the Cabinet Office, where he was commissioned to write the history of Britain's post-Second World War engagement with Europe. Taking a First in the BA History at University College London, he read for the PhD at the Department, being awarded the degree in 1960. He had extensive connections with centres of learning in Europe, holding posts in Italy and Norway. Endowed with a forensic mind and an impressive command of languages, he produced a substantial body of work that contributed to an understanding of the contemporary history of Europe and the European project, work that was renowned for its incisive analysis, meticulous investigation in the archives and accessible style. Max-Stephan 'Max' Schulze, appointed in 1993, strengthened the quantitative and European dimension of research and teaching in the Department. Schulze had taken a first degree in economic history at the University of Freiburg, spending a year abroad

funding of lectureships. The social sciences predominated among the lectureships, with a few appointments in history and literature, all funded for ten years by the UGC.

in the Department as part of that programme. He completed a PhD in the Department in 1993, where he was supervised by Baines, with whom he subsequently collaborated. Writing extensively about aspects of late industrialisation and development in Central Europe, he worked with quantitative historians (within and beyond the Department) to produce seminal pieces on growth, trade, human capital and economic convergence/divergence. Five years after Schulze's appointment, the arrival of Tim Leunig added to the cohort of quantitative historians and to the pool of business historians.

In the late 1980s, the NIE had an impact on the discipline, as it did on other branches of the social sciences, an impact that would prove to be more profound than that of cliometric history.¹² This may have been due to the increasing interest among economic historians of the nineteenth and twentieth centuries in the 'internationalisation of industrialisation'. It may also have arisen because of an interest in the study of comparative development in departments that were becoming home to scholars teaching about distinct parts of the non-North Atlantic world. While 'hard numbers' might have been more difficult to come by for some parts of the world than others, exploring the relationship between institutions and growth was feasible, though still challenging. Among economic historians, the writings of Douglass C. North, and his debate with Robert Bates, were especially influential. Borrowing from Ronald Coase, North sought to embed a theory of institutions more firmly into economics, revising and refining neoclassical theory by demonstrating that market rationality was not the norm, and that ideas and ideologies matter; they influence choices especially where information is incomplete and asymmetric, and determine transaction cost—the cost of doing business. This approach, which would have been applauded by George Unwin, was as appealing to historians of the pre-modern period like Stephen 'Larry' Epstein¹³ as to those of 'exotic' parts of the world, not least as it suggested that there was more than one 'rationality' and more than one

¹²The term 'new institutional economics' is usually attributed to Williamson (1975).

¹³Larry Epstein was appointed Lecturer in Economic History at LSE in 1992 and was rapidly promoted: by 1997 he was Reader and, in 2001, Professor of Economic History. A specialist in the economic history of medieval and early modern Europe, his appointment signalled the interest of the Department in subjects and periods that had been under-represented since the departure of the BA Historians. Larry's research and publications speedily attracted international attention. A provocative article in *Past & Present*, published in 1991, about institutions and trade in medieval Sicily and Tuscany, overturned conventional wisdom, became the new orthodoxy and set the benchmark for innovative thinking and an imaginative use of manuscript sources (Epstein 1991). His first major monograph, *Freedom and Growth: The Rise of States and Markets in Europe, 1300–1750* (Epstein 2000), which drew on the NIE, was recognised by the US Economic History Association as an outstanding publication, contributing to the consolidation of methodology and to the emergence of global economic history.

path to growth. Bates developed a new political economy approach which he applied mainly to Africa and, to a lesser extent, Latin America, a theoretical framework that melds game theory with more conventional approaches in order to explore the interaction of domestic politics and international political economy (see Bates 1987, 1997; see also North 1990). Bates analyses ideas, ideologies and interests of lobbies and sectional groups to assess the importance and action of non-market institutions in shaping behaviour, especially the perverse incentives that can prevail in imperfect markets.

Interest in the new institutionalism, which had been growing in the Department since the appointment of Professor Nicholas 'Nick' Crafts in 1995, was considerably strengthened during the short convenorship of Epstein, with a recruitment round in 2006. Partly by chance, though mainly by design, Epstein engineered the appointment of Gerben Bakker, Debin Ma, Chris Minns and Tirthankar Roy. To varying degree, all were influenced by the new institutionalism, or would embrace and apply the methodology. An historian of business and a student of management, Bakker was initially appointed jointly with the Department of Accounting, and had been trained at the University of Groningen and European University Institute, Florence, from which he obtained his PhD. Minns was principally an historian of labour markets, while teaching aspects of the economic history of North America, having been trained in Canada and the UK. Ma specialises in long-run economic growth, human capital formation, living standards and productivity, with particular reference to East Asia. Roy, who was already a full professor at the Gokhale Institute of Politics and Economics, Pune, when offered a Lectureship in the Department, worked on the history and development of South Asia, with several widely acclaimed books to his credit. The appointment of Ma and Roy, with later appointments in such fields as the economic history of Africa, Europe and Latin America, would strengthen and sustain the presence of the Department in these areas as well as the coming interest in global economic history.¹⁴ María Alejandra Irigoin and Leigh A. Gardner were appointed 2010–2011. Having initially researched on finance and State-formation in the River Plate during the mid-nineteenth century, Irigoin subsequently wrote about silver, trade, fiscal policy and fiat

He died tragically in 2007, aged 46. His life and work are marked by the annual Epstein Memorial Lecture (see <http://www.lse.ac.uk/economicHistory/whosWho/Stephan%20%28Larry%29%20Epstein.aspx>).

¹⁴See below for further comment on teaching initiatives and programme development associated with new currents in economic history.

currency systems in Asia and the Americas during the eighteenth century. Gardner works on money, taxation and fiscal federalism in colonial Africa.

Interest in global economic history was illuminated by the contemporary process of globalisation—or the second era of globalisation as would soon be argued by historians—then underway, while also encouraging new enquiry about the history of non-North Atlantic regions prior to their ‘integration’ into the world system after the mid-nineteenth century. There was, too, a legacy of radicalism, partly indebted to approaches associated with structuralism and developmentalism (for some, neo-Keynesianism) and related concepts that had dominated much social science writing about the history and contemporary predicament of the non-developed economies since the 1960s. In addition, there was a challenging, alternative radicalism advanced by proponents of the new institutionalism, sometimes conflated by its critics with neoliberalism. Perhaps it was a short step from research agenda questions about the determinants of different forms of industrialisation (an industrious revolution or an industrial revolution?) and the role of institutions in promoting/inhibiting growth—notably involving evaluations of the impact of distinct manifestations of European colonialism on parts of the Americas, Africa and Asia—to the new global economic history. Gareth Austin argues that the meta-dimension of global history derives from exploring comparisons and connection on a world scale, and is informed by four key areas of debate: the impact of human activity on the physical environment—man-made environmental change and responses to it that involve State action and resistance by the less powerful; early phases of market integration (and disintegration), including long-distance commercial networks pointing to world systems than can be traced back over millennia; the origins and nature of industrialisation, which questions notions of European exceptionalism and causal relations between levels of productivity and real wages in parts of Eurasia in the pre-industrial period, and differences between ‘labour-intensive’ and ‘capital-intensive’ paths to development in the very long run; and the endowments versus institutions controversy which, although emerging from debates about different institutional legacies and natural resources and the commodity lottery of colonial-period Americas, has a larger relevance (see Austin 2008a: 99–101).

In the Department, as elsewhere in the School, the new institutionalism proved particularly appealing to students of those parts of the world where existing theories appeared inadequate, or unconvincing, in explaining perverse, non-rational economic behaviour. The result, in collaboration with colleagues drawn from the then Development Studies Institute (DESTIN) (now the Department for International Development), was the organisation

in 1993 of a conference, held under the auspices of the Third World Economic History and Development Group and funded by the Overseas Development Administration, STICERD and the British Academy. The conference led to the publication of an edited volume that demonstrated a congruence of interest among development theorists, policy makers and historians of economic change, reinforcing the view of the importance of common lines of enquiry and debate between those concerned with the present and the past, not just because they happened to study the same geographical area (see Harriss et al. 1995). In addition, the conference and volume illustrated the way in which economic historians engaged seriously and often with economic and other social science theory, bringing historical perspective to bear on coming and mainstream theories.¹⁵ Such initiatives also influenced teaching.¹⁶ Departmental contributions to the emerging sub-discipline of global economic history were significant due to the thinking and drive of Patrick O'Brien and Austin, and later, Stephen Broadberry.¹⁷ In 1999, O'Brien returned to the Department as Centennial Professor of Economic History—he had read the BSc(Econ) between 1956 and 1958, having been awarded the Lilian Knowles Scholarship. He was one of the first Centennial Professors to be appointed by the School, holding the post for ten years, when he became Professor of Global Economic History. Previously, while Director of the Institute for Historical Research, University of London, O'Brien had established the Global History Seminar and the Leverhulme-funded Global History Network, endeavours which he brought to LSE, and developed further with European Research Council (ERC) funding. Initially for four years, the wide-ranging, innovative research project, entitled 'Regimes for the Production, Development and Diffusion of Useful and Reliable Knowledge in the East and the West', focused on Eurasia from the mid-fourteenth to the mid-nineteenth century. Based in the Department, under the acronym URKEW, this ERC project considered the comparative histories of education and knowledge diffusion in China, Europe, India, Japan and the Islamic world. The intention was to promote research and publications through the development of new methodologies that supported comparative enquiry over the very long run. There was also support for conferences and academic exchange, activities designed

¹⁵For a well-received and widely read historical critique of the vogue, especially a tendency among some growth economists to compress different historical periods and paths, and over-simplify when failing to differentiate (and disentangle) causal relations, see Austin (2008b).

¹⁶For related teaching initiatives, see below.

¹⁷For further comment on Stephen Broadberry, see below.

to facilitate the formation of a dynamic network of scholars that would endure.¹⁸ Tangible outcomes, in addition to publications and conferences, included the *Journal of Global History* and new teaching programmes and courses (see Austin 2008a). As such, global economic history is now as established a feature of the subject as cliometric history, business history and the new institutionalism, or earlier specialisms like urban history, demographic history and political economy.

One further recent initiative (or reinvigoration) deserves mention at this point: the history of economic ideas. As a distinct strand of philosophy, the history of economic thought predates political economy, though by the time of Adam Smith it was absorbed within it. When LSE was founded, as elsewhere, the study of economic thought was a well-established subject. W.A.S. Hewins, founding Director at the School, and the Reverend William Cunningham, recruited from Cambridge as a part-time teacher of economic history, would not have questioned the significance of the subject: courses were offered by those who would have regarded themselves as equally at home intellectually in a department of economics as of economic history. By the mid-twentieth century, however, the history of economic thought tended to be taught mainly in departments of economics. Such was the situation at LSE. Yet, as economics became more technical and theoretical—tendencies associated with new methods in the fields of econometrics and microeconomics—institutional interest in the history of economic thought declined in economics departments. In time, this would lead to the growth of teaching and research in development and political economy outside mainstream economics departments, as was the case at the School where DESTIN was created in 1990. At around this time, a revival of interest in the history of economic ideas was pioneered in the Department by Mary Morgan, specialising in the history and methodology of economics and econometrics, including the development of measurement in econometrics and economic modelling. Economic historians were repatriating the subject. In Morgan's case, this was done in association with colleagues in the departments of Philosophy and Economics. Her work in this area was recognised by the award of a British Academy Wolfson Research Professorship, the first such award to be made to a woman, and the first tenable at the School, and Morgan's subsequent appointment to the Albert O. Hirschman Chair, another first.

¹⁸For details of the depth and scope of the project, and its range across time and space, see: <http://www.lse.ac.uk/economicHistory/Research/URKEW/aboutUrkeW.aspx>.

For some ten years following 2007, the principal research and teaching interests of new appointments to the Department were quantitative and geared towards the study of finance and economic stress. Two appointments were made in 2007, Albrecht Ritschl to a Chair in Economic History and Oliver Volckart to a Lectureship. Ritschl, who had previously held posts in his native Germany, as well as Switzerland and Spain, worked largely on twentieth-century economic and business cycles, Volckart on financial markets in medieval Europe, having previously studied and taught in Berlin. Trained at Sciences Po, Paris, Olivier Accominotti was appointed in 2011, working in the fields of nineteenth- and twentieth-century monetary and financial history. A couple of years later, two further quantitative appointments were made: Joan Roses from Carlos III, Madrid, and Neil Cummins, from Queens College, New York. Roses' research focused on historical geography and factor markets in nineteenth- and twentieth-century Europe, especially Spain; trained in the Department, Cummins worked on living standards and social mobility in eighteenth-century England. Later tenure-track appointments included Eric Schneider (2015), working on comparative social conditions and social welfare in Europe and the Americas, and Natacha Postel-Vinay (2016), who focuses on twentieth-century financial history. Schneider had read for the DPhil at Nuffield College, Oxford, and Postel-Vinay for the PhD in the Department, where she had been supervised by Ritschl.

The community of economic historians at LSE was, however, never confined to the Department of Economic History. From around the mid-twentieth century, economic historians, or academics who contributed to the development of the discipline, were to be encountered in various departments. Notable examples of scholars working close to economic history in other parts of the School included Peter Bauer, Meghnad Desai, David Glass, Jane Falkingham, Jane Lewis, Hla Myint, Leslie Pressnell, Basil Yamey and, arguably, Lionel Robbins. Glass, a distinguished author in the fields of social and demographic history, was based in the Sociology Department for 30 years from 1948. Myint, a pioneer of development economics, read for the PhD at the School and taught in the Economics Department from 1966 to 1985. His work, notably his contributions to trade theory—he is the father of the vent-for-surplus model—and export-led growth, is widely read by students of economic history, particularly those studying African and Asian economic history. Near contemporaries of Myint, Bauer and Yamey also produced studies of developing economies that have become important source material for economic historians: Yamey taught at the School from 1950 to 1984, Bauer from 1960 to 1983. Pressnell, whose doctoral research was supervised by Ashton, spent very little

time at LSE, but he was responsible for establishing the Monetary History Group there, a workshop that sustained an interest in the subject when banking and monetary history had fallen out of favour in mainstream economic history departments, and flourishes in the distinct climate of today, when research on monetary and financial crises is very much in vogue. Appointed to a Lectureship in Economics in 1965, Desai taught at the School for almost four decades. His principal contributions have been in the fields of economic theory and the history of economic thought, global history and the economic history of India.¹⁹ The attention that his work commands among the economic history community is reflected in the fact that he was invited to contribute a chapter to the volume published to mark the 75th anniversary of the Economic History Society (see Desai 2001: 58–61). Lewis, a social historian who wrote about the history of women in England and the history of social policy, was associated with the Department of Social Policy and Administration between 1979 and 1996. Falkingham, who was trained in the Department, and works on demographic history, specialising in such aspects as ageing and poverty, was based at LSE between 1986 and 2002. As with Tony Wrigley, most of these scholars contributed to the development of economic history and collaborated with colleagues in the Department working close to their respective fields.

To conclude, economic history asks questions about demand and supply, about the cost of production, about levels of income and wealth, about the volume, value and direction of investment and trade and about sociopolitical contexts, including prevailing ‘ideas about the laws of motion of economic life’, asking such questions within regional, national and global contexts. It also deals with individuals and groups, with enterprises, organisations and lobbies, and the institutional framework within which they operate. Economic historians recognise—or should recognise—that economic phenomena and processes have no existence independent of the social, cultural, political, religious and spatial environment in which they occur (see Coleman 1985: 35–36). Consequently, while economic historians may usefully borrow theories and models from other branches of the social sciences, they need to elaborate their own approaches and concepts to test the research questions they ask and organise available data. The subject has proved to be innovative and appealing when it has been methodologically diverse, and radical and relevant. For its part, the Department of Economic History at LSE long sought to encourage and nurture methodological diversity.

¹⁹For further discussion of Desai’s life and career, see the chapter in this volume by Raja Junankar.

3 Economic History and Economic Historians at the School: The Impact of the 'First' and 'Second' Generations—And Beyond

LSE was conceived as a research and teaching centre of the applied social sciences, to which economic history was central. The focus and function of the institution were spelt out in the first issue of the School's *Calendar*:

The growing importance of social and economic subjects has drawn attention to the need of further provision for systematic training in economic and political science, and for the promotion of original investigation and research. While great success has followed the organization of economic and political studies in certain foreign universities, in the *École Libre des Sciences Politiques*, Paris, *Columbia College*, New York, and other institutions in foreign countries, no similar provision has been made for these subjects in the United Kingdom. It is now proposed to remedy this deficiency (LSE 1895: 3).

To this end, the work of the School would provide lectures and classes in 'Economics (including Economic Theory and Economic History), Statistics, Commerce, Commercial Geography, Commercial History, Commercial and Industrial Law, Banking and Currency, Finance and Taxation, and Political Science' and prepare students 'for public examinations arranged by the Civil Service (including the Indian Civil Service), Council for Legal Education, Institute of Bankers, Institute of Actuaries, London University (Mental and Moral Science), London Chamber of Commerce (Commercial Education)' (ibid.: 1). 'From its foundation in 1895, the London School of Economics (LSE) placed economic history centrally among the social sciences' (Harte 2001: 1). Economic history was the integrating core of the social sciences and was viewed as such by some of the founders and their near contemporaries, reflecting the strong prevailing interest in social and economic problems (see Lloyd 2001: 219).

When LSE was founded, there were already Chairs in Political Economy at Oxford, Cambridge and Manchester, but the subject was studied only as part of a broad curriculum. At the School, on the other hand, the study of the theoretical and applied social sciences would be centre stage (see Hayek 1946: 1–2). The Fabians, Christian Socialists and reformers who created LSE saw economic history as the laboratory of the social sciences. Widely read publications by the likes of Tawney, the Hammonds and the Webbs helped shaped the thinking of the age about the origin and nature of social and economic challenges (see Koot

1993: 641–642). Some of these public intellectuals may or may not have thought of themselves as card-carrying economic historians, yet they would be regarded as such by subsequent scholars. W.A.S. Hewins taught economic history. He recruited the Reverend William Cunningham from Cambridge, explicitly to counter the growing influence of Marshall and to ensure the promotion of historical methods among economists and the larger School community.²⁰

This initial impetus ensured the early centrality of economic history as much as economics. Cunningham's approach reinforced that of the Webbs and their associates, dovetailing with their pedagogical aspirations for the institution as a centre for research and teaching that was applied and practical—vocational even, all of which chimed with their interest in social and political reform—attitudes reflected in the curriculum. The core programme offered lectures in economics, commercial history, commercial and industrial law, geography, political science, public administration and finance and statistics—sociology was soon added to the list, and some years later, social science and administration; the student body was composed mainly of young civil servants, teachers, bank officials and 'a few women of leisure interested in the subject or engaged in public work' (Hayek 1946: 8). The BSc(Economics), or BSc(Econ) as it was generally known, was formally instituted in 1901, following recognition of LSE as a component of the University of London; the first degrees were awarded in 1904. Economic history was a compulsory component of the degree taken by all finalists, along with papers in economics and public administration, plus four papers

²⁰Hewins was trained in mathematics at Pembroke College, Oxford, and appointed to lecture in economic history on the University extension programme. At the School, in addition to serving as Director, he was appointed to teach economic history, or as the subject was described in the prospectus, commercial history. In 1903, he left the School to head the Tariff Commission set up by Joseph Chamberlain; Hewins had published on early modern English trade and finance and had written about tariff reform. A member of the Conservative Party, he was elected Member of Parliament for Hereford in 1912 and served under Prime Minister Lloyd George in the wartime coalition government as Under-Secretary of State for the Colonies. Cunningham read moral philosophy at Gonville and Caius College, Cambridge, before taking orders as an Anglican priest. His initial passion was for philosophy, which led him to economics and economic history. His faith, intellectual interest in the social sciences and first-hand experience of poverty and working conditions in the industrial cities of northern England led to a determination to write economic history textbooks and treatise on economic theory as well as methods critical of laissez-faire capitalism—works that reflected strong Christian principles and nationalistic, Conservative values. Before moving to LSE, Cunningham had taught economics and economic history at Trinity College, Cambridge, and held the Tooke Chair in Economic Science and Statistics at King's College London. Convinced of the superiority of historical methods in economics—as opposed to the orthodox or classical approach favoured by Marshall—Cunningham was the leading light of the English Historical School by the beginning of the twentieth century, an approach sometimes described as 'neo-mercantilist' due an emphasis on protectionism and State action.

in optional subjects. These required elements of the degree reflected the applied and vocational imperative. Trained in these subjects, young minds would be better informed about social and economic problems of past and present and how to address them through progressive policy (see *ibid.*: 2).

Of Hewins's early appointments, those of Lilian Knowles (née Tomn) and Eileen Power were probably the most influential for the future development of the subject at the School. Both had been researchers in economic history and occasional teachers at LSE, before being offered full-time posts; both published extensively and were regarded as inspirational teachers. Knowles held the first full-time Lectureship in the discipline in the UK, a post created at the School in 1904. A formidable teacher and engaging character, she proved to be a 'first' in many respects: she held not only the first Lectureship in the subject but went on to hold the first Chair in Economic History at LSE and was the first in a long line of women to hold personal and named Chairs in the Department (see Berg 1992: 316; Koot 1993: 646). She read History and Law at Girton College, Cambridge, studying with Cunningham. Despite gaining First Class marks, Knowles did not graduate because, at the time, Cambridge did not award degrees to women. She undertook research at the School from 1896 to 1898 and was employed as a part-time teacher ('Occasional Lecturer') in 1897 and 1898. In 1907, she was promoted to Reader and to a Chair in 1921, which she held until her death in 1926. Between 1920 and 1924, she was Dean of the Faculty of Economics of the University of London. A powerful personality, she is credited with having a profound impact on the intellectual life of LSE during its formative period, both as a teacher and researcher (see Hayek 1946: 10). Knowles's intellectual formation and interests blended Toryism and Fabianism: she would probably have preferred the descriptor Tory, than Conservative. This formation was reflected in her work, in which she developed ideas about the importance of the economic and social role of the State and aspects of what would now be called international political economy. She was especially interested in the imperial economy and tariff reform.

On appointment, Knowles observed that the study of English history at the School ended with the mid-nineteenth century and that there was little teaching of other parts of the world, something that she was determined to change (see Berg 1992: 316). Her initial work focused on the role of trade, transport and communications and technology, while acknowledging the importance of social and labour history. In addition to publishing research-based work in these fields, Knowles recognised the need for textbooks, the bedrock on which future teaching and research depended. She planned to fill this gap by writing textbooks about the economic development

of the Great Powers and the British Empire.²¹ Her much reprinted work, *The Industrial and Commercial Revolutions in Great Britain During the Nineteenth Century* (first published in 1921), combined many of these themes in a study that melded aspects of a research monograph and textbook. Yet Knowles is probably best remembered for her contributions to the economic history of Empire. According to a former student and associate, Allan McPhee, writing in the 1920s:

Fifty years ago the economic development of England was almost wholly unrecorded. Since then William Cunningham, Thorold Rogers, and other historians have done much to record the unrecorded. To-day the Empire is in the somewhat same state as England was fifty years ago. There is, therefore, urgent need for writing an Economic History of the Empire ... The foundation has already been laid by the late Professor Knowles' book, *The Economic Development of the British Overseas Empire*, published in 1924 (McPhee 1926: ix).

Conceived as a multi-volume work, only the first appeared before her death. As recorded on the contents page of the first volume—which contained an overview of the Empire as a whole while focusing principally on African and Asian tropical territories—an ambitious collection of companion volumes was planned (see Knowles 1924: xiii). These included a comparative book about the self-governing dominions, to be followed by specific case studies of Canada and Newfoundland, Australia and New Zealand, the Union of South Africa, and Rhodesia. In the event, most of this work was brought together in two textbooks, which became the second and third volumes of *The Economic Development of the British Overseas Empire*, prepared for the press by her husband (see Knowles and Knowles 1930, 1936).

Like her near contemporary Knowles, Eileen Power had been a student at Girton College, Cambridge. She also taught there, before being appointed by LSE in 1921. Initially recruited to teach political science, she was promoted to Reader in Economic History in 1924, and to a Chair in Economic History in 1931, which she held until 1938 on returning to Cambridge. She died unexpectedly, in 1940, aged 51. Three years earlier, she had married Michael Postan, more than ten years her junior, who had been her student

²¹Before beginning a projected multi-volume study of the British Empire, Knowles had virtually completed the manuscript of a textbook on the Great Powers yet set this aside to work on imperial economic history. The manuscript was published posthumously as *Economic Development in the Nineteenth Century: France, Germany, Russia and the United States* (Knowles 1932). In the Preface (p. vii), her husband, Charles Matthew Knowles, who prepared the draft of *Economic Development* for publication, wrote that it should be viewed as a companion volume to *The Industrial and Commercial Revolutions in Great Britain During the Nineteenth Century*.

and with whom she collaborated—they ran the famous Power–Postan medieval history seminar at the Institute of Historical Research, London. One of the most distinguished historians of her generation, she wrote prolifically and was regarded by many as the equal of Tawney and Postan. Arguably one of the principal medieval historians of the interwar period, she played a leading role in the development of the discipline, not least as the first Secretary of the Economic History Society, becoming the driving force behind its early development. Power encouraged historians to borrow ideas and concepts from other branches of the social sciences and was notably committed to the teaching of European and world history and, above all, social history, while promoting an interest in the medieval period (see Berg 1992: 323). Her major contributions included works on social and economic life, as well as edited collections of documents. In collaboration with several others, including her sister, Rhoda, the broadcaster and writer of historical novels, she produced a large volume of academic and popular works. Her most influential scholarly studies include *Medieval English Nunneries* (Power 1922), *Medieval People* (Power 1924), an essay on *The Wool Trade in English Medieval History* (published posthumously as Power (1941)), based on her 1939 Ford Lecture, as well as jointly edited and jointly written volumes like *Tudor Economic Documents* (Tawney and Power 1924), *Studies in English Trade in the Fifteenth Century* (Power and Postan 1933) and *The Cambridge Economic History of Europe from the Decline of the Roman Empire: Volume I: The Agrarian Life of the Middle Ages* (Clapham and Power 1942).

Much loved by colleagues and students, Power was remembered as an inspiring and vivacious individual, an independent spirit and a woman endowed with charisma, charm and beauty. She was an inspirational teacher, admired for her magnetic and elegantly crafted lectures, as much as for her writing. Awarded a Kahn Fellowship, the first woman to receive the scholarship, she travelled extensively in the Far East, where she was romantically involved with Reginald Johnston, tutor to the last Emperor of China. She kept a diary of her travels, acquiring material and insights that would later influence her writing and teaching (see Berg 1992: 322–323). While the impact of her work was powerful at the time, her writing and indeed her contributions to the development of the economic history have since become neglected. This may be due to an untimely death, possibly because the body of work subsequently produced by Postan overshadowed hers, perhaps because her principal interest was in social history—by the 1950s, economic history was becoming more ‘economic’, though not yet economicist. It may also have been that friends and associates were so influenced by the loss of such a bright personality that they tended to focus on the individual,

not her scholarship, thereby allowing her academic legacy to be neglected and her contribution to the historiography forgotten (see *ibid.*: 321–327; Harte 2001: 2; Mellor 1996: 1–5). Berg, however, offers other explanations. She sees the marginalisation of Power's contributions as due to her radicalism and feminism. Unlike her colleague Knowles, who was politically conservative, Power was an advocate of social and political reform: she favoured Home Rule for Ireland, Scotland and Wales, promoted education and enquiry into labour and working conditions. After the First World War, she was an active peace campaigner and an internationalist, strongly supporting the League of Nations, and opposed the rise of fascism. She was critical of British foreign policy during the 1930s and joined William Beveridge in setting up the Academic Freedom Committee in 1933, founded to help academics fleeing Germany. Power broadcast on these subjects and penned newspaper articles. She was also unafraid to view the findings of her research through the optic of concerns of the period in which she lived. For Berg, these elements in Power's story offended male colleagues, who were inclined to feminise and thus trivialise her achievements (see Dahrendorf 1995: 236). This, rather than an untimely death and relatively circumscribed body of academic publications, accounts for the subsequent neglect and marginalisation of her academic writing and contributions to scholarship (see Berg 1992: 321–327; 1996).

LSE and Cambridge, notably Girton College, played a key role in the development of economic history. They were the institutions where teaching and research were pioneered and nurtured. Close institutional and personal links were forged by scholars based in London and Cambridge, and movement between the two centres was fairly common in the formative period of the School. Ellen McArthur was among the first to establish the Cambridge–London connection. She taught economic history at Cambridge between 1902 and 1912. A history tutor at Girton, she lectured in economic history for the University extension courses and was an Occasional Lecturer in Economic History at LSE. As Chairman of the Council of Girton College, William Cunningham had fostered the study and teaching of economics and history. Unlike his Cambridge colleague Marshall, Cunningham actively promoted the careers of women students and by means of college scholarships and publications funds supported their research. McArthur and Knowles were among his early, notable collaborators, and he was instrumental in the positions obtained by both at the School. He encouraged McArthur to give lectures there and was similarly supportive when Knowles took the same path from Girton to London (see Berg 1992: 314–316).

However, as Berg argues:

After McArthur and Knowles, there appeared to be a direct route through Girton College and the Cambridge Historical Tripos to research at the L.S.E. Yet Girton College, single sex, feminist in outlook, and isolated both geographically and educationally from the rest of Cambridge University, was a world away from the L.S.E., co-educational, centred on the social sciences, and resolutely involved in the political and economic issues of the day. While Girton provided the mainstream academic route to economic history for a number of women, the L.S.E. developed the subject and brought in a much broader range of students, both men and women, from all kinds of unconventional backgrounds (ibid.: 316).

Another formidable early appointment was Eleanora Mary Carus-Wilson—Nora to her friends. Born in Canada—her father became a distinguished Professor of Electrical Engineering at McGill University—she spent most of her life in England. Carus-Wilson studied at Westfield College, University of London, which had been co-founded by her grandfather. As such, she did not share the Cambridge connection of some of her predecessors and near contemporaries. Carus-Wilson did, however, share with some of them, and the founders of LSE, firm Christian principles. Her early career was as a teacher: she did not begin to develop major research projects until the mid-1930s, on being awarded a Leverhulme Scholarship. Also specialising in medieval economic history, her work combined the study of textiles, commodities and long-distance international trade, research that included an investigation of cloth production and manufacturing technology in various parts of England and trade with the Low Countries and Scandinavia. She published statistical material on English exports from the thirteenth to the sixteenth century, one of the first such time series to be systematically organised. Carus-Wilson originally published in these fields in the late 1920s, though her most prolific period of research and writing were in the decades that followed, despite several years spent as a civil servant during the Second World War. At this time, her contributions were influenced by Power—she was an active member of the Power–Postan Seminar. Her achievements were recognised by various bodies, including the Royal Academy of Belgium and the British Academy. She was a stalwart of the Economic History Society, becoming its first woman President in 1966, and had been instrumental in efforts to diversify the publishing activities beyond the *Economic History Review*. In 1954, she edited the first volume of *Essays in Economic History*, a selection of key articles from the *Review*, possibly designed to make material more accessible to an undergraduate readership; two more volumes followed in 1962.

According to Joyce Youings, Carus-Wilson was very much a grande dame: ‘Tall, slim, elegant, vivacious and ever-youthful...with a supreme

self-assurance which could often be intimidating. But there was no pretence about her' (Youings 1977: iii). Notwithstanding a reputation for research excellence, in the 1950s and 1960s she became even more regarded as a teacher and lecturer. Appointed at LSE in 1945, she was promoted to Reader in 1948 and to Professor of Economic History in 1953, a position she held until retirement in 1965. Known affectionately to generations of students as 'Lady Carus-Wilson', she commanded the attention of the lecture theatre with a lucid, authoritative exposition. She was among the first researchers and teachers to shape the development of economic history and, with Knowles and Power, established the tradition of distinguished women professors of economic history at the School. It is worth restating that all three, Knowles, Power and Carus-Wilson, held Chairs in economic history at LSE.

Vera Anstey (née Powell) was a bluestocking of the same generation, though her scholarship is less remembered than that of her predecessors, despite having a room named for her at the School—the VAR. She took a First in Economic History at LSE in 1913 and was appointed to a teaching post in 1921, on returning from India after her husband died. Percy Anstey had been principal of Sydenham College, Bombay. Initially appointed an Assistant Lecturer, by 1941 she had been promoted to a named Readership, Sir Ernest Cassel Reader in Commerce. Uniquely, Anstey held two positions: she was Reader in Commerce and Lecturer in Economic History (see Berg 1992: 308). The main focus of her work was on India. Her seminal monograph, *The Economic Development of India* (Anstey 1929), examines the origin and consequences of poverty, which she attributed to the prevailing backward social order that posed an obstacle to growth, provoking such distortions as high population growth and unenterprising and irrational attitudes. Her analysis was widely accepted at the time (see B.B. 1930; see also Roy 2014: 20). A good citizen of the School, she served as Dean of the Faculty of Economics of the University of London between 1950 and 1954. She retired in 1964. Yet her academic production was overshadowed by female and male colleagues among the economic history community—Tawney, Postan and Ashton, in addition to Knowles, Power and Carus-Wilson.

According to Dahrendorf, Power, Tawney and Postan constituted the second generation of economic historians at LSE (see Dahrendorf 1995: 233). Richard Henry 'R.H.' Tawney was formally appointed to the staff at LSE in 1920, although he had been connected with the School from around the time that Anstey was completing the BSc(Econ). Promoted to Reader in 1923, he was awarded a personal Chair in Economic History in 1931, a position he held until retirement in 1949. A Christian socialist, Tawney was an active member of the Church of England and lifelong friend of

William Temple, who became Archbishop of Canterbury, and strong advocate of adult education (see Kirby 2016). Having had read Modern History at Oxford, he acquired an interest in economics while tutoring for the Workers' Education Association (WEA) in Yorkshire. On marrying William Beveridge's sister, Jeanette, in 1909, the couple moved to Manchester. His religious convictions, democratic socialist principles and reflections on urban and industrial problems of the day echoed those of the Webbs, the Hammonds and other prominent supporters of LSE. In modern parlance, Tawney would be deemed a public intellectual; in his day, he was regarded as a public figure. In addition to a distinguished academic career, Tawney stood for the Labour Party in several elections, but was never successful. He had joined the Fabian Society in 1906, and served on the Sankey Commission, enquiring into conditions in the mining industry, as a representative of the Miners' Federation in 1919 (see Harte 1971b: xxviii; 2001: 2–3). An acute observer of social predicaments of the day, Tawney's search for an explanation of contemporary economic and political conditions led him to the study of the emergence of modern capitalism. His principal works include *The Agrarian Problem of the Sixteenth Century* (Tawney 1912), *The Acquisitive Society* (Tawney 1921) and, above all, *Religion and the Rise of Capitalism* (Tawney 1926; subsequently reprinted by Pelican), in which he explores how beliefs shaped social and economic attitudes, lamenting that the system of modern capitalism that emerged in the seventeenth century lacked ethical or religious justification and *Equality* (Tawney 1931). In these works, and in pamphlets and contributions to the press, Tawney set economic history in a wider context that included the moral and philosophical. He advocated the need for social reform, democracy and collective responsibility and was a religious thinker of considerable force and influence (see Kirby 2016: 822).²²

For much of his time at the School, Tawney was a contemporary and active collaborator with Power and Postan, until the former's death in 1940, and the latter's move to Cambridge in 1935, where he succeeded Clapham. At LSE and Cambridge, Postan contributed to the development of economic history in Britain and internationally (see Flinn and Mathias 1982). As with Tawney, his principal works were read extensively in the English-speaking world and beyond. In the early 1920s, Postan took the BSc(Econ) and read for a Master's degree at the School. In 1926, he was appointed Research Assistant to Power, his wife to be. He held a Lectureship in Economic

²²For a fuller appreciation of the scope of Tawney's work and impact, see the chapter in this volume by Noel Thompson, Fisher (1961), Jackson (2007: 168–169) and Goldman (2013).

History at LSE from 1931 to 1935, following a teaching stint at University College London. During the 1920s and 1930s, he established a reputation as a medievalist, largely through joint publications with Power on English trade in the fifteenth century and a series of individual authored trailblazing articles about trade, credit and the accumulation of capital. Challenging the orthodoxy of the day, Power and Postan speculated that embryonic capitalistic relations were observable during their period, and that there were connections between commercial exchange in the medieval period and modern capitalism. Perhaps because he heeded Tawney's advice to escape—at least partially—from the medieval period, Postan's broader contributions to the literature and institutional development of the discipline occurred mainly during his time at Cambridge, where he was appointed to a Lectureship in 1935, and then to the Chair in Economic History, which he held from 1938 until retirement in 1965. His notable endeavours were as editor of, and contributor to, the eight-volume *Cambridge Economic History of Europe*—the first volume appeared in 1966; as editor of the *Economic History Review* between 1934 and 1960; as co-founder of the International Economic History Association in 1960; and his lectures on social theory, philosophy and radical thought (see Daunton 2017). For almost half a century, as Editor of the *Review*, and as Vice-President and Honorary Vice-President of the Economic History Society, he was a leading influence on the development of economic history. His leadership in the field was recognised with the award of a knighthood in 1980.

Working on the Industrial Revolution, Thomas Southcliffe 'T.S.' Ashton's acceptance in 1944 of the Chair in Economic History previously held by Power, and vacant since her death four years earlier, marked a distinct chronological shift—as well as the beginning of the 'third generation'. He held the post until retirement ten years later. Confessing that 'I am more of an economist than a historian' in his Inaugural Lecture, Ashton acknowledged an intellectual debt to George Unwin—Ashton had taught at Manchester University before taking up the appointment at the School—signalling how a training in economics and statistics shaped his research on the key industries that made the Industrial Revolution (coal, iron and steel) and study of economic fluctuations (see Ashton 1971: 163). At Manchester, Ashton had brought lustre to economic history and to the study of industrial history (see Fitton 1989: xi). Tawney, who had winkled Ashton out of his beloved North, anticipated that Ashton would do the same in London. He did. Arguably his best work, or the work for which he is best known, *The Industrial Revolution, 1760–1830* (Ashton 1948), was published four years after the move to London. For long regarded as the classic study,

Ashton advanced the case for positive economic and social benefits brought by the Industrial Revolution, rejecting the thesis of 'Merrie Englanders' who focused on the catastrophic desecration of the countryside and deteriorating living standards and working conditions for the common man (and woman) during the phase of rapid industrialisation. Technical innovation, economic growth and social opportunity yielded material gains and cultural progress. Ashton continued to publish after retirement, donating funds to the Economic History Society, money now used to finance the T.S. Ashton Prize, awarded annually for the best article published in the *Economic History Review*. Ashton was succeeded in the Chair by Jack Fisher, who had particularly enjoyed teaching economic history in the 1920s because, 'There was very little written. You could make it up as you went along' (Fisher quoted in Dahrendorf 1995: 234–235).

Frederick Jack Fisher spent most of his adult life at LSE; it was where he was trained and taught. He entered as an undergraduate in 1926, reading History, the same year the Economic History Society held its Inaugural Meeting at the Aldwych, and took an MA, with Distinction, in 1931. He was quickly drawn to economic history, which he saw as a vibrant new field. This 'conversion' was effected by Power and, especially, Tawney, his senior by thirty years, to whom he was devoted and with whom he enjoyed a fruitful collaboration. Fisher had been Tawney's student and was his research assistant (see Corfield 1990: 4–6; Coleman 1988: 343). In his seminal article, 'The Rise of the Gentry, 1558–1640', Tawney acknowledged Fisher's critical contributions (see Tawney 1941: 22, fn. 1). To cite Harte once again, 'Writing and publishing came unfortunately low among his [Fisher's] priorities' (Harte 1990: 23), an opinion endorsed by another colleague, Donald C. Coleman (1988: 344). Fisher did not finish two large projects on which he worked on and off for several decades, namely the economic history of early modern London and a wide-ranging study of the early modern English economy. Yet this in no way diminishes the quality and impact of what he wrote. Fisher was an essayist, whose most important articles remain critical starting points for any research on the study of pre-industrial England. However, his impact on the subject probably derives even more from his teaching career. Appointed successively Assistant Lecturer in Economic History at LSE in 1930, Lecturer in 1935 and Reader in 1947, in 1954 he succeeded Ashton to the Chair, which he held until retirement in 1975. He was thus responsible for training virtually two generations of scholars during the formative phase in the development of the discipline and its drive to maturity. Many remember his inspirational lectures, particularly those offered to first-year students. Having spent most of the Second World War in the armed forces

overseas, Fisher's approach, arguing that the problems that confronted early modern Britain were similar to those encountered by post-1940s India, Africa and the Middle East, engaged and resonated with successive cohorts of undergraduates, not least those who had also served abroad during the War (see Elkan and Roberts 2000: 368). For those who encountered him in his prime, such phrases as 'a most exhilarating and remarkable teacher', 'he was the very best teacher of first year students', 'a most uncommonly gifted teacher', 'one of the most impressive university teachers I have ever met; a stimulating tutor, a fine scholar, an engaging man of great wit and warmth of spirit', were commonplace (see Harte 1990: 26, 30, 31, 34).

Charlotte Erickson was the first woman to hold the Chair in Economic History after Carus-Wilson. She was trained in the USA at Augustana College and Cornell, where she developed her skills in quantitative history and an interest in the history of agriculture and rural life and where she was awarded a PhD. This training confirmed a commitment to the history of immigration, a topic that she made her own, and to which she brought insights from her personal story and that of her family, Swedish Lutherans who had settled in the Midwest. Her interest in economic and demographic history was sharpened in the UK, when she studied at the School in the late 1940s with T.S. Ashton and David Glass, an experience that strengthened her conviction that history was a social science. Her appointment as Lecturer in the Department of Economic History in 1955 marked the end of what contemporaries may have regarded as a recruitment 'boom'. Jim Potter, who wrote on the US economy in the twentieth century, had been appointed Assistant Lecturer in American Economic History in 1948, and Arthur H. John, whose early research involved British agriculture and trade, was appointed a year later (see below).²³ Erickson was awarded a personal Chair in 1979, some 15 years after John had received a Chair, later moving to the named Chair in Economic History. In addition to pioneering work on the social and economic history of immigration—telling the migrant story from both sides of the Atlantic—she wrote about British industrialists, work that was recognised as an innovative fusion of economic, social, demographic and entrepreneurial history and which combined quantitative and qualitative approaches. Following Erickson, it would be some years until the Department was again headed by distinguished women convenors—Mary Morgan and Janet Hunter.

²³The next recruitment boom would take place some years later, involving Dudley Baines, Peter Earle, Malcolm Falkus and Eddie Hunt.

For students, Erickson was a splendid teacher, particularly when championing the cause of American history—she became a key figure in the British Association for American Studies, founded in 1955, and was a warm, approachable individual. Thoroughly at home in the UK, it was always clear that she hailed from the other side of the Atlantic, despite Carus-Wilson's best endeavour to encourage her to adapt to the English ways of doing things (see Harte 2008). She was her own person, and helped steer the Department through a difficult period, despite not enjoying the best of relationships with some senior male colleagues who resented her steady promotion. Erickson's managerial style was inclusive and consultative, as well as meticulous, much suited to the challenges then being confronted by LSE, and indeed the discipline of economic history. Erickson left the School in 1982 to become the first holder of the Mellon Chair in American history at Cambridge, the first female Fellow of Corpus Christi College, moving in the opposite direction to that of some of her predecessors several decades earlier and later securing a prestigious (and lucrative) MacArthur Fellowship.

Dudley Baines and Peter Earle were equally engaging teachers—in quite distinct fields and much influenced by Jack Fisher, who was Convenor when they were appointed. In association with colleagues working on US economic history, and like Erickson writing about the history of trans-Atlantic migration and the development of the international economy, subjects about which he has written extensively, Baines was key to the success of what was to become the mainstay of first-year teaching, a comparative study of the economic history of the UK and USA, which subsequently became even more international in focus when headed by Tim Leunig. A skilled, motivational teacher and supervisor, Baines established the reputation and popularity of the course as early as the 1970s. It was taken as a non-compulsory option by more undergraduates than any other non-compulsory course at LSE. A hint as to the nature of the course, and his teaching then and since, is captured in various acknowledgments. Such comments as 'most inspiring teacher', 'I am especially indebted to...Dudley Baines...for [his] invaluable advice and encouragement' and 'He made complex principles of economics come alive in the classroom through deep and colourful historical narrative' are commonplace (see Itoh 2001: viii). Colleagues were similarly appreciative of his style as Convenor: he was an effective, inclusive head of department—for example, on his watch, Crafts was enticed from Warwick. Earle taught and wrote in a style that captured the imagination of students and readers alike. Author of critically acclaimed studies of economy and society in the seventeenth century, he also wrote popularly—or had the flair of writing in an accessible manner that ensured the popularity of carefully

researched monographs.²⁴ Earle taught what he wrote, and wrote what he taught, to the delight of students.

The writing and teaching of early generations of economic historians at the School had an impact within and beyond the college. In the formative period, when the bibliography was limited, ‘writing it up’ was a necessary task—or ‘making it up’, as Fisher would have preferred. Yet even at this stage, figures such as Power and Postan recognised the role of teaching—giving lectures and taking classes that enthused students and made the subject available to a wider world. That accessibility—and popularisation—was achieved through critically applauded publications and scintillating teaching. Arguably, such research and writing skills, and imaginative teaching, are even more important in the twenty-first century. With a growing emphasis on teaching quality and student satisfaction, institutionalised by the National Student Survey, reflected in the media and commanding the attention of university administrators, comments such as those mentioned above have a relevance and resonance. Plaudits earned by Power, Postan, Fisher and later generations may be even more significant today than they were then in sustaining the appeal of economic history and the Department, along with establishing connections between the content of courses and programmes with the issues of the day.

4 The Department and the Scholarly Community

In view of its relative size (despite a substantial expansion of research and teaching staff in the twenty-first century, the Department remains fairly small), economic historians contributed substantially to the organisation and administration of the School. It was not by chance that Hewins was an economic historian. Later generations have also contributed to the life of LSE and, indeed, the broader scholarly community. Fisher, for example, was the first academic to chair the Committee of the British Library of Political and Economic Science (BLPES), holding the position from 1965 to 1975 (see Wise 1990: 38–39). Previously, the post had been filled *ex officio* by the Director. Fisher also served as Vice-Chair of the Academic Board, an elective

²⁴Earle’s principal publications, which demonstrate his range, include *The World of Defoe* (Earle 1976), *Monmouth’s Rebels: The Road to Sedgemoor, 1685* (Earle 1978), *The Treasure of the Concepción: The Wreck of the Almirante* (Earle 1980), *The Pirate Wars* (Earle 2006) and *The Sack of Panamá: Captain Morgan and the Battle for the Caribbean* (Earle 2007).

post in which he deputised for the Director from time to time. John initially enjoyed a dual academic and administrative career. He became Assistant Registrar at the School in 1946–1947, when he took up an appointment as Registrar at the University of Nottingham, only to return to LSE as Lecturer in Economic History in 1949. Five years later, he was promoted to Reader and ten years after that, in 1964, was awarded a personal Chair in Economic History, which he held until his death in 1978 (see Stern 1979: 8). This trajectory was opportune. He was an Academic Governor during the ‘Time of Troubles’ at the School in the late 1960s and subsequently became Pro-Director. In addition to standing in for the Director when required, the principal role of Pro-Director was to act as a link between the academic body and the administration.²⁵ John also played a significant role, alongside Lionel Robbins, in securing funding for, and planning the development of, the new library building. This involved the purchase and conversion of Strand House, the W.H. Smith repository adjacent to LSE. Securing and converting Strand House to the needs of the BLPES was the largest estate projects considered by the School up to that point (see John 1971; Howson 2011: 986–988). The library building is now named for Robbins.

A generation later, Leslie Hannah followed John, being appointed Pro-Director in 1995, and served as Acting Director of the School, 1996–1997. Less than ten years after Hannah, in 2004, Paul Johnson was Pro-Director. Trained at Nuffield College, Oxford, where his doctoral research had been supervised by Nick Crafts, Johnson joined the Department of Economic History in 1984 and was awarded a Chair in 1999 in recognition for his work on the making of Victorian corporate capitalism and the emergence of the welfare state.²⁶ Johnson’s research interests and publications echo that of many famous predecessors, combining the historical and the current, the academic and the practical. His study of the history of welfare capitalism informs contributions to such pressing contemporary debates as the funding of social insurance systems and pension reform, including research commissioned by bodies such as the World Bank, the European Union and government agencies in Australia and the UK. As Pro-Director, Johnson oversaw key curriculum changes and structural reorganisation, changes that reflected developments at the institutional and national levels and occasioned considerable discussion at LSE. When Howard Davies’s term as Director was

²⁵Until the late 1990s, the School appointed only one Pro-Director—later there would be four.

²⁶Johnson’s principal works include *Twentieth-Century Britain: Economic, Social and Cultural Change* (Johnson 1994), *Making the Market: Victorian Origins of Corporate Capitalism* (Johnson 2010) and the three-volume *Cambridge Economic History of Modern Britain* (Floud and Johnson 2004).

drawing to a close, there were rumours that Johnson, who had become Vice-Chancellor of La Trobe University, Melbourne, in 2007, might return to the UK to head the School.

Other, recent senior administrative positions have been held by Peter Howlett.²⁷ He held significant positions in LSE's administration, including the post of Dean of Undergraduate Studies, as well as serving the Economic History Society. It would be tedious to list members of the Department and the posts that they have held, or the committees on which they have served, suffice it to say that the Department has punched above its weight. As already suggested, members of the Department of Economic History have served the wider scholarly community as officer holders of the Economic History Society. As mentioned, Power and Tawney played leading roles in founding the Economic History Society and establishing the *Economic History Review* (see Harte 2001: 2). When appointed editor of the *Review*, Postan was still based at the School, though left shortly for Cambridge. Harte records how the Society and *Review* were well-served by office holders connected with the Department from the 1920s to the 1970s (ibid.: 2, 7). Subsequently, Hunt, Hunter, O'Brien and others have filled the main *Review* and Society offices, as well as Howlett. From 1926 until 1948, the Society Annual General Meeting, which functioned also as the annual conference, was held at LSE. The first residential conference, held at the beginning of Easter vacation, took place in Birmingham in 1950, breaking with the tradition of the one- and two-day meetings-cum-conferences that hitherto had always been held at the School.

According to Tomlinson, the Department played an important role in the institutional expansion of economic history as a discipline at a critical phase during the expansion of higher education in the UK: 'In the expansion of higher education in the 1960s and 1970s, the LSE-trained economic historians were able to staff many of the new departments and able to ride on the flowing tide of popularity of the social sciences' (J. Tomlinson 1997: 232). But that was before 'The Cuts'. It would be a gross distortion and arrogant to argue that the Department has trained the world. However, given the size of its Master's and doctoral programmes, and coupled with the closure and merger of departments elsewhere in the UK, the Department has made a marked contribution to academic training. Since the 1980s, the number of

²⁷Appointed in the 1980s, Howlett strengthened the Department's teaching and research in the areas of nineteenth-century British business and labour history, as well as teaching accessible quantitative options.

PhD students supervised in the Department has been substantial. Although electronic versions of all School dissertations have been automatically digitised only for the last decade or so, the number of economic history dissertations available electronically (including some earlier works that have been digitised) points to the range and diversity of work completed.²⁸ Many former students have secured appointments in major research institutions in the UK, Europe, the Americas, Asia and Africa. Finally, as already suggested, several distinguished economic historians, possibly primarily associated with other (or several other) institutions, have served time in the Department. Such scholars include Austin, Broadberry, Crafts, Jones, O'Brien and Postan. This does not mean that economic history in the UK is 'owned' by the Department or by the School, as colleagues based elsewhere have not been slow to remind the Aldwych community, yet the Department has played a unique role in the development of the subject.

5 The Future: By Way of a Conclusion

Does economic history need an institutional and organisational base to survive? The answer is probably 'yes'. While, in various parts of the world, economic historians are to be found in departments of history and of economics—sometimes in fairly large clusters, offering programmes of economic history as well as contributing individual courses to undergraduate and postgraduate programmes in social science and history departments—the survival of separate departments of economic history is critical. It is instructive that such departments exist in parts of Asia, Latin America and Scandinavia and Iberia. Yet, the existence of an institutional base is not enough. To flourish, economic history needs to value diversity in methods and approach in research and teaching that includes the local and the global, while ranging over time and space. If the discipline becomes methodologically narrow by over-focusing on quantitative approaches, it risks becoming a second-rate branch of economics—or viewed by students as a soft option; if the insistence on using social science theory to frame research agenda and organise data is lost, what is there to distinguish economic history from history?

As has been argued here, the discipline has been at its most dynamic and attractive when practitioners have sought, in their research and teaching, to

²⁸See <http://etheses.lse.ac.uk/view/sets/LSE-EH.html>.

bring the past into the present and to use the past as an optic to appraise the present. The Department of Economic History at LSE has also been a particularly collegial place when it has been a focus for collaborative research and teaching—though which is the chicken and which the egg may be debated. Plurality of method and individual research interests that feature distinct periods or places has not been—and should not be—a barrier to collaboration. Such collaboration, whether in writing or teaching, is also appreciated by students, as some of the comments recorded above imply, another factor that contributed to collegiality across the larger community.

The community of economic historians at the School was not the first to be established in the UK, but it can claim several ‘firsts’. These are the vibrancy of the early group of scholars who came to constitute the Department, the prominence gained in the field by a succession of critically acclaimed women researchers and teachers, the quality of research and its impact and pioneering initiatives in teaching. This further connects with matters of plurality and inclusiveness. If method and approach narrow, or research and teaching cease to reflect the relevance of the study of the past for understanding the problems of the present, the Department would risk losing its reputation for intellectual rigour and vitality.

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3

Accounting and the Influence of Economics at LSE

Christopher J. Napier

1 Introduction

Accounting and economics are closely related disciplines. Central concepts of economics such as capital (see Nobes 2015), profit, income, cost and value have their roots in the practices of accountants. A century ago, the German economist Werner Sombart (1919; see also Most 1979) claimed that double-entry bookkeeping, the technique for recording and processing transactions used by many business organisations since the Middle Ages, was necessary for modern capitalism to emerge. Although this view has been widely challenged (e.g. Yamey 1949), it has been argued that the theoretical concept of capitalism could not have been developed by economists in the nineteenth century without the existence of double-entry bookkeeping and in particular the capital account (see Chiapello 2007). This allowed entrepreneurs to look beyond the specific resources that made up their business—properties, equipment, inventories, accounts receivable—by determining an overall monetary measure for the business: the owner’s capital. This capital could be manipulated through calculations (what Max Weber called *Kapitalrechnung*—see Miller and Napier (1993: 635)) that provided a rational basis for making profits.

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Accounting theory was already developing in Continental European countries such as France, Italy and Germany by the eighteenth century (see Näsi et al. 2014), often as an integral part of economics. For example, in Italy, accounting was (and still often is) closely associated with *economia aziendale* (see Zambon 1996; Viganò 1998), the economic study of organisations across time and space (sometimes referred to as business economics). In Britain (and the USA), accounting was slower to emerge as an academic discipline, and a business economics tradition similar to those in many Continental countries (see Biondi and Zambon 2013) did not develop to the same extent (see Napier 1996). The most important place in Britain where economics and accounting came together intellectually from the 1930s onwards is the London School of Economics (LSE).

Economic thinking seems to suggest solutions to several accounting problems, including the problem of *valuation*. Businesses produce periodical statements of financial position (traditionally known as the balance sheet), in which their assets (the resources that the business controls and uses to generate future cash inflows) and their liabilities (the claims on these resources) are included at monetary amounts. But how should these amounts be determined? Traditionally, asset measurement was based on the original cost incurred to acquire the asset, but this was subject to various accounting conventions, such as depreciation, that had developed in the nineteenth century, with the rise of the corporate economy. However, a cost-based measure might not, particularly for assets that had been owned for several years, be a fair representation of the *value* of the asset, whether that was thought of as the amount that could be realised in cash through selling the asset or the future amounts that would be recovered in the form of cash inflows earned by using the asset in the business.

Another problem is that of *income* or *profit*. Businesses also produce periodic statements of financial performance (traditionally known as the profit and loss account), and again the determination of periodic profit is subject to accounting conventions. These provide guidance on the timing of revenue recognition (e.g. what has to be achieved for a business to include the revenue from a sale of goods or supply of services in the profit and loss account) and on associating (accruing or matching) the costs involved in earning specific revenues to calculate the net profit for a period. However, was this accrual-based approach to profit a reasonable basis for measuring the performance of a business, particularly as it reflected only gains that had been realised through a sales transaction, but not unrealised increases in the value of assets?

The third problem is that of *cost*. Entrepreneurs and accountants developed various methods of calculating the cost of manufactured goods.

Some components of total cost, such as materials and labour (direct costs), can be attributed directly to specific units of production, but other costs, including most overheads (indirect costs), had to be allocated using some formula if they were to be included in cost numbers. This approach is known as full costing or absorption costing because the full costs of production are absorbed in the costs attributed to specific units. The distinction between direct and indirect costs is similar to the economist's distinction between variable and fixed costs, so accounting theorists began to look to economic ideas about costs for insights into whether common business approaches made sense from an economist's perspective. Economists tended to emphasise marginal costs, and often suggested that absorbing fixed overheads in the cost of production used arbitrary bases of allocation that resulted in fallacious cost measurements, and hence led to bad business decisions.

The application of economic reasoning to accounting has been a recurring feature of the accounting discipline at LSE. In the 1930s, young economists with strong accounting backgrounds, in particular the 'three Ronalds' (Baxter 1991: 139)—Ronald Edwards, Ronald Coase and Ronald Fowler—applied economic reasoning to suggest radical changes in accounting practice. In the 1950s and 1960s, the 'LSE Triumvirate' (Whittington 1994)—William Baxter, Harold Edey and David Solomons—and colleagues such as Jack Kitchen, John Flower, Bryan Carsberg and Susan Dev—used economics to recommend changes to the balance sheet and the profit and loss account that would provide more useful information for investors and other users. From the mid-1980s, LSE Professors such as Michael Bromwich and Richard Macve have continued to refer to economic theory to understand and reform accounting practice, even though many accounting academics at the School, particularly Anthony Hopwood and his followers, have taken a more sociological direction. They have criticised the influence of economics on accounting thought and research, while observing that accounting calculations have enabled organisations to apply economic methods to their operations. However, in the early years of accounting at LSE, the direct influence of economics is difficult to discern.

2 The Beginnings of Accounting at LSE

Accounting has been taught at LSE since 1902, soon after the establishment of the School in 1895. The discipline was part of the commerce field, along with commercial geography, commercial law and railway economics, and contributed to the 'study and investigation of the concrete facts of industrial

life', as set out in the School's original prospectus (quoted in Dahrendorf 1995: 20). Accounting teaching was offered not only on the Bachelor of Science in Economics degree—BSc(Econ)—but also on the railway course, a programme of study for the managers of railway companies, which was an important source of revenue for LSE (see *ibid.*: 118). The School appointed Lawrence Dicksee to teach 'Accountancy and Business Methods' (see Kitchen and Parker 1994a: 219). Dicksee was an accounting practitioner who had taught accounting since the 1880s, and had written several influential books, including *Auditing: A Practical Manual for Auditors* (Dicksee 1892). He was also part-time Professor of Accounting at the University of Birmingham.

In *Auditing*, Dicksee discussed the valuation problem for assets in the context of preparing the balance sheet. He distinguished between permanent and floating assets, corresponding to the classical economic distinction between fixed and floating (or circulating) capital. During the nineteenth century, in legal cases concerned with the ability of companies to pay dividends (see Yamey 1941; French 1977), judges used economic notions of fixed and floating capital to determine what exactly was meant by the legal requirement that companies should not pay dividends out of capital. Dicksee disagreed with these decisions, because the courts permitted companies to take account of unrealised increases in the value of assets constituting fixed capital. To Dicksee, these were merely fluctuations in value that should not be reflected in the accounts. Dicksee applied a going concern approach to asset valuation, assuming that fixed assets such as buildings and machinery will be used in the business over the long term rather than sold to generate a short-term profit. Hence, the accounting problem for such assets is how to reflect wasting, i.e. the loss in value through use or the passing of time. Dicksee discussed how this wasting could be reflected through depreciation, where the cost of a fixed asset would be gradually written off over its life using a systematic formula, such as straight-line depreciation, which spreads the original cost equally over the expected life of the asset.

As well as his auditing textbook, Dicksee published *Advanced Accounting* in 1903, noting how accounting courses were springing up not only in Birmingham and at LSE but also in Manchester and other provincial universities. However, this book refers to economics only in passing. The book gained increased importance at LSE because of the introduction of the Army Class in 1906, where army officers were provided with education in business administration (see Kitchen and Parker 1994a: 219), and teaching this course alongside the existing offerings meant that Dicksee no longer had time to work at Birmingham as well as LSE, while maintaining his professional practice.

Dicksee was given the title Professor of Accounting and Business Organisation in 1914 (see Napier 2011: 188). He introduced courses in business administration and costing, and he also took part in the development of Higher Commercial Education more generally. In 1917, Sidney Webb, LSE's co-founder, wrote to William Pember Reeves, the then Director, concerned that the London County Council planned to provide high-level business education and that this would threaten the School's standing. Although Reeves reminded Webb that LSE was already offering a large amount of teaching in this area, he developed a plan for what was to become the Bachelor of Commerce degree—BCom—and raised £150,000 (worth around £7.5 million in today's money), mainly from the financier Sir Ernest Cassel (see Dahrendorf 1995: 130–131). Much of this endowment was used to establish new academic posts in areas relevant to commerce, and in 1919, Dicksee became the Cassel Professor of Accounting and Business Methods (see Kitchen and Parker 1994a: 220). The expansion of commercial education—by 1928, about a quarter of all the students at LSE were studying for the BCom—meant that Dicksee could no longer deliver all the courses by himself, and a new lecturer in accounting was appointed, Frederic de Paula (see Kitchen and Parker 1994b).

De Paula, a chartered accountant with a small practice in London, taught professional accounting students, and he had written a textbook *The Principles of Auditing*, published in 1914. On Dicksee's retirement in 1926, de Paula succeeded him as Cassel Professor of Accounting, and later that year, he gave an Inaugural Lecture on the place of accounting in commerce. De Paula argued that the chief accountant of a business 'should not be merely the head book-keeper but he should be the chief financial officer of the concern, being responsible to the general manager for the whole of the finances of the business and its financial control' (de Paula quoted in Kitchen and Parker 1994b: 232–233). De Paula envisaged the chief accountant as responsible for the preparation not only of financial accounts but wider business statistics that would be used by management for controlling the business and also for future planning.

During the 1920s, corporate financial reports came under increasing criticism for failing to provide adequate information about the underlying financial position and performance of companies (see Edwards 1989: Chapter 11). Both Dicksee and de Paula advocated more extensive, and better-structured, corporate disclosure. Readers of corporate financial statements often could not ascertain how assets had been valued and were not told how much depreciation had been provided in respect of fixed assets. In a series of articles for the weekly professional magazine *The Accountant* in

November 1925, de Paula argued for greater openness in financial reporting, observing that ‘the arguments in favour of secrecy are exaggerated’ (de Paula quoted in Kitchen and Parker 1994b: 232), but he was ahead of his time, as reforms to company law in the late 1920s made only marginal improvements to corporate financial statements.

De Paula left LSE in 1930 to become Chief Accountant of the Dunlop Rubber Company. Before leaving, he wrote to the then Director, Sir William Beveridge, noting the establishment of a Department of Business Administration. In de Paula’s view, accounting belonged in such a department, where ‘the place of Accounting in the general organization of a business and its uses to Management’ could be studied and taught in a ‘scientific manner’ (de Paula quoted in Napier 2011: 189–190). The more conventional accounting teaching was to continue as before, provided by Stanley Rowland (see below), while the Department of Business Administration was to emerge as the location of innovative thinking about accounting, strongly influenced by economics.

3 Thinking About Accounting Through the Lens of Economics: The Plant Years

The Department of Business Administration, Research and Training (DBA) at LSE was financed by ‘a number of firms and individuals interested in developing the study of business administration and the training of men for responsible posts in business’ (LSE 1931: 234). Students would follow a one-year programme for a Diploma in Business Administration (see Napier 2011: 190). The DBA was unusual for LSE, which until 1962 was not formally organised into separate academic departments. The departure of de Paula had left a vacancy in the Cassel Chair, and this was filled through the appointment of Arnold Plant.

In his early twenties, Plant had been working as a manager in an engineering company when he was encouraged to ‘learn something about management before doing much more of it’ (Coase 1987: 891). He decided to study for both the BCom and the BSc(Econ) degrees at the same time, concentrating on economic history. Plant was particularly influenced by Edwin Cannan, ‘whose views and common sense approach to economic analysis and economic policy were to be reflected in Plant’s own work’ (ibid.). Immediately after graduating, Plant was appointed Professor of Commerce at the University of Cape Town, returning to LSE in 1930 as Cassel Professor of Commerce with special reference to Business Administration.

Plant has been described as ‘an inspiring teacher—tall, formidable, free alike with praise and rebuke. A student likened his lectures to the dissecting of a cadaver by a master surgeon’ (Baxter 1991: 139).

In his Inaugural Lecture, Plant suggested that the creation of the DBA was evidence that ‘reticence among practising business men is giving way to a readiness to co-operate more widely in the work of training and research’ (Plant 1932: 46). He reiterated the tension between secrecy and openness in his comments on company accounts:

It is surely an amazing fact that doubt still seems to exist in business circles whether the owners of a business, that is to say its shareholders, are entitled to know at specified intervals the current value of their property, as accurately as it can be ascertained without unreasonable expense. Recently prominence has once again been given to the question whether the non-disclosure of the precise values of reserves is compatible with the fulfilment of this expectation ... Yet clearly it is high time that business leaders, professional accountants and students of business practice faced squarely the compatibility of secret reserve policy with the duties of agents and trustees (the board of directors) towards their principals and clients (the shareholders) (ibid.: 48–49).

Plant was referring here to the recent conviction of Lord Kylsant, the Chairman of the Royal Mail Steam Packet Company, for issuing a false prospectus that failed to disclose that the firm’s reported profits during the 1920s had been artificially inflated by the use of undisclosed (secret) reserves to hide the fact it had made a series of trading losses (see Arnold 1991).

Plant attracted a group of young colleagues, some of whom had backgrounds in accounting. Among these were the so-called three Ronalds—Edwards, Coase and Fowler: ‘[T]he name Ronald seemed to guarantee excellence’ (Baxter 1991: 139). Ronald Edwards had qualified as a certified accountant in 1930 at the age of 20 and then took the BCom. He came to the attention of Plant for the quality and originality of his work and was appointed to an Assistant Lectureship in business administration in 1935 (see Ackrill 2004). Ronald Coase followed the ‘Industry’ pathway of the BCom, ‘an option supposedly designed for those who were going into works management ... I took courses in works and factory accounting and cost accounting as well as financial accounting’ (Coase 1990: 3). Coase became a protégé of Plant, who secured a travelling scholarship so that Coase could visit the USA to study industrial organisation. After graduating in 1932, Coase held academic appointments at Dundee and Liverpool, where he taught himself economics by reading both classical and more

contemporary works (see Medema 2017). He returned to LSE as an assistant lecturer in 1935; Coase had kept up a connection with the DBA through research into producers' expectations with Ronald Fowler during the vacations (Coase and Fowler 1940). Fowler himself was an LSE graduate who was appointed as a Lecturer in commerce upon graduation, and specialised in statistics (see Thatcher 1997). He wrote *The Depreciation of Capital* (Fowler 1934), an analytical study that challenged standard accounting methods of depreciation.

Coase, Fowler and Edwards worked together to analyse the financial statements of businesses (Coase et al. 1938), particularly those in the iron and steel industry. They found that only public companies (which usually had stock exchange listings) were required to publish financial statements, so 'economists who are studying the vital problems of trade cycle theory are robbed of the valuable data contained in balance sheets' (ibid.: 1). Observing that 'public balance sheets *were never intended for use by economists*' (ibid.: 2; italics in original), they suggest that the usefulness of profit numbers is limited by the tendency of company directors to use accounting methods that smooth out reported profits (creating secret reserves in good years by reporting a lower profit figure than what was actually achieved, then drawing on those secret reserves in poor years to augment profits, or even convert actual losses into reported profits). They also criticise the general absence of consolidated financial statements for businesses organised through a holding company and subsidiaries, with the effect that detailed information about the assets, liabilities and profits or losses of large businesses was contained only in the unpublished financial statements of subsidiaries, not in the published statements of the holding company. The study concludes by observing:

[S]ome of the major difficulties are due to the lack of uniformity in the practice of accountants and also, which is to some extent the reason for this, the obscurity of much of their reasoning and the distinctions which they employ. It follows that an improvement in accounting theory would materially assist the work of economic research (ibid.: 12).

The Coase et al. (1938) study was published by the Accounting Research Association (ARA). This had been set up by Edwards, together with Cosmo Gordon, the librarian of the Institute of Chartered Accountants in England and Wales (ICAEW), in 1936 (see Zeff 1997: 7). The editor of *The Accountant*, Vera Snelling, also supported the ARA, and the magazine's publisher, Gee & Co., printed and circulated the ARA's publications. Edwards and Coase used *The Accountant* to create a 'new, iconoclastic and more scientific literature'

(Parker 1999: 24), with Edwards contributing a series of articles under the collective title ‘The Nature and Measurement of Income’ from July to September 1938 (Edwards 1938) and Coase writing articles on ‘Business Organisation and the Accountant’ from October to December of the same year (Coase 1938). The ideas contained in these articles were examples of the ‘LSE tradition in cost theory’ (Buchanan 1973: 3), which will be examined in the next section.

4 Accounting and Cost Theory

In the 1930s, the leading economist at LSE was Lionel Robbins. He was the same age as Plant, and they had been contemporaries as undergraduates. Robbins was quite different from Cannan, who regarded economics as a matter of common sense and avoided mathematical analysis (see Coats 1982: 23). Robbins, however, argued that ‘it is better to push ahead with our analysis, embrace technicality with open arms if technicality will help us, and come to be so frequently right that we acquire the respect now given without question to the practitioners of the natural sciences’ (Robbins 1930: 24).

The views of Robbins about the relationship between analysis and technicality were expressed forcefully in a memorandum he wrote about the BCom. He thought that this degree was too technical and many of the subjects covered did not deserve inclusion in a university syllabus. His criteria for inclusion were that ‘The subject must relate in some way to the “nature of things” or the main issues in the life of humanity’, and that ‘The subject must be architectonic and capable of affording *aesthetic* satisfaction’ (Plant Papers, BLPES Archives: 222; italics in original). For Robbins, subjects such as mathematics, history and accounting, ‘taken by themselves, fail miserably’ (ibid.). The strange coupling of accounting with mathematics and history may suggest that Robbins was objecting not so much to the disciplines in their own right as to how they were taught on the BCom: ‘Many of the papers from the intermediate onwards involve merely the regurgitation of large masses of facts’ (ibid.). However, this memorandum suggests that the teaching of accounting was held in some contempt by LSE’s leading economists. Reforming accounting through applying economic ideas could therefore be seen as raising its status from technical to analytical.

Robbins was influenced by the Austrian School of economists, including Ludwig von Mises, who was also known to Plant (see Coats 1982: 25). Robbins invited Friedrich von Hayek, a colleague of Mises, to give a series of lectures at LSE in 1931, and Hayek was subsequently appointed as Tooke Professor of Economic Science and Statistics in 1932 (see Brittan 2004).

Robbins, Hayek and Plant had similar views about the effectiveness of markets as co-ordinating devices in comparison with central planning, and these views rubbed off on the three Ronalds. Coase cites all three economists in his seminal paper 'The Nature of the Firm' (Coase 1937), and subsequent commentators have shown how Coase's argument that organisations will co-ordinate activities internally where the costs of doing so are seen to be less than the transaction costs of using markets was inspired by contemporary economists at LSE (see Foss and Klein 2014; Bertrand 2015).

The Plant group considered that current accounting practice relating to cost measurement was deficient. As Solomons (1952a) shows, cost accounting developed as an offshoot of double-entry bookkeeping, where maintaining accounts for different stages of production allowed businesses to attach costs to products. However, difficulties arose with respect to costs that could not be directly attributed to specific products. As businesses became more complex, with many distinct products being produced using common technology, managers needed to allocate joint costs across the different products. Allocation formulae were often used. For example, various indirect overheads (sometimes called oncosts) would be allocated to specific products in proportion to the labour time spent on each product, or in proportion to the cost of labour. However, was there an optimal allocation formula? More radically, was allocation of overheads economically irrational? As Gould (1974: 96) sums up:

Although cost accounts were installed primarily for purposes of administrative control, it was natural that businessmen should turn to their accountants for figures to aid decision-making: for example, the businessman concerned with selling price would ask what an article cost to manufacture. This seemingly straightforward question turned out to contain hidden difficulties, among which were the valuation of materials drawn from stock, the computation of depreciation, and the allocation of overhead expenses. By the 1930s little progress had been made towards the resolution of these problems, other than by patently arbitrary devices, and orthodox accounting was, to a perceptive critic, a manifestly unsatisfactory tool for decision-making.

The Plant group accepted the view of Robbins (1932: 15) that economics is 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses'. For decisions, the focus should be on marginal costs—what *additional* or incremental costs would be incurred as a result of taking a specific decision, or, from another angle, what costs would be *avoided* by *not* taking the decision? Identifying the relevant incremental costs would require the manager to investigate what would

change if a particular decision were made. For example, an engineering company that accepted an order to produce a machine would need to use materials. If these had to be purchased specifically, then the cost of the materials would be easy to observe, but what if the materials were withdrawn from inventory? The original cost of these materials would be irrelevant—what mattered was any *incremental* expenditure that would result from using the materials for this order rather than the next best alternative. If the materials would simply be replaced in inventory, then the cost for the order was the replacement cost of the materials, as this was the incremental expenditure. However, if the materials had no alternative use, they would not be replaced. In this case, using the materials on the present contract meant that the business would forego the chance of selling them, so the incremental cost was the sales proceeds that would otherwise have been received.

This is an opportunity cost approach—cost is what the organisation sacrifices by using a resource in one way rather than another. As Edwards (1937: 290) describes it, ‘we test the profitability of increased output by examining marginal variations in cost and revenue. In other words, we compare increments to cost with increments to revenue, rather than totals or averages’. This study was to be quoted by Coase in his articles in *The Accountant* the following year (see Coase 1938). Coase begins the articles by explaining the notion of avoidable cost and its link with the economist’s notion of marginal cost. He stresses the importance of uncertainty for business decision-making, noting the role played by managers’ attitudes to risk:

Consider now a businessman trying to decide between alternative courses of action, each of which might produce so many different results. It is clear that the choice will depend partially on the attitude to risk-taking of the person deciding. Some businessmen will be influenced much more by possibilities of high profits which are not very probable than will others. There is no one decision which can be considered to maximize profits independently of the attitude of risk-taking of the businessman (ibid.: 537).

Uncertainty becomes a more significant problem as the time period over which forecasts of future receipts and payments have to be made lengthens, and Coase advocates the use of present value (discounted cash flow) calculations to reflect the time value of money. Combining present value with opportunity cost leads Coase to define ‘the value of an asset’ as ‘the present value of the net receipts which it is estimated will be obtained from ownership of that asset’ (ibid.: 631). Because of the incremental approach adopted, it is *changes* in the value of an asset such as a machine that matter for decisions. For example:

If a machine is used in the present instead of leaving it idle, it may well be that its life is shortened. This means that profits that would have been earned at the end of its life will now no longer be received. This loss of profits in the future through the use of a machine in the present is a cost of using the machine which must be taken into account (*ibid.*).

Coase referred in passing to orthodox accounting writers, but Edwards, in his series of articles in *The Accountant* (Edwards 1938), engaged directly with leading accounting scholars and practitioners in both the UK and USA. Edwards was also aware of broader American scholarship, including the work of economist John B. Canning, whose *The Economics of Accountancy* (Canning 1929) had discussed issues of income measurement and asset valuation. Canning developed what Whittington (1980: 238) has labelled the opportunity difference approach to valuing assets in terms of the minimum cost of replacing the asset's services, assuming that replacement is economically appropriate (see also Zeff 2000). Another influence was James C. Bonbright, a Professor of Finance at Columbia University, whose *The Valuation of Property* (Bonbright 1937) was to be quoted at length in the Edwards articles.

Edwards argues that orthodox business accounting has important functions but achieves them poorly, often because it either lacks clear principles or applies them inconsistently. One function is the measurement of income or profit. Edwards proposes an increased net worth concept of income: individuals or businesses would calculate their net worth at the beginning and end of a period, and income for the period would be the increase in net worth (adjusted for any value added to or withdrawn from the business during the period by its owners). Traditional profit measurement involves identifying revenues and expenses attributable to a particular period and is asymmetrical as regards unrealised gains and losses—gains are not normally included in the measurement of income until they are realised, while losses are included when they arise, whether realised or not. The latter is an application of the accountant's prudence principle. Edwards (1938: 290) claims that traditional accountants 'are deliberately abandoning the process of looking forward in favour of looking backward'. This is inconsistent with the *economic* reasons for measuring income, which Edwards sees as helping investors to decide whether or not to invest in a business and individuals to plan their consumption. For these purposes, net worth needs to be measured in terms of money value, which Edwards explains as the present value of future services: '[P]resent value is determined by the market's expectations of the quantity of services, [their] future prices, and the rate of interest' (*ibid.*: 46).

Edwards (*ibid.*: 138) concludes that ‘Published accounts should have as their object the provision of information for a judgment of net worth. The clearer and more relevant this information, the easier it is for the shareholder to calculate his income’. Edwards is clearly focusing on the information needs of investors in companies with stock exchange listings, and his writing signifies a move away from the more traditional notion that corporate financial statements are accounts of how a company’s directors and managers have preserved and employed the capital invested in the company, to justify the payment of dividends. His forward-looking approach for financial reporting meant that the three accounting problems mentioned in the introduction to this chapter—asset valuation, income measurement and cost determination—were reduced to one, namely the valuation of assets and liabilities on an opportunity cost (or what would subsequently be referred to as a value to the owner) basis.

The accounting reforms proposed by Coase and Edwards did not go uncriticised. Some of the most severe criticism came from within LSE itself. After de Paula had left in 1930, various part-time teachers provided accounting courses, including Stanley Rowland, who was one of Dicksee’s colleagues in his professional practice. Rowland was a chartered accountant and had also taken a law degree; he had been teaching accounting at the School since the mid-1920s. He wrote to *The Accountant*, while the Edwards articles were still appearing, to state his ‘most fundamental disagreement’ with Edwards, and later he described Edwards’ articles as ‘dangerous nonsense made the more dangerous by the fog of words in which assumptions are disguised as truths’ (Rowland quoted in Parker 1999: 25). Professional jealousy may be involved here, as Rowland, rather older than Edwards and Coase (and, indeed, Plant), possibly felt that the economists were trying to take over from the accountants not just at LSE but more generally. As Baxter was later to observe: ‘Rowland was in a very difficult position ... He wasn’t on good terms with his staff’ (Baxter 2005: 24).

The Plant group broke up because of the Second World War, with its members generally entering government service. Plant, Edwards and Coase returned to LSE after the war, but Coase emigrated to the USA in 1950. He was awarded the Nobel Prize in 1991, partly for his work at LSE in the 1930s (see Bertrand 2015). Plant himself retired in 1965. He had continued to co-ordinate a one-year course for students working in industry, commerce, finance and public service, and following his retirement, this formed the nucleus of LSE’s General Course. However, this was no longer an opportunity for young managers to gain exposure to academic thinking about business administration, but rather allowed undergraduates mainly

from outside the UK to study for a year at LSE (see Dahrendorf 1995: 498). Despite efforts by Edwards to develop links with business through his Seminar in Problems of Administration (see Arena and Minkes 2017), engagement with business education was not an LSE priority during the 1960s (see Dahrendorf 1995: 422).

5 The LSE Triumvirate

By the end of the Second World War, Rowland was still heavily involved in teaching accounting at LSE, but he needed additional colleagues. The first appointment was that of David Solomons, who was an LSE graduate (Plant was one of his teachers) and a chartered accountant. From 1942 to 1945, Solomons was a prisoner of war, when, ‘to relieve the monotony of camp, he began to teach accounting and economics to his fellow students’ (Accounting Hall of Fame 1992; see also Zeff 1995). This encouraged Solomons to seek a teaching position (originally part-time) in accounting at LSE in 1946. Unfortunately, Rowland died suddenly soon after the start of the academic year, and Solomons was left in sole charge of teaching accounting.

There had not been a professor of accounting since de Paula’s resignation in 1930. This was remedied by appointing William T. (‘Will’) Baxter to the first full-time chair in accounting not just at LSE but also in any British university (see Zeff 1997: 9). Baxter was no stranger to the School. Born in 1906, he gained the BCom degree of Edinburgh University and qualified as a chartered accountant. After graduating, he worked for Walter Annan, part-time Professor of Accounting at Edinburgh. Annan encouraged Baxter, who by now was teaching at Edinburgh University, to accept a Harkness Fellowship to study in the USA for two years, and for most of that time, Baxter worked at Harvard University on the business papers of John Hancock, one of the signatories of the American Declaration of Independence (see Baxter 1945). Returning to Britain in 1933, Baxter could not immediately secure an academic position, so he attended the courses of Robbins, Hayek, Plant and others at LSE. He became involved with the Plant group, and even after gaining a Lectureship at Edinburgh, he would return to the School in the vacations (see Accounting Hall of Fame 2005; Bromwich et al. 2006). Baxter has described attending Plant’s seminar for final-year undergraduates and how he was astonished ‘that these young men were so well grounded in Economics. Each week a student read a paper, and he would talk about the subject, and he’d go to the blackboard and

draw diagrams, as if he were a Professor of Economics' (Baxter 2005: 22). Baxter was also exposed to economic thinking at Edinburgh from his colleague Kenneth Boulding, who was later to bemoan the lack of 'intellectual intercourse between economists and accountants at the professional level' (Boulding 1977: 86).

In 1937, Baxter became Professor of Accounting at the University of Cape Town, at Plant's recommendation. Several of Baxter's colleagues at Cape Town were LSE graduates, including George Thirlby, an economics lecturer who had been taught by Plant. Thirlby 'constituted himself a kind of tutor' for Baxter: 'I was made to try and defend things like allocating oncost and writing off depreciation and historical cost, and so on, and it became impossible for me to go on accepting the accounting doctrines' (Baxter 2005: 26–27). Thirlby contributed to the opportunity cost literature while at Cape Town (see Thirlby 1946), arguing that the accountant's notion of cost was inconsistent with the use of costs for business decision-making, and he continued to apply economic ideas of cost to business decisions after his return to LSE in 1947 (see Thirlby 1960).

The third member of the LSE triumvirate was Harold Edey, who had qualified as a chartered accountant after leaving school and then begun to study for the BCom at LSE as an evening student. Service in the Royal Navy during the Second World War interrupted this, and Edey completed his degree after the war, attracting the attention of Frank Paish, who taught banking and finance. After Edey graduated, he registered as a research student under Paish's supervision, and from 1948, he taught an accounting course at the School. Baxter invited Edey to become a full-time lecturer in accounting and business finance, and he began work in 1949, boosted by his father's comment that 'a university lecturer's appointment was a "gentleman's position"' (Edey quoted in Bailey 2009: 63).

The contribution of the LSE Triumvirate to the development of price-change accounting has been documented in detail by Whittington (1994). In the late 1940s, prices in the UK rose rapidly to 60% or more above pre-war levels. The use of original costs as the basis of financial reporting created two problems. First, asset values could be significantly understated, especially if a business owned properties measured at pre-war prices. Secondly, replacement prices for resources such as raw materials used in making products for sale would exceed the original (historical) cost of these resources. This could lead to an overstatement of economic profit. For example, suppose that a business sells some goods for £150, where the original cost of the goods was £100. Traditional accounting would record a profit of £50. However, because of rising prices, it might cost £130 to replace the goods.

If the business paid out its full historical cost profits as a dividend, there would not be enough cash to replace the goods and the business would not be sustainable. The solution to both these problems was obvious to the LSE Triumvirate: adjust financial statements to reflect price changes. But how should such adjustments be made?

Adjusting financial statements for inflation had been a necessity in Germany because of the hyperinflation of the early 1920s, and German scholars had formulated the system now referred to as constant purchasing power (CPP) accounting. This had been extended by the US economist Henry Sweeney into stabilised accounting (see Graves 1991). The historical cost financial statements would be adjusted for general price inflation by restating every item into stable units of CPP. For example, if a company had a building purchased several years earlier at a cost of £1 million, and the CPP equivalent of this amount was £2 million, then the building would be reported at £2 million in the CPP balance sheet. However, the prices of particular assets may change at different rates from the overall average change in prices. For example, if property prices had grown rapidly, the building could actually be worth £3 million. Hence, the adjusted historical costs for assets shown in the CPP balance sheet and profit and loss account could differ significantly from their current value.

Reflecting current values in corporate financial statements was thus seen to be necessary. But how should current values be determined? The economic literature, to which the LSE's academics had contributed in the 1930s, suggested three main approaches. The first of these is replacement cost (RC), which measures the amount that the business would need to pay to replace assets sold or used during a period, or held at the balance sheet date. The second approach is to regard current value as the amount that a business would realise by selling an asset: this is sometimes referred to as net realisable value (NRV). The third approach is to estimate the future economic benefits that will be expected to flow to the business from its ownership of a specific asset, and discount these benefits to determine the net present value (PV) of the asset. However, Baxter and his colleagues realised that there were circumstances where one or more of these measurement bases would be inappropriate. RC made sense only if it would be both possible and economically desirable to replace the asset. There was also the problem of what actually was being replaced. Usually, older machines would be replaced with more modern models. To determine an appropriate RC, it would be necessary to exclude from the price of the replacement machine an amount for the additional services provided by the new and improved model in comparison with the existing partly used machine. Similarly, NRV made

sense only if it was economically appropriate to sell an asset rather than keeping it to generate future inflows.

The influence of the opportunity cost approach can be seen in the advocacy by the LSE Triumvirate of the value to the owner principle, sometimes referred to as deprival value. Suppose that a business that owns a specific asset wishes to use that asset (e.g. materials held in inventory) in generating revenues: What is the economic cost of using the asset? What alternative opportunity does the business sacrifice by using the asset? This will be the next best use of the asset, which will be the proceeds from selling the asset in its current state (NRV), or the present value of the cash inflows arising from using the asset in some other way (PV). The business will choose whether to sell or keep the asset depending on whether NRV is greater or less than PV. However, if the business can replace the asset at a low enough cost, it will still be able to achieve the alternative outcome, so the amount that the business sacrifices by using the asset in the planned manner is RC. Hence, value to the owner is determined as the lower of RC and the higher of NRV and PV (for more on value to the owner, see Macve 2010). In practice, for longer-lived assets, calculation of RC could be highly complex, and Baxter developed a technique for comparing cash flows that would arise if a firm retained (the 'have' budget) or were deprived (the 'have not' budget) of an asset (see Baxter 1975: 127). As Macve (2010: 114) notes, this approach to determining RC 'might be criticised as "too subjective"', as it relies on management estimates of future cash flows rather than observable replacement prices.

The overall system developed by the LSE Triumvirate was at a disadvantage compared with those of other accounting academics around the world because it did not appear in book form until the 1970s (see Baxter 1971, 1975). Alternative systems had been presented by Edwards and Bell in *The Theory and Measurement of Business Income* (Edwards and Bell 1961), Chambers in *Accounting, Evaluation and Economic Behavior* (Chambers 1966) and other authors. The Edwards and Bell study was influenced by Baxter (whose contribution is acknowledged in generous terms by the authors), and much of the work for the book was undertaken by Philip Bell during a visit to LSE in 1958 (see Napier 2011: 195). Baxter himself visited Columbia University around this time, one product of this being an article with Alfred R. Oxenfeldt, a marketing professor, comparing economists' and accountants' approaches to pricing (see Oxenfeldt and Baxter 1961). This article sets out the rationale for using opportunity costs for decision-making, such as determining selling prices, but concludes that the widespread use by business of cost-plus approaches (where the selling price is determined by

adding a markup to the calculated cost) suggests that they often provide reasonable approximations to a theoretically more accurate cost calculation.

Until the 1960s, there were few scholarly journals in the English language dealing specifically with accounting. Baxter and his colleagues found a dearth of relevant literature that they could use in their teaching, and they remedied this deficiency by putting together sets of readings. Baxter edited *Studies in Accounting* (first edition, 1950), collecting work by accounting practitioners, businessmen and academics, including a revised version of the Edwards (1938) articles. Solomons drew more substantially upon academics, both economists and accountants, in *Studies in Costing* (first edition, 1952b), including the Coase (1938) series of articles, and Solomons' own history of costing (Solomons 1952a). With LSE economist Alan Peacock, Edey wrote an introductory book *National Income and Social Accounting* (Edey and Peacock 1954), as well as the basic textbooks *Business Budgets and Accounts* (Edey 1959) and *Introduction to Accounting* (Edey 1963).

Many of the publications of the LSE Triumvirate appeared in professional accountancy magazines, giving their ideas only limited circulation. It can be difficult to identify the exact moment when specific economic ideas influenced Baxter and his colleagues. Baxter himself was to state that 'we must have been influenced by J.R. Hicks' book on value' (Baxter 2005: 31). This was a reference to *Value and Capital* (Hicks 1946). Hicks had been an Assistant Lecturer and Lecturer at LSE in the second half of the 1920s and the first half of the 1930s, but had moved to Cambridge in 1935. The LSE Triumvirate used Chapter 14, on 'Income', from this book as a guide to the way in which economists conceptualised income. Hicks gives a general definition of 'a man's income' as 'the maximum value which he can consume during a week, and still expect to be as well off at the end of the week as he was at the beginning' (ibid.: 172). This is a forward-looking or *ex ante* notion of income as a guide to consumption during a coming period, but Hicks is aware that the notion of well-offness needs to be operationalised. He does this through a series of approximations, first of all defining well-offness in terms of 'the capitalized money value of the individual's prospective receipts', then 'the maximum amount the individual can spend this week, and still expect to be able to spend the same amount [in money terms] in each ensuing week', and finally 'the same amount *in real terms* in each ensuing week' (ibid.: 172, 174; italics in original). He finally notes that, in practice, individuals (and indeed businesses) are interested in income *ex post*, what they have actually achieved during a particular period.

To Hicks (ibid.: 180), 'income is a very dangerous term' for the theoretical economist, while for practical purposes, 'it is not necessary to have

an exact definition of income; something quite rough, suitable to a rough practical precept [such as “living within one’s income”], will do quite well’ (ibid.: 181). However, the general notion of income as change in the present value of future cash flows was consistent with the ideas of Edwards (1938) already mentioned, while the notion of income as maintainable consumption made sense to business accountants who saw corporate accounting as determining profits to justify a steady stream of dividends. The recommendation to maintain income in real terms also fitted with the stress on reflecting price change in financial statements. Hence, Hicks’s ideas on income influenced academic accountants’ thinking. For example, an article by Solomons, ‘Economic and Accounting Concepts of Income’, refers specifically to the Hicksian income concept and attempts to reconcile accounting and economic income. Solomons endorses Hicks’s scepticism about the value of income measurement, speculating that ‘so far as the history of accounting is concerned, the next twenty-five years may subsequently be seen to have been the twilight of income measurement’ (Solomons 1961: 383). Students of accounting at LSE in the 1960s still remember the emphasis placed on the Hicksian concept of income in teaching at that time (see Stevenson et al. 2017: 10).

The Hicksian income approach has been acknowledged by the writers of conceptual frameworks for financial reporting as a key principle that should underpin corporate financial statements. There is a direct connection with LSE here, as Solomons, who emigrated to the USA in 1959, was one of the writers of the first US Conceptual Framework (see Accounting Hall of Fame 1992), which focuses on the balance sheet and on the recognition and measurement of assets and liabilities, rather than on the profit and loss account and the measurement of income. LSE-accounting Professors Michael Bromwich and Richard Macve (together with US academic Shyam Sunder) have investigated whether the accounting standard-setters have misunderstood Hicksian income, concluding that Hicks’s analysis ‘does not provide a conceptual basis for the [standard-setters’] exclusive focus on a balance sheet approach to financial reporting. Nor does it help address the difficult problem of measuring and reporting business performance and identifying drivers of value creation’ (Bromwich et al. 2010: 365).

Although the LSE Triumvirate had by the mid-1960s developed a coherent system of corporate financial reporting grounded in the economic income and opportunity cost concepts, by 1970 the international academic accounting mainstream was moving away from what was dismissed as armchair thinking in favour of empirical studies of accounting numbers and their influence on security prices. Although Baxter and his colleagues

were left behind by these developments (see Napier 2011), they had a lasting influence on the many accounting academics who studied and worked at LSE. From 1962, accounting formed a separate academic department at the School, but there was little accounting teaching: the LSE *Calendar 1964/1965* (LSE 1964) lists only five undergraduate courses, and students specialising in accounting studied more economics than accounting. As well as Baxter and Edey, there were two lecturers in 1964, Peter Bird and John Flower, with Bryan Carsberg listed as a Fellow in Management Studies. Carsberg had been appointed initially to the Management Studies Research Division, a short-lived research centre co-ordinated by Edey, but was to become a lecturer, studying at the same time for the MSc(Econ) degree in Accounting. As Dev (1980) and Wallace (1997) have shown, the first full-time accounting professors at many UK universities had LSE connections. For example, Flower became professor at Bristol University in 1969, Carsberg moved to Manchester in the same year, Bromwich moved to the University of Wales Institute of Science and Technology (UWIST) and Bird to Kent in 1970, with Macve going to Aberystwyth in 1978. Susan Dev remained at LSE: she was promoted to professor in 1979. All of these researchers used economic reasoning in their early research. For example, Flower (1966) applies economic analysis to critique overhead analysis methods in the context of a large government contract, Carsberg (1966) examines the valuation of business goodwill in terms of the alternatives available to a hypothetical purchaser, and Dev (1978) employs linear programming to calculate factor prices as opportunity costs.

Baxter retired in 1973 but continued to teach and research well into his 90s. Edey was heavily involved with the ICAEW's initiatives in accounting research and standard-setting. Meanwhile, both professional and governmental attempts to reform accounting in response to high rates of inflation in the early 1970s brought issues of asset and liability measurement and income determination to the fore. The UK government's Sandilands Committee on inflation accounting (Inflation Accounting Committee 1975) adopted a value to the business approach consistent with the LSE Triumvirate's position. This was not attributed to Baxter and his colleagues, who, unlike some other accounting academics, were not acknowledged or cited in the Committee's report. In his memoirs, Edey does not mention any involvement in submissions to the Committee from the professional accountancy bodies, though he discusses his role as a founder member of the ICAEW's Accounting Standards Steering Committee (see Bailey 2009: 77). However, the influence of LSE on Sandilands was probably more indirect, through the Committee's technical liaison officer, Chris Westwick,

an LSE-accounting graduate and part-time teacher. The subsequent development of price-change accounting in the UK has been discussed by Tweedie and Whittington (1984), but with the retirement of Edey in 1980, the period of the LSE Triumvirate was over.

6 Later Developments

Following Edey's retirement, Susan Dev became Convenor of the Department of Accounting, which changed its name to the Department of Accounting and Finance. As in many universities in the UK, a considerable amount of finance research and teaching at LSE took place in the Department of Economics and associated research centres, and the Department of Accounting and Finance concentrated on corporate finance and investment. In 1981, Bryan Carsberg was appointed Professor of Accounting, his Chair being supported by the international accounting firm Arthur Andersen. Carsberg had recently spent a period at the US Financial Accounting Standards Board, and he thought that the Department's staff needed to be more aware of the quantitative and statistical turn in accounting research in North America. However, in 1984, Carsberg became Director-General of the Office of Telecommunications (OfTel) and left LSE. Dev retired in order to concentrate on her work with the University of London's external programme for distance learning, so this left the School without an accounting professor for the first time since 1947. The Convenor's role was taken on by Martin Walker, who was a Reader in Accounting and an economist by training. He contributed an article on the economic theory of choice as applied to accounting standard-setting (Walker 1984) to the *Accounting Review* around this time. LSE obtained funding from the Chartered Institute of Management Accountants (CIMA) for a Chair in Management Accounting and from the accounting firm Ernst & Young (now EY) for a Chair in International Accounting and Financial Management.

The School was fortunate in appointing (in 1985) two of the most prominent accounting academics in the UK to these positions. Michael Bromwich had trained as a cost accountant before taking the BSc(Econ) at LSE, specialising in Industry and Trade. He had been a Lecturer in Accounting at LSE before moving to UWIST and later the University of Reading. Several of his academic publications were critical of the simplistic use of concepts, such as present value, in accounting (e.g. Bromwich 1977), while *The Economics of Capital Budgeting* (Bromwich 1976),

published by Penguin Books, was a widely read source of information on how economists thought about the allocation of scarce resources. Bromwich was active in the accountancy profession and was to be President of CIMA in 1987/1988.

The Ernst & Young Chair was filled by Anthony Hopwood, who had studied accounting at LSE—he and Bromwich were friends as students, sharing accommodation in one of the School's halls of residence—where he attracted the attention of Baxter. As one of the outstanding students of his time at LSE, Hopwood was encouraged by Baxter to pursue post-graduate studies in the USA, and he took an MBA and then a PhD at the University of Chicago. Returning to the UK, Hopwood worked at the Manchester Business School, Henley and Oxford before becoming Professor at the London Business School in 1978. Hopwood founded the journal *Accounting, Organizations and Society* in 1976 and was a central figure in the organisational, behavioural, social and critical branches of accounting research (see Baker 2011; Birnberg et al. 2013). At home in both quantitative (Hopwood 1972) and more discursive (Hopwood 1983) modes of research, Hopwood was a genuinely international figure, founding the European Accounting Association in 1976 (see Accounting Hall of Fame 2008; Hopwood 2002). He and Bromwich organised a series of conferences at which leading academics and practitioners would discuss current accounting research, with the papers later published in book form (e.g. Bromwich and Hopwood 1981).

From the 1970s, a growing focus for economists was the economics of information (see Strong and Walker 1987). Traditional economics tended to assume that market participants all had full knowledge, but economists were increasingly aware that they needed to analyse more realistic situations where information was partial and asymmetrical (e.g. managers of firms were likely to have more information about their firms than investors). Beaver and Demski (1979) had shown that, if markets were perfect and complete, everyone could use market prices to deduce all available information and had no need to base decisions on accounts. However, as markets are actually imperfect and incomplete, there is a potential role for accounts for both ex ante decision-making and ex post performance evaluation. However, Demski (1973) had previously argued that there was no way to arrive at a set of accounting principles, rules or standards that could be guaranteed to please everyone. Bromwich (1980) had written a rebuttal of Demski's argument, and soon after arriving at LSE, he published *The Economics of Accounting Standard Setting* (Bromwich 1985). This book showed, using economic analysis, the circumstances in which accounting regulation would be

appropriate, discussed the economic consequences of accounting regulation and compared the government as an accounting standard-setter with private-sector bodies.

Bromwich's interest in economic analysis of corporate financial reporting led to a further book, *Financial Reporting, Information and Capital Markets* (Bromwich 1992), while he has continued to apply economic analysis to management accounting. For example, with Martin Walker, he reviewed the residual income approach to performance measurement. Residual income is an economics-based measure, as it assesses an organisation's performance by deducting a cost of capital (interest on the opening value of the organisation's net assets) from the organisation's profits (see Bromwich and Walker 1998). Other accounting academics at LSE applying economic reasoning include Miles Gietzmann (1996), who used the notion of incomplete contracts to analyse how different accounting calculations can affect the relationship between organisations and their subcontractors. However, economic viewpoints of business management, strategy and accounting are often contrasted with more sociologically informed positions. For example, Alnoor Bhimani has observed that the more prescriptive strategy literature has often assumed that organisations 'engage in strategic choice making in an *economically rational* manner within the constraints of limited information, cognitive biases and causal ambiguity' (Bhimani and Langfield-Smith 2007: 6; italics added), while questioning the applicability of this assumption in practice. More recently, Wim van der Stede (2011) discusses prospects for management accounting research in the wake of the global economic crisis of 2007–2008, employing economic concepts such as incentives and information economics. His main message is that studies of organisation design and management accounting systems need to reflect both structure and culture.

In stressing the relevance of culture, van der Stede is firmly within one of the main LSE traditions of accounting research since the 1980s. The appointment of Anthony Hopwood introduced a relatively new mode of thought, where ideas from social, political and cultural theory began to challenge the previous hegemony of economics. Hopwood was comfortable with applying economic ideas to accounting, but as just one mode of research and analysis among several. In one of his last articles before his death in 2010, Hopwood bemoaned how, at a management accounting conference, 'Analytical and economic perspectives appeared to be more prevalent with fewer occasions when economics, organization theory, and sociology were intermingled and interrelated within a session' (Hopwood 2008: 4). Soon after their appointments, Hopwood and Bromwich presided over a

significant expansion in the academic accounting staff, and several appointees are now professors of accounting at LSE or other institutions. In addition to Alnoor Bhimani, appointments by Bromwich and Hopwood included Peter Miller and Michael Power. Miller was originally a sociologist, who pioneered the application of the ideas of French social theorist and historian Michel Foucault to accounting and organisation studies, with seminal contributions to both the sociology (Rose and Miller 1992) and the accounting literature (Miller and O'Leary 1987). Power obtained a philosophy of science doctorate before qualifying as a chartered accountant. He has become famous for developing the concept of the Audit Society (see Power 1997), showing how audit, monitoring and performance measurement and assessment have penetrated into all aspects of modern life. In 2000, Power co-founded LSE's Centre for Analysis of Risk and Regulation (CARR), where he has emphasised that risk is more than an economic concept and that understanding risk management requires a wide range of analytical perspectives (see Power 2007).

Whereas earlier accounting academics had often looked to economics to provide critiques of accounting practice, Hopwood, Miller and others argued that economics needed the calculative practices of accounting to achieve its influence on human life. Launching a new journal, the *European Accounting Review*, Hopwood not only noted how economic thinking had taken root in academic accounting thought, but also suggested that discordances between accounting practices and economists' recommendations were 'suggestive of the difficulties of economics ... For even though it claims to provide a positive rather than a normative knowledge of the world, economics seemingly always wants to make the world more economic than it is' (Hopwood 1992: 130). Miller (1991) has studied the relationships between national governments involved with economic planning and the adoption of specific methods of calculation, such as net present value, for management accounting decisions. He argues that earlier support for this approach, for example the articles of Ronald Edwards in 1938, was ineffective because endorsement for discounted cash flow was seen as 'an onslaught by economists upon accountants' (Miller 1991: 741). In the UK, wider adoption of discounted cash flow needed the support of the government, who argued that use of the method would lead to better economic decisions and hence would enhance economic growth. Specific accounting calculations become embedded in the management of national economies. To paraphrase Hopwood (1992: 131–134), economics was initially an idea, but not a fact: accounting calculations, the accounting eye, provided a new means for intervening in organisations and economies.

Both Miller's and Hopwood's studies were part of a research field emerging particularly at LSE in the late 1980s and early 1990s, the so-called New Accounting History (see Miller et al. 1991). Historical research in accounting had long been associated with the School, particularly through the work of Basil Yamey. A South African by birth, Yamey had been taught by Baxter at the University of Cape Town before coming to LSE in the early 1940s, where he specialised in the economics of international trade and development. Yamey (1949, 1964) used economic theory to rebut the claims of certain economists that double-entry bookkeeping was necessary for capitalism, and also examined the double-entry ledgers of early traders through an economic lens to show that the information in the ledgers was of little value for economic decision-making (see Yamey 1959). With a US accounting professor, A.C. Littleton, Yamey edited *Studies in the History of Accounting* (Littleton and Yamey 1956), which included historical work by Baxter and Edey. The interest of the LSE Triumvirate in accounting history meant that Yamey remained in close contact with the accounting academics even after the School was split into separate academic departments in 1962. In the late 1970s, Yamey taught a course on the MSc in Accounting and Finance, in collaboration with Leslie Hannah of the Business History Unit, in business and accounting history. The New Accounting History regarded much previous historical work in accounting as either atheoretical or excessively dominated by neoclassical economic reasoning, and Yamey in particular was to come under criticism for a 'narrowing and delimiting of the domain of accounting history' (Miller and Napier 1993: 636). The vehemence with which the New Accounting History rejected economics has perhaps inhibited historical accounting research using econometric methods, which was certainly seen as a possible research direction in the late 1980s (see Napier 1989).

7 Conclusion

As an academic discipline with deep roots in professional practice, accounting teaching has always had to balance the need to provide an effective technical education with the desire to expose students to a range of conceptual analyses that provide insight into, and also a critique of, what accountants actually do. Whereas the first generation of accounting academics at LSE (Dicksee, de Paula and later Rowland) saw law as the defining discipline for accounting, the second generation (Baxter, Solomons and Edey) drew on the ideas of economics being developed at the School in the 1930s by such

economists as Robbins, Hayek and Hicks and diffused by Plant, Coase, Edwards and Fowler into a criticism of existing financial and managerial accounting practices. The economic analysis of value, income and costs developed and taught by the LSE Triumvirate influenced the next generation of British accounting academics, but by the 1970s the School was no longer part of the international accounting research mainstream, as influential North American scholars did not respect conceptual analysis as a significant contribution to accounting research (see Napier 2011). Instead, capital markets research into how accounting information was apparently used by investors became dominant, and this meant the use of large data sets and econometric analysis that the LSE-accounting academics were not trained to use.

Today, some LSE-accounting academics work in this field: for example, Peter Pope has examined the impact of international accounting standards on investor and creditor decision-making (see Florou et al. 2017), while Bjorn Jorgensen uses sophisticated economic analysis (drawing on game theory and bargaining theory) to show how conservative profit measurement induced by regulations may affect the willingness of businesses to abandon projects (see Chen and Jorgensen 2016). However, the current worldwide reputation of the School's Department of Accounting is based on the more sociological research identified with Anthony Hopwood and his successors. Economics is still important to this research, but the focus is no longer on how economics can reform accounting practice. Instead, as Miller and Power (2013: 558; italics in original) put it, accounting is studied as 'a mechanism by which the *economization* of organizational life becomes elaborated and institutionalized'. So, whereas Yamey the economist denied a significant role for accounting in the development of capitalism, Miller the sociologist and Power the philosopher-accountant put accounting at the heart of the modern calculative society.

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4

Business History at LSE: An Empiricist Voice

Leslie Hannah

1 Introduction

The relationship between empirical and theoretical work in economics has always been contentious. In the nineteenth century, the German Historical School claimed to be more grounded in economic experience than the abstractions of Alfred Marshall's neoclassical marginalism, and Sir John Clapham, doyen of British interwar economic historians, complained of the 'empty economic boxes' of Marshallian economic theory (Clapham 1922).¹ Today, free market economists like Paul Romer are worried about colleagues' airy dismissal of identification problems behind a fog of deep mathematical theory concealing falsifiable model assumptions. His accusations of professional hypocrisy tellingly mesh with the criticisms of

¹For an authoritatively global survey of business history as an academic discipline, locating it firmly within this tradition, see Kipping et al. (2016).

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fact-free economics by leftist political economists like Ha-Joon Chang (Romer, forthcoming; Chang 2010). In fact, at the time of Clapham's similar early complaint, Marshall was renowned as the author not just of *Principles of Economics*, but also of *Industry and Trade* and *Money, Credit, and Commerce*.² The latter displayed his intimate knowledge of recent industrial and banking developments in Germany, the USA and UK, though today's industrial and financial economists might classify them as business history or business journalism, not economics. Nineteen-fifties Cambridge had already, for some tastes, left that empirical strand woefully behind, when an industrialist alumnus (the Chairman of Unilever)—frustrated at the lack of realism in the imperfect competition models touted by Joan Robinson and others—commissioned Cambridge historian Charles Wilson to write a corporate history, strengthening that strand in the evolution of business history.³

2 The Beginnings of Empirical Business Economics

In the first seventy years of LSE economics, there were several drivers towards developing more systematic empirical work on business of the kind which fostered the development of business schools (notably in the USA), technically orientated *grandes écoles* (in France) or the discipline of *Betriebswirtschaftslehre* (in Germany) (see Rocke 1984; Tribe 1995: 95–139). LSE's founders' first statement of aims included studying the 'concrete facts of industrial life' (quoted in Dahrendorf 1995: 20) and in its difficult early years, the School's socialist sponsors, struggling to find revenue streams to support their new institution, offered part-time evening courses for railway and bank clerks and other aspirants to managerial promotions. LSE was a business school in all but name: with accounting, scientific management and railway economics among the subjects in high demand and evening part-time degrees dominating (see Dahrendorf 1995; *The Economist* 1918). Attracting Rockefeller and other funding, William Beveridge, LSE's Director from 1919 to 1937, turned it into Britain's leading research centre in the social sciences, with a budget increased sevenfold, permitting more

²Published in 1919 and 1923 respectively.

³This resulted in three volumes by Wilson: *The History of Unilever: A Study in Economic Growth and Social Change* (Wilson 1954, two volumes) and *Unilever, 1945-1965: Challenge and Response* (Wilson 1968). A fourth and fifth were later completed by one-time members of the BHU: *Fifty Years of Unilever, 1930-1980* (Reader 1980) and *Renewing Unilever* (Jones 2005).

academic freedom to theorise, but still rooted in the real world. He wanted social sciences that were applicable but analytical rather than purely descriptive. The BCom degree was initiated in 1919, with funding from the City Financier Sir Ernest Cassel, as more appropriate for those intending business careers than the BSc(Econ). One of its first students, Arnold Plant (see Plant 1974; Coase 1994: Chapter 12), succeeded to the Professorship of Commerce in 1930 and became a key figure, developing the popular undergraduate BCom course and a small business-funded postgraduate business administration course.

The Economic History Department partly reflected these developments.⁴ Its first professor was Lilian Knowles, who also served as Dean of the Economics Faculty until 1924. Her Tory imperialism meshed with Fabian views on the potentially beneficent role of government and her much reprinted textbook on 'the influence of the ideas of liberty, equality and fraternity, combined with machinery, railways, telegraphs and steamships' on nineteenth-century British business now looks surprisingly modern in exploring links between political and economic institutions (see Knowles 1921 and subsequent editions; Berg 1992). Under the later sway of R.H. Tawney and Eileen Power, the business focus switched to earlier periods, notably Power's Ford Lectures on the medieval wool trade (Power 1941). Tawney's classic work on the relation of Protestantism to the rise of capitalism interested Weberian sociologists more than economists (Tawney 1926). His ignorance of the nature of management was all too evident in his prescriptions on nationalisation (contributing flaws to the Labour Party's preparations for government) (Tawney 1920, 1937) and some of the best historical work on modern business history was in the Accounting Department, not in Economic History.⁵ T.S. Ashton, Tawney's later colleague as Professor of Economic History, was more sympathetic to business. He was co-opted by Friedrich Hayek—defender of classical liberalism and believer that the New Deal and Keynesianism would lead to economic ruin and political totalitarianism—to what at the time many considered a forlorn defence of capitalism's historical performance.⁶

⁴See the chapter in the current volume on LSE and economic history by Colin Lewis.

⁵Compare, for example, the published work on joint-stock companies of the Fabian economic history lecturer, H.A. Shannon, who favoured German-style regulation to prevent excessive corporate risk-taking (Shannon 1932), with the—at the time—unpublished theses of Alan Essex-Crosby (1937) and James B. Jefferys (1938 thesis, later published as Jefferys 1977), exploring the creative roles of capital markets. See also Hill (1950).

⁶Hayek (1954) was largely an attack on socialist history as empirically faulty. Hayek was at LSE from 1931 to 1950 and Chicago from 1950 to 1962 (see the chapter in the current volume on Hayek by Boettke and Piano).

LSE had Hayek, but no one of the stature of another Austrian émigré economist, Harvard's Joseph Schumpeter, in allying theory with empirics and history to produce new appreciative (informal) theories, inspiring the establishment of the Harvard-based Research Center in Entrepreneurial History⁷ (see McCraw 2007: 472–474). However, two came close. Edith Penrose, an American refugee from both McCarthyism in her home country and the Baathist Revolution in Iraq, was at LSE for only five years.⁸ Her *Theory of the Growth of the Firm*, published on her arrival, though castigated for its imprecise formulation by some economists, later became an inspiration for many business historians, as well as for developers of the resource-based theory of the firm. Before moving in the opposite direction across the Atlantic, Ronald Coase spent nearly twenty years at LSE, starting as a BCom student of Plant's, taking no economics department courses and specialising in his final year on works and factory accounting, cost accounting and financial accounting (as he later recalled, 'a circumstance which I believe gave me a freedom in thinking about economic problems which I might not otherwise have had' (Coase 1990: 3)). Plant encouraged Coase to deepen his understanding by visiting businesses and thinking about institutions (Charles Bruce-Gardner at the Bank of England gave him introductions to US industrialists and businessmen when he was awarded an LSE travelling scholarship). He was appointed Lecturer (and subsequently Reader) at LSE from 1935 to 1950, producing his important work 'The Nature of the Firm' (which began life as an LSE undergraduate project) and some company histories, including one on the development of the BBC (Coase 1950; see also Medema 1994: 1–21), precursors to the second strand of his life's work (on the problem of social cost).⁹ In 1991, he was awarded the Nobel Prize (which must be unusual even for *revised* undergraduate essays), but a better life than Britain's post-war austerity beckoned. In 1950, he turned down an LSE Chair to emigrate to the University of Buffalo (see Howson 2012:

⁷Housed at Harvard, this was essentially an informal group of interested Cambridge and nearby professors with some project staff and junior visitors, attending a ('post-cocktail') seminar and publishing the journal *Explorations in Entrepreneurial History*. It was mainly funded by a Rockefeller grant of \$230,000, coordinated by Arthur H. Cole and others (see Crandall 1960).

⁸She left the USA for Australia and Iraq to study the oil industry, with her British husband, after defending her Johns Hopkins colleague, Owen Lattimore, falsely accused by McCarthy of being a Soviet spy; the couple then fled the Baathist Revolution in Baghdad to London in 1959. She held a joint University of London Readership at SOAS and LSE from 1959 to 1964, spending her later career at SOAS and INSEAD. Her *Theory of the Growth of the Firm* was first published in 1959.

⁹Coase's later work on whether lighthouses are a public good remains a masterpiece of applied British business history (Coase 1974). For a fuller list of his writings in business history, see Landes et al. (1983).

720). Later moving to Chicago, he promoted empirical work as editor of the *Journal of Law and Economics* between 1964 and 1982, establishing its influential prominence, still holding somewhat aloof from the ‘blackboard economics’ (Coase quoted in Medema 1994: 11; see also 156–160) of Chicago’s mainstream.

An LSE professor at the time wrote of the academic divide between ‘monks’ (who pursued pure theory) and ‘technocrats’ (who wanted to engage with the world). The original LSE had been very much a world of technocrats, but by the 1970s the monks had definitely taken over many departments. The Economics Department, under Robbins, had eclectic tastes, though in the post-war years the BSc(Econ) increasingly drove out the more practical BCom degree (see Dahrendorf 1995: 417–422).¹⁰ Ronald Edwards (another of Plant’s students) and Harry Townsend in the Economics Department continued the empirical tradition. Edwards worked with Coase on how accounts could be used not only as retrospective tallies but also to provide quantitative information informing both economic analysis and business decisions about the future.¹¹ He also ran a remarkable 452 seminars between 1946 and 1973 in which businessmen presented papers on how they ran their organisations (its records still remain a rich historical source). However, Edwards was only a part-time professor from 1957, when he became Deputy Chairman of the Electricity Council (and Chairman from 1961), using his experience in industrial economics to drive transformations in off-peak pricing policy and productivity improvements in the nationalised electricity industry; he was later (1968–1975) Chairman of Beecham, the pharmaceutical giant.¹² The enthusiasm of the political and business worlds for co-opting thoughtful academic technocrats like Plant and Edwards delivered them knighthoods, but meant their empiricist drive became a somewhat muffled undertone in the School.

¹⁰Lord Robbins (1971: 126–127) notably praises Plant, Coase, Yamey and Edwards (as well as more obvious theoretical giants like Hicks and Hayek), yet fails to mention Tawney, Power, Ashton or Penrose (though he praises the labour historian/economist, Henry Phelps-Brown and the economic historian/development economist Arthur Lewis). Both Beveridge and Robbins successfully opposed the use of the Harvard case method, except for short executive courses (Howson 2012: 221–222).

¹¹See Coase (1973), a shortened, compiled version of a series of 12 articles which appeared in *The Accountant* between October–December 1938, and for Coase’s later reflections on the benefits of interaction between accountants and economists, based on his LSE experience, see his ‘Accounting and the Theory of the Firm’ (Coase 1990). For his other articles on public utility pricing, see Landes et al. (1983).

¹²Some were published in Edwards and Townsend (1958, 1961, 1966). A full set of the 452 papers delivered in 1946–1973 is in the LSE Archives, Reference MISC 332.

3 The Business History Unit: Mark I

In some universities, such initiatives had led to the formation of business schools, but LSE economists had waning interest in the empirical tradition and Edwards himself—as is sometimes the way with working class lads who succeed in the corridors of power—could be unhelpfully arrogant towards junior economists from more privileged backgrounds. The die was cast when LSE and Imperial College agreed in 1965 to sponsor a new business school in Regent's Park and the London Business School, which became Britain's leading such school, was born, soon forging independence from its sponsors; in 1966, LSE dropped its own Business Administration degree. LSE's Economics Department aspired to be at the cutting edge of theoretical economics and was impressively successful. Some considered the Department's more empirical past an unnecessary diversion in a world where more generous public funding now rewarded a different style of work. Elsewhere, economics and business school education were sometimes more comfortably integrated, arguably to the advantage of economics, notably in financial economics. This was not then strongly developed at LSE¹³ and an alternative venue where it might have been, the Accounting Department, also failed to nurture this financial economics strand (see Napier 2011; see also the chapter by Napier in this volume).

Yet still in the 1970s there was some receptivity to applied and interdisciplinary work. Many academics and businessmen had discussed initiating a research and teaching initiative in business history, but the prime catalysts for a new initiative in 1977 were Professor Theo Barker, recently appointed to the LSE Chair of Economic History and a consummate academic entrepreneur (in a profession which undervalued that skill) and Sir Alastair Pilkington (the inventor of float glass which had transformed his firm into the global leader).¹⁴ Barker persuaded Sir Alastair to spearhead a successful appeal to the business world for funding, orchestrated by a pioneering UK university fund-raiser, Jennifer Pinney. LSE and Imperial College were to be the host institutions of what was unpretentiously named the Business History Unit (hereafter BHU): when £229,000 had been raised, they advertised the post of Director. Leslie Hannah, a University Lecturer

¹³Its Financial Markets Group was not established until 1987.

¹⁴They had become acquainted while Barker was writing Barker (1976) which, amongst other things, clarified that Sir Alastair, a Cambridge-trained engineer, was no relation to the family which founded and still owned the firm.

at the University of Cambridge (teaching economics and statistics for historians) and financial tutor at Emmanuel College, was then working on British electric utilities under public and private ownership (published as Hannah 1979, 1982a). He was appointed from a strong field, though the School's Director, Ralf Dahrendorf, sternly warned all those shortlisted (who had tenured jobs elsewhere) that the post was untenured, with extension beyond seven years dependent on financial as well as academic performance. Hannah's Oxford economics PhD thesis on interwar mergers in British manufacturing had been supervised by one of the founders of the resource-based theory of the firm, George Richardson.¹⁵ Amended and published as *The Rise of the Corporate Economy*, it had already become a staple text on business courses (unusually for a PhD, or for any business history book outside Japan, it eventually sold tens of thousands of copies).¹⁶

Hannah took up the post in August 1978 in the newly refurbished Lionel Robbins Building at LSE, next to Michio Morishima's new Suntory and Toyota International Centres for Economics and Related Disciplines (STICERD),¹⁷ which shared an aspiration to encourage interdisciplinary work. Both timidly explored the research and administrative potential of revolutionary (now museum-piece) microcomputers with 8-inch single-sided, double-density discs. Hannah (apart from a year's leave at Harvard Business School in 1984/1985) occupied the BHU Director's post for its first ten years. Two more core posts funded by the Unit followed. Geoffrey Jones was recruited in 1979 as a Research Officer from Cambridge, where

¹⁵Richardson was then writing his seminal (Richardson 1972). The second supervisor was John Wright (who doubled as economist and economic historian) and his external examiners were G.C. Allen (pioneer management researcher and student of Japan's rise) and Aubrey Silberston (an industrial economist specialising in patents and monopolies). As an undergraduate historian, Hannah had been strongly influenced by his tutor, Keith Thomas (both Hannah and Paul Johnson in LSE's Economic History Department later contributed to the Festschrift for this pioneer of using anthropological insights to understand early modern society and beliefs, who inspired work far beyond his own specialisms). John Kay, an Edinburgh mathematics graduate and contemporary postgraduate economics student at Nuffield and St John's, was also a formative influence: he and Hannah had co-authored *Concentration in Modern Industry: Theory, Measurement and the UK Experience*, which showed the emptiness of the variance of logs as a measure of concentration, located the Herfindahl-Hirschman Index as an arbitrary point on a theoretical continuum and demonstrated that mergers (not the Gibrat effect) mainly accounted for Britain's unusually high levels of concentration. Between Oxford and Cambridge, Hannah had been an economics Lecturer in the remarkably young department fostered by Tony Atkinson and Christopher Bliss at Essex University, most of whose members became Ivy League economics professors.

¹⁶*The Rise of the Corporate Economy* was published in 1976 (Hannah 1976a), with various subsequent hardback and paperback editions, including a Japanese translation by Takeshi Yuzawa entitled *Dai Kigyo Keizai no Kouryu*, which appeared in 1987.

¹⁷See the chapter in the current volume on Morishima by Naoki Matsuyama.

he had been a college Research Fellow in history. His brief was to develop work on multinationals, where his early work in both government and company archives convinced him of the need to understand the politics of international diplomacy as well as business strategies (Jones 1981; Bostock and Jones 1989). His major project on multinational banking was initially funded by HSBC (Jones 1986, 1987).¹⁸ A year later, Jonathan Liebenau was recruited from Thomas Hughes's well-regarded history of science and technology PhD programme at the University of Pennsylvania. With an eye on the joint BHU sponsorship of Imperial College, Liebenau's brief was to drive initiatives on science-based industries. In particular, he cast a sceptical eye on the ethics of interactions between the pharmaceutical business and the medical community (e.g. Liebenau 1984, 1987, 1988a, b).

These core appointments gave the BHU an unusually youthful leadership profile (Hannah, Jones and Liebenau, when offered appointment, were, respectively, 30-, 26- and 25-years-old), but they were balanced by a steering committee and academic management committee of distinguished economic historians, social scientists and businessmen, averaging twice their age.¹⁹ These advisers were remarkable in their solid support and probing questioning, but refrained from meddlesome interference; they also used their influence to support a strategy of diversifying funding so as to ensure that no one sponsor could threaten academic independence.²⁰ The representation of other universities (Cambridge, Glasgow, Kent, Manchester, Oxford, Southampton and Glasgow) on the BHU's managing committees cemented its stated ambition to act as a national centre (BHU 1979: 3; a collection of the BHU's annual reports is deposited in the LSE Archives).

¹⁸Major synthetic works on the subjects that Jones made his own included Jones (1988, 1993, 1996, 2000) and Hertner and Jones (1986).

¹⁹At various stages, they included, from academia, Leslie Pressnell, Sir Douglas Hague, Donald Coleman, Rupert Hall, Dorothy Wedderburn, Basil Yamey, John Smith, Ben Roberts, Harold Edey, Susan Strange, Fred Halliday; and, from outside, the Labour politician Edmund Dell, freelance writer William Reader, and businessmen including Sir Peter Parker of British Rail, Sir Donald Barron of Rowntree, Sir Arthur Knight of Courtaulds and the National Enterprise Board, Sir Michael Caine of Booker McConnell and Sir Anthony Part of Orion Insurance.

²⁰On two exceptional occasions, the Chairman of a company sponsoring research attempted to censor or change results intended for publication. One complained of low-quality research and an internal enquiry agreed he was right: the wrong was remedied by redoing the work (whose extra costs the company generously and voluntarily funded). Another threatened to initiate a libel suit that would bankrupt the researcher, if conclusions that he (wrongly) considered unsupported were not excised. The BHU Director, who had good press contacts from his time on a student newspaper, informed the complainant that the offending results would be published regardless and advised him to contemplate that his crude blackmail might be exposed in *The Sunday Times* the following weekend. No more was heard of the complaint, the offending report was published uncensored, and no more funding was received.

This was strengthened when Professor Sheila Marriner and her Liverpool University colleagues, who had founded and for twenty years edited the journal *Business History*, asked the Unit to take over its editorship and management. The Business Archives Council also invited the Director to join its board and the Pasold Research Fund (which financed research and published the journal *Textile History*) transferred to LSE trusteeship—first under the direction of Negley Harte (of University College, London) and later of Mary Rose (Professor of Entrepreneurship at the University of Lancaster)—creating synergies. The Economic and Social Research Council (ESRC) reinforced the national orientation with some major funding decisions. The Unit's Director was in 1980 asked to spend a month visiting US business schools to report on what could be learned from their experience about expanding the role of business historians in UK business education (see Hannah 1991). There followed new ESRC-funded posts in business schools, studentships for PhDs within the Unit and in 1981 a joint conference with the London Business School exploring the potential for business historians researching and teaching business.

The ESRC also financed a major project, the *Dictionary of Business Biography* (Jeremy and Shaw 1984–1986),²¹ which not only provided a database for prosopographical research on entrepreneurs and managers but involved a wider range of established business historians in the work of the BHU through service on advisory panels and authorship. This was the Unit's first externally funded project, enabling the appointment from 1980, of David Jeremy as Research Fellow and editor. After brief spells at Princeton, the Smithsonian, Delaware and the Merrimack Valley Textile Museum, he had returned to the UK as Head of the History Department at Southend-on-Sea Comprehensive School, completing a self-funded LSE PhD in 1978 on American technological borrowing, supervised by Professor Charlotte Erickson.²² Moving to the BHU, he recruited a harmonious and talented team which, in a field notorious for missed deadlines, completed the six volumes of the *Dictionary* to time and on budget in 1984–1986, covering 1181

²¹This funding preceded the Director's appointment as Chairman of the ESRC's Economic and Social History Committee and membership of the Industry and Employment Committee. Unaccountably, the ESRC later refused funding for the data analysis which it was intended should follow, but for initiatives that made up for this (see Jeremy 1984, 1990 and Section 5 below).

²²His PhD was published by MIT Press as *Transatlantic Industrial Revolution: The Diffusion of Textile Technologies Between Britain and America, 1790–1830*, and in 1981 received the Dexter Prize of the Society for the History of Technology and the Dunning Prize of the American Historical Association. Jeremy later coedited Farnie and Jeremy (2004).

individuals.²³ The *Dictionary* was emulated by many business historians in other countries, though the rich internationally comparative data that now exist have stimulated little comparative analysis (see, for example, Slaven and Checkland 1986; McNeill and Unterburger 1987; Kurgan-van Hentenryjk et al. 1996; Villanueva 2000; Daumas 2010; Foreman-Peck et al. 1998; Nuvolari et al. 2016; Tortella et al. 2010).

4 New Developments

A younger generation brought in by the *Dictionary* and other research projects added to the atmosphere of youthful ferment. The roster of several dozen PhD theses in business history and related subjects completed within the Economic History Department at the School from 1978 onwards—some with financial or logistical support from the Unit or supervised by its staff—also included a significant portion of those who later developed the subject at other universities and business schools,²⁴ as did some of those

²³The *Dictionary* received a commendation from the judges of the Colvin Medal for reference works, and many contributors further strengthened the business elements of the *Oxford Dictionary of National Biography* (published by OUP from 2004 onwards). The most prolific contributors were Richard Davenport-Hines (64 entries), David Jeremy (51), Christine Shaw (50), Geoffrey Tweedale (29) and Robin Higham (25). Richard Davenport-Hines's larger study *Dudley Docker: The Life and Times of a Trade Warrior* was published by Cambridge University Press in 1984 and won the Wolfson Prize. Geoffrey Tweedale's later studies—*Steel City: Entrepreneurship, Strategy, and Technology in Sheffield, 1743–1993* (1994) and *Magic Mineral to Killer Dust: Turner & Newall and the Asbestos Hazard* (2000), both published by Oxford University Press—twice won the Business Archives Council's Wadsworth Prize. Useful by-products of the *Dictionary*'s systematic sampling included lists of the largest employers in 1907, 1935 and 1955 (see Shaw 1983; Johnman 1986; Jeremy 1990).

²⁴The Department's PhDs included (in alphabetical order, with their later university affiliations in brackets): Tony Arnold (Essex, Exeter and Leicester), Gerben Bakker (Essex and LSE), Rosineida da Silva Bentes (Pará, Brazil), Andy Bielenberg (Cork), Michael Aldous (Belfast), Sergio Birchal (FGV Rio de Janeiro), Susan Bowden (York), Gordon Boyce (Newcastle NSW), Carlos Brando (Bogotá), Rajeswary Brown (Royal Holloway), Francesca Carnevali (Birmingham), David Chambers (Judge Institute, Cambridge), Martin Chick (Edinburgh), Chris Colvin (Belfast), Harold Dutton (Lancaster), Roy Edwards (Southampton), Anthony Gandy (London Institute of Banking and Finance), Andrew Godley (Reading), Eric Golson (Warwick), Francis Goodall (BHU), Regina Grafe (North-western and EUI Florence), Nicolas Grinberg (Buenos Aires), Naveed Hasan (Lahore), Richard Hawkins (Wolverhampton), Peter Howlett (LSE), David Jeremy (Manchester Metropolitan), Lewis Johnman (Westminster), Terrence Lapiere (Wharton Business School), Giuliano Maielli (Queen Mary London), Gregory Marchildon (Johns Hopkins and Toronto), Ulrich Marsch (Munich), Helen Mercer (Greenwich), Ioanna Pepelasis Minoglu (Athens), Carlo Morelli (Dundee), Timo Myllyntaus (Turku), Sarah Palmer (Queen Mary London and Greenwich), Natacha Postel-Vinay (Warwick and LSE), Duncan Ross (Glasgow), Catherine Schenk (Glasgow and Oxford), Max-Stephan Schulze (LSE), Hiroshi Shimizu (Hitotsubashi), James Simpson (Carlos III Madrid), Peter Sims (Warwick and LSE), Anna Spadavecchia (Reading), Toshio Suzuki (Tohokudai), Kevin Tennent (York), Nick Tiratsoo (Luton), Adam Tooze (Cambridge and Yale), Ali Tunçer (UCL), Geoffrey Tweedale (Manchester Metropolitan), Maki Umemura (Cardiff), Aashish Velkar (Manchester), André Villela (Rio de Janeiro),

from the Unit's Imperial College sponsor.²⁵ As one of the latter remarked retrospectively, 'the BHU was a model for the serious study of 20th century science and technology'.²⁶

The first conference the Unit held (in 1979) was on 'Economic Theory and Business History', and, significantly, a later authoritative survey noted that a major differentiator of the British tradition of business history from that in the USA, Germany or Japan was a distinct emphasis on economic models and approaches, demonstrated by a comparative content analysis of national journals (Lamoreaux et al. 2008). But other social sciences were not neglected: political scientists and historians were encouraged to explore business dimensions.²⁷ The 'Edwards Seminar' tradition (with papers by leading businessmen analysing their experience) was revived under the chairmanship of Sir Arthur Knight (formerly head of Courtaulds and the National Enterprise Board). The first academic visitors in 1979 were Shin-Ichi Yonekawa, of Hitotsubashi University (the 'LSE' of Tokyo), William Reader, the doyen of freelance British company historians,²⁸ and Howard Gospel, an LSE PhD in industrial relations, then lecturing at the University of Kent. Gospel organised a BHU conference exploring the desirability of labour historians taking more cognisance of managerial policies. This promoted a broader movement to enrich the industrial relations mainstream with an

Kazuo Wada (Nanzan and Tokyo), James Walker (Henley), Lorna Weatherill (St Andrews), Leonardo Weller (São Paulo), Timothy Whisler (St Francis College Pennsylvania), Jong-hyun Yi (Gachon) and Nuala Zahedieh (Edinburgh). In addition, four Cambridge PhD students were supervised in the BHU by special arrangement: James Bamberg (Cambridge and Warwick), Wayne Lewchuk (McMaster), Ratna Sudarshan (Delhi) and Steven Tolliday (Leeds). Peter Scott (Reading) and John Singleton (Wellington and Sheffield Hallam) did Master's at LSE and PhDs elsewhere. The Unit's first administrative secretary, Shirley Keeble, completed an LSE PhD part-time in 1984. Some PhDs chose careers in government, consulting, finance or as entrepreneurs: Robin Cohen joined London Economics, a business consultancy established by LSE professors and others, eventually becoming its Managing Director, while David Kynaston became a freelance writer, his multi-volume history of London as a financial centre combining fine historical scholarship with the readability of a trade book (for more than a decade he vied with Michael Porter on the bookshelves of senior executives I visited).

²⁵The BHU's supporters included Rupert Hall in the History of Science Department and Aubrey Silberston and Dorothy Wedderburn in the Department of Social and Economic Studies. Among Imperial College PhDs, John Hendry converted to business history at the London Business School under the BHU/ESRC initiative and subsequently ran the Cambridge Judge Institute of Management MBA and was Dean of the Reading University Management School (publishing on general management and business ethics) and Mari Williams, after working in the BHU, transferred to the BP corporate history team in Cambridge.

²⁶Email from David Edgerton to the author, 12th December 2016. Among his publications as a visitor to the Unit was Edgerton (1984, 1987, 1988).

²⁷The 1980 BHU conference is reported in Turner (1984).

²⁸His commissions, among many others, included Reader (1970/1975, 1976, 1979, 1981).

understanding of personnel management techniques being developed on the employer side,²⁹ which was also a theme in a project on the growth of UK occupational pension plans, funded by Legal & General Insurance (see Hannah 1986a).

The BHU naturally had a British focus, but its members participated in international conferences developing institutional comparisons and the Unit welcomed overseas visitors and liaised with similar foreign initiatives, such as that of Franco Amatori at Bocconi University in Milan, or Maurice Lévy-Leboyer and Patrick Fridenson at the Maison des Sciences de l'Homme in Paris. Links were particularly strong with Japan, where the subject had more practitioners than anywhere else,³⁰ and the Fuji Conferences were a venue for global discussions (see Hannah 1980; Jones 1984). Leading Japanese scholars visited the BHU, including, in addition to Professor Yonekawa, Takeshi Yuzawa (Gakushuin University) and Yoshitaka Suzuki (Tohoku and, later, Hitotsubashi).³¹

The international star of business history at that time was Alfred D. Chandler, who had moved from Johns Hopkins to become Professor of Business History at Harvard Business School. He had promoted a more analytical and thematic approach in a subject with strong tendencies to the narrative and antiquarian, distinctively convincing some economists, sociologists and business strategists that historians had useful things to say. His first key work, *Strategy and Structure*, showed how the strategy of diversification had been managed by innovating the multidivisional organisational form: its original insights are still largely validated by modern research (Chandler 1962; Whittington and Mayer 2000). His *Visible Hand* was a key to understanding US corporate development in the era before 1960 and contributed to a renewed interest among economists in Coasean transaction costs (Chandler 1977; Williamson 1985). There was initial enthusiasm for Chandler's work in the BHU: the Director had many productive discussions with him since first meeting Chandler (then visiting All Souls, Oxford) in

²⁹See Gospel and Littler (1983). Littler was then a Research Officer at Imperial College, and among those attending were Hugh Clegg from Oxford, Jonathan Zeitlin from Birkbeck, Joseph Melling from Glasgow, Heidrun Homburg from Bielefeld, Wayne Lewchuk from Canada and Reiko Okayama from Japan. See also Gospel (1988, 1992, 2005) and Gospel and Fiedler (2013). In the USA, a parallel line of development was promoted by Sanford Jacoby and others.

³⁰The Business History Society of Japan, founded in 1964, had within a few years enrolled 350 members, earlier than American and European equivalents.

³¹Professor Yonekawa was a pioneer of global business history, with comparisons of the UK, USA and Asia (see, for example, Yonekawa 1987, 1990, 1994; Farnie and Yonekawa 1988; Okochi and Yonekawa 1982; also see Suzuki 1991; Yuzawa 1985, 1994).

the early 1970s (see Hannah 1976b; McCraw 1988: 14–15, 21–22). Some local critics even accused the BHU of what Australians memorably condemn as ‘cultural cringe’: a belief that foreigners do the best work, and malign because it kills domestic self-confidence and capabilities. The staff extensively participated in a series of international conferences working out comparisons between Europe, America and Japan, often driven by Chandlerian insights (see Chandler and Daems 1980; Teichova et al. 1986; Davenport-Hines and Jones 1988; Davenport-Hines and Liebenau 1987; Hannah 1982b; Horn and Kocka 1979; Lévy-Leboyer 1979; Teichova et al. 1986).

Admiration turned to dismay, however, when Chandler published his *Scale and Scope* in 1990, attempting to generalise about the British and German business developments in the first half of the twentieth century compared to those in the USA. In varying degrees, those connected with the Unit dissociated themselves from his interpretation of family businesses, the divorce of ownership from control and relative performance in multinational development (see Cassis 1997; Edgerton and Horrocks 1994; Gourvish 1987: 34; Hannah 1995, 1999, 2006; Hannah and Wada 2001, 2009; Jeremy 1998; Jones 1997; Liebenau 2013). This scepticism was largely shared by other British business historians (see Alford 1994; Church 1990; Clarke and Trebilcock 1997; Supple 1991), and German specialists were also somewhat bemused by an analysis which underappreciated family enterprise (in the land of the *Mittelstand*) and ignored the damage to German corporate capitalism of the dire events of 1933–1945 (which, after the judicious post-war collective amnesia of the compromised, younger German business historians were honestly and comprehensively coming to terms with) (see Herrigel 1996; Kleinschmidt and Welskopp 1993; Dunlavy and Welskopp 2007). In the USA—where the undercurrent of criticism that Chandler missed out too much of government, labour and the resilience of traditional firms and networks had long been discordant sounds off—stronger mainstream voices were heard castigating Chandler as misleading (see Lamoreaux et al. 2004). The fact remains that he was the leading developer of interesting new ideas in the field and the profession remains profoundly in his debt. Those criticising the inadequacies of his comparative book sometimes forget that it, too, forced people to think harder about many issues. Science progresses not through certainty, but through questioning, so Chandler also served when interesting but wrong. None of Chandler’s contemporary or successor business historians approached his pervasive influence on the development of other social sciences and management.

The BHU’s most durable international link was forged with Youssef Cassis of the Universities of Grenoble and Geneva, who became an Associate

when he finished his Geneva PhD on British bankers in the City under the supervision of Professor Eric Hobsbawm in 1982 (Cassis 1984). He remained attached to the Unit for nearly thirty years, until in 2011 he became Professor of History at the European University Institute, Florence. His wisdom was valued by faculty and students alike, and he brought a distinctive insistence on the traditional methods of the historian and a sensitivity to social issues in understanding business. His facility for involving those engaged in national research in comparative approaches resulted in multiple influential conference volumes on the theme of financial history over many decades.³² The multicultural ease of this inspiring British-Swiss scholar of Ottoman-Lebanese origin—comfortable in English, French and German—was a beacon to the rest of the staff. Most struggled to emulate his comparative skills (as Chandler's fate showed, comparative history can defeat the very best) and his own work made him the European comparative business historian par excellence (see Cassis et al. 2016).

The early years of the Unit were a time of profound political change in Britain, coinciding with the 'Winter of Discontent', the advent of Thatcherism and cuts in university funding. Yet these same factors meant an increased receptivity to a focus on business and entrepreneurship and this was a propitious time for fund-raising. The Director's Inaugural Lecture on Entrepreneurship was not only published in *Economica* (Hannah 1984) but also appeared as a full-page spread in *The Times* (Hannah 1983). There was also a prizewinning multipart BBC TV series *All Our Working Lives*, communicating the findings of British business historians to a wider public.³³ The involvement of some connected with the Unit in successfully recommending a 'hard' electricity privatisation (with a competitive structure) to Mrs Thatcher's government (see Hannah et al. 1987) gave it, in some quarters, a Thatcherite reputation, though its work was as likely to be reported in the *New Statesman* as in the *Financial Times*. In the latter, John Plender pointed out that BHU insistence on understanding the social roots of entrepreneurship undermined the current Thatcherite taste for tax tinkering, while lauding small business lending reforms and creative female and immigrant entrepreneurs (see Plender 1983; Keegan 1983; Benton 1986);

³²An early example was Van Helten and Cassis (1989).

³³Hannah was the overall series adviser. The book of the series was Pagnamenta and Overy (1984). While such initiatives are now lauded under the rubric of 'impact factors', they were rarer at the time and were not limited to enhancing public understanding: many years later these documentary films were still being used as teaching materials in history courses both at Harvard and in Cambridge, England.

the last of these was a response to a lecture by the Director on Channel 4 television, summarised in Hannah (1986b). In fact, most of the businessmen on its Steering Committee were more corporatists than Thatcherites and its staff were overwhelmingly centrist and perhaps more centre-left than centre-right.³⁴ Yet its business focus was undoubtedly an advantage in the marketplace: for some years most private and research council funding of history that took place within the University of London was concentrated on the BHU.

5 The Business History Unit: Mark II

Its first decade was generally counted a success, but the Steering Committee's views on its future from the mid-eighties decisively added fire to a fractious internal debate. One view was that, having served its purpose of stimulating the development of the discipline, the BHU should simply disperse, like the diaspora from Harvard's Research Center in Entrepreneurial History (1948–1958).³⁵ Some would leave and spread the word in new directions, while others might (as early discussions with the University Grants Committee had envisaged) develop business history within the Economic History Department (where two had already taken permanent appointments, initially funded by the Unit). The advocates of this strategy saw the future of business history as part of a broader interdisciplinary community of economics-orientated economic historians interested in institutions and organisations, not as an inward-looking sect. The alternative view was that the BHU should be extended as an independent unit, seeing separate disciplinary development as central to its purpose. Corporate supporters, fearful of the business focus being lost, joined independent academics from other universities on the Steering Committee in insisting by a large majority that a successful experiment with distinct approaches should not be abandoned. It is sometimes easier to launch initiatives than

³⁴Attracting both Eric Hobsbawm (an unreconstructed Marxist) and Arthur Seldon (of the free market think tank, the Institute of Economic Affairs) to its meetings (providentially not at the same time!) was no defence against the partisan labelling of the day.

³⁵The Harvard Center closed because its multiple funding applications were rejected in 1957/1958. It had hosted (albeit in some cases briefly (see Crandall 1960) some of the most distinguished economic historians of two generations, including Hugh Aitken, Bernard Bailyn, Noel Butlin, Alfred Chandler, Thomas Cochran, Lance Davis, Alexander Gerschenkron, Hrothgar Habakkuk, David Landes, Henrietta Larson, Maurice Lévy-Leboyer, Douglass North, Fritz Redlich, Henry Rosovsky, Barry Supple, Sylvia Thrupp, Charles Wilson and William Woodruff.

to terminate them, and the acute dilemma was resolved in a classic academic fudge: effectively, LSE (the institution clearly in the driving seat) chose both of the above.

Terry Gourvish was appointed the Unit's new Director in 1989: an LSE PhD then teaching at the University of East Anglia, he had to his credit major studies in business history, notably railways (Gourvish 1972, 1980). He inherited the Unit's core fund (which had grown slightly), with some staff, associates and visitors, and his brief was to develop new funding streams, projects and appointments, to support continuing independence and renewal. The original joint sponsor, Imperial College, developed its own separate programme of research, including science and technology in business, under the leadership of David Edgerton, who had been a BHU visitor.³⁶ Howard Gospel became one of the founding Professors at the Said Business School in Oxford.³⁷ The former core staff also took new positions, with some of the diaspora effects that the proponents of termination advocated. Leslie Hannah became LSE's Pro-Director (or what, in American academia, would be termed Provost). Geoffrey Jones moved to the Economics Department at Reading in 1988, promoted to Reader and taking with him the journal *Business History*, having recently succeeded Hannah as editor.³⁸ Business history as a separate discipline was then coming of age, and Jones helped drive the formation of the (UK) Association of Business Historians (of which he was President in 1992–1993) and the European Business History Association (President 1997–1999), whose larger, member-financed, annual conferences superseded the BHU's conferences. Jonathan Liebenau worked at the policy-orientated Technical Change Centre (a quango hosted by Imperial College) and Boston University before returning to LSE's Information Systems Department and later the new Management Department where he specialised in research and teaching on the management of technology, the digital economy and the adoption of technologies in developing countries. David Jeremy moved to Manchester Metropolitan University in 1987, where he developed his work in business prosopography

³⁶From 1993, at its new Centre for the History of Science, Technology and Medicine, which transferred in 2013 to King's College London, opposite LSE. Edgerton deepened an earlier theme in questioning Whiggish, techno-nationalist interpretations of business history and current science policy, notably Edgerton (1991, 2008, 2010) and Edgerton and Horrocks (1994).

³⁷And later a closer neighbour to LSE, as founding Professor of Management at King's College London.

³⁸Promoted to Professor in 1991, in 1993 he set up the Centre for International Business History (which still thrives), enriched by synergy with leading Reading economists studying multinationals. In 2002, he moved to the Harvard Business School, where he was soon appointed to the profession's historic pinnacle, the Chair in Business History that Chandler had once occupied.

(Jeremy 1984, 1990) and was promoted to a Chair in 1996. Other BHU staff and students pursued careers as freelance writers and/or taught and researched elsewhere.³⁹

Under newly reinvigorated leadership, the BHU itself faced more competition (in fund-raising and academic output), both from other universities and from other departments within LSE. Impressively, Terry Gourvish extended its role as a research centre for another quarter-century (he retired in 2014).⁴⁰ Several similar research centres had been set up elsewhere⁴¹: if imitation is the sincerest form of flattery, this amounts to a considerable accolade for LSE's pioneering, but competition made life much harder for the new Director. There are few sustainable first-mover advantages in academia below the level of university reputation, and it proved impossible to maintain the historic level of funding. However, stronger links were forged with the Business Archives Council: Gourvish became Chairman (2001–2013) and President (from 2015) and, more generally, vigorously championed the empirical approach (Gourvish 1995, 2006a).⁴²

Links with Japan were maintained with a series of Anglo-Japanese conferences (jointly sponsored by the Business History Society of Japan) and a steady stream of Japanese academic visitors (see, for example, Abe and Gourvish 1997); there were also collaborative conferences with French and Latin American colleagues (see Cassis et al. 1995; Giroletti et al. 1999).⁴³ Seminars continued (some supported by ING Baring), with papers by economists and government regulators complementing those by businessmen, business historians and archivists; they attracted large audiences from

³⁹See footnote 24 above for students. Richard Davenport-Hines, after a spell as a Visiting Fellow at Reading, became a widely admired freelance writer and public intellectual. Stephanie Zarach, Stephanie Jones and Theo Barker set up companies operating bespoke corporate history services. Christine Shaw returned to her earlier career as a Renaissance historian at Warwick. Geoffrey Tweedale gained a Wellcome Trust grant to study the business response to asbestos-related diseases, later becoming a Professor at Manchester Metropolitan. Margaret Ackrill and Judy Slinn completed several firm histories and taught at Oxford Brookes University.

⁴⁰His colleagues marked his achievements in a Festschrift (Coopey and Lyth 2009). He remained a Visiting Fellow and became President of the Business Archives Council in 2015.

⁴¹For example, the Centre for Business History in Scotland, initially under Professor Tony Slaven, on its establishment in Glasgow in 1987, and now under Professor Ray Stokes, had a large initial endowment and continuing support for operational expenses (from the Aggregate Foundation (now the William Lind Foundation) and Ballast Trust) and close integration with Glasgow's Economic and Social History Department and Adam Smith Business School.

⁴²Among the archives which the BHU placed in the LSE Library were the much-cited papers of the Management Research Group No 1, donated by Mr. Harry Ward, its secretary. See: <https://archives.lse.ac.uk/TreeBrowse.aspx?src=CalmView.Catalog&field=RefNo&key=MRG>.

⁴³Carlos Davila (Colombia) and Domingos (Brazil) were BHU Visiting Fellows and Colin Lewis in Economic History coordinated other links.

other London institutions. Financial history was supported by new projects, including a tercentenary conference volume for the Bank of England (Roberts and Kynaston 1995) and a history of Britain's premier venture capital company originally established under pressure from the Labour government in 1945 (Coopey and Clarke 1995; see also Coopey 2004). Youssef Cassis edited multiple volumes of comparative financial history (see, for example, Battilossi and Cassis 2002) and in 1994 founded a new specialist journal, the *Financial History Review*. Alice Teichova joined the Unit, under an ESRC grant, leading comparative work on universal banking, particularly strengthening the East European dimension (Teichova et al. 1994).

The Director continued his own work on brewing, with a much-admired volume with Richard Wilson on this UK industry (Gourvish and Wilson 1994), winner of the Wadsworth Prize in the same year). With Michael Anson⁴⁴ as Research Officer, he also worked on railways: two volumes critically analysed the muddle of public ownership, privatisation and a quasi-private phase in recent railway history, and he also undertook the official UK Cabinet Office history of the Channel Tunnel project (Gourvish 2002, 2006b, 2008). Gourvish contributed to the public debate about post-privatisation railways with his 2008 critique of labour policy in *Britain's Railways, 1997–2005: Labour's Strategic Experiment*. The interaction of business and politics was further pursued in conferences (see, for example, Gourvish 2003) and in an ESRC project on the role of the State in promoting industrial efficiency (see Tiratsoo and Tomlinson 1993, 1998; Gourvish and Tiratsoo 1998). Under a Leverhulme grant, the BHU handled the British arm of a multinational project to measure comparative business performance of leading European firms over the twentieth century, which again undermined the earlier Chandlerian hypotheses on comparative European history (Cassis et al. 2016). A bibliographic guide to the international business history literature was produced (Goodall 1987; Goodall et al. 1996), and several corporate histories were undertaken, notably of Glaxo, the pharmaceutical giant, and of Rowntree, the chocolate maker.⁴⁵

Meanwhile, business history also continued to thrive in other departments of the School. In the Economic History Department, Gerben Bakker

⁴⁴Anson later worked for Forrest Capie on the official history of the Bank of England and is now that Bank's Archive Manager.

⁴⁵The former undertaken by Davenport-Hines and Slinn (1992) and Jones (2001), the latter by Fitzgerald (1995). There were also histories of Leopold Joseph (a City private bank), Tannoy, Abbott Laboratories (a US pharmaceutical business with operations in the UK) and the Timber Trade Federation as well as studies of mail order selling and the popular music industry.

focused on innovation studies, with a particular emphasis on the entertainment industries (see, for example, Bakker 2008; Bakker et al. 2015), Janet Hunter's contributions to Japanese business and labour history included a study of Anglo-Japanese business relations (Hunter and Sugiyama 2002), Albrecht Ritschl revised Anglo-German productivity comparisons and published a study of the MIGROS retailing group (see, for example, Ritschl 2008; Ritschl et al. 2003), Debin Ma delineated the financial revolution in Republican China (Ma 2016), Colin Lewis assessed British direct investments in Argentina (Lewis 2015), Tom Nicholas analysed the *Dictionary of Business Biography*, quantifying the link between social and educational factors and British management performance (Nicholas 1999a, b; 2000a, b)⁴⁶ (as also did Olivier Blanchard in the Centre for Economic Performance (Blanchard 1993)),⁴⁷ Margaret Ackrill and Leslie Hannah completed a history of Barclays Bank, notable for providing more statistical underpinning than some predecessors (Ackrill and Hannah 2001, winner of the 2002 Wadsworth Prize), and Tim Leunig critically examined Anglo-American productivity differences (Leunig 2003). Linking business history to the growing interest in institutions as levers of development, Paul Johnson cast a sceptical eye over Victorian capitalism in *Making the Market* (Johnson 2010), while Leslie Hannah (working with James Foreman-Peck at Cardiff) presented a more positive view of British businesses then pioneering the divorce of ownership from control and the global spread of the corporation (Foreman-Peck and Hannah 2012, 2013, 2015, 2016; see also Hannah 2015; Hannah and Kasuya 2016). When Terry Gourvish retired, the mantle of Business History was taken over by the Department, with the research seminar continuing as Bakker's 'The Economic History of Firms and Industries'. In the International History Department, Tony Howe examined business influences on Britain's free-trade policies (Howe 1997).

Outside core history departments, the wholesome tradition of recruiting cerebral journalists to leaven academic studies continued. Susan Strange, an LSE economics graduate and *Economist* journalist, recruited as Professor of International Relations in 1978–1988, had developed critical studies in international finance and political economy, insisting (in extension of Braudel and

⁴⁶At the time, Nicholas was a British Academy Postdoctoral Fellow in the LSE Economic History Department, but soon joined the Entrepreneurship Unit at Harvard Business School. He is now Professor of Business Administration there, responsible for teaching the 'Coming of Managerial Capitalism' course on the MBA, established by Chandler, McCraw and Tedlow.

⁴⁷Blanchard was then visiting from MIT where he was Professor of Economics; he was later Chief Economist at the IMF. See also Hannah (1992).

Polanyi) that divergences in varieties of capitalism were weakening (Strange 1986, 1988, 1997). Sir Geoffrey Owen, after distinguished service as editor of the *Financial Times*, joined LSE's Centre for Economic Performance in 1990 and later the new Institute of Management.⁴⁸ His *From Empire to Europe* (Owen 1999) remains the best account of Britain's post-war industrial decline and revival, while his history of Courtaulds is a model of careful analysis of corporate decline, eschewing the simplicities of jejune business strategy texts (Owen 2010). In the Economics Department, John Sutton used the resources of business historians in testing more parsimonious (and modest) models that set new standards in industrial economics (Sutton 1981, 2001, 2012). Others largely bypassed historical evidence though succeeded in amassing impressive comparative data from recent experience on the effects of managerial practice on performance (Bloom et al. 2014). In the Centre for Economic Performance, Ron Dore developed his version of the 'varieties of capitalism' literature (Dore 2000). Tony Giddens interested George Soros, as well as business historians, in his theories of reflexivity, attempting to make sense of modern paradoxes of global capitalism (see, for example, Hutton and Giddens 2000). In industrial relations, Keith Bradley analysed the Spanish cooperative conglomerate Mondragon (Bradley and Gelb 1983). In accounting, Basil Yamey—who was a trustee of the Tate Gallery (as well as accounting Professor and member of the BHU Management Committee)—combined his two worlds in *Art and Accounting* (Yamey 1989), while Richard Macve worked with Debin Ma on divergences between Chinese and European accounting (Ma and Macve 2016; see also Hoskin and Macve 2012). Finally, Duncan Clark, Visiting Senior Fellow at the Institute for Global Affairs and founder of the investor advisory firm, BDA China, wrote *Alibaba: The House That Jack Ma Built* (Clark 2015).⁴⁹

6 Conclusion

Business history was a minor strand in the development of economics and related subjects at LSE, but business historians (or others writing or using business history)—whether in the BHU, the Economic History Department, the Economics Department or elsewhere—played a useful

⁴⁸He is now a Senior Fellow in the Department of Management.

⁴⁹Shortlisted for the FT/McKinsey Business Book of the Year Award 2016. Clark completed his undergraduate degree in Economic History (1990) at LSE and is Chairman of the British Chamber of Commerce in China and a major donor to LSE.

role in demonstrating insights from empirical work that were relevant to the development and testing of hypotheses about management and economics. From those early remarkable individuals, Ronald Coase and Edith Penrose, who (like Monsieur Jourdain and his prose) wrote business history without calling it that, to their successors more obviously differentiating themselves as business historians—Francesca Carnevali, Youssef Cassis, David Edgerton, Geoffrey Jones, Kazuo Wada and many others—those trained in the subject at LSE, and/or teaching and researching there, extensively contributed to the development of the subject within British and overseas universities and business schools. They did not form a distinctive school—if there was a central theme, it was simply the methodology of blending empirical work in archives with representative samples for understanding business institutions and organisations—but all reflected and contributed to the eclectic discipline which business history has now become.

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5

LSE's Contributions to the Economics of Social Policy

Howard Glennerster

1 Introduction

The study of 'social policy' as a distinctive intellectual endeavour only dates back to the period immediately after the Second World War. Professor Keith Hancock, Chichele Professor of Economic History at Oxford, was charged with organising a series of civil histories to take their place alongside the official military and diplomatic histories of the Second World War. He and Margaret Gowing (Hancock and Gowing 1949) contributed a volume of economic history, but Hancock argued in his introduction: 'The problems of war-time social policy stood clearly defined and were entrusted to Mr. R.M. Titmuss' (ibid.: xiii). That account, *Problems of Social Policy*, gave the term currency and at least the beginnings of coherence (Titmuss 1950). A collection of apparently disparate government activities, from health care to cash support, could be seen to have a common rationale—maintaining 'society's will to survive as an organic whole' (Titmuss 1958: 39). Without such collective action, support for democratic institutions might well erode as it had in the 1930s.

Prior to 1950, it is therefore futile to look for anything that might be called 'the economics of social policy'. Moreover, the LSE Economics

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Department had been pro-free market and generally hostile to government intervention in whatever form for much of its history. Yet some elements of what would now be seen as a recognisable social policy interest are to be found from the outset. The work of Edwin Cannan and his successors is one example.

2 Taxation

Cannan gave five lectures at the end of 1895 on the history of local taxation during which he sought to explain how local taxes came to be levied on ‘immovable property’. An extended version was published as *The History of Local Rates in England* (Cannan 1898). The second edition of the volume (Cannan 1912) took the whole topic much further. The book’s title was extended to add ‘*in Relation to the Proper Distribution of the Burden of Taxation*’. The topics he addressed were: How far should local taxes fall on different income groups? How far should central government be involved in funding local services? How far should its grants reflect the differential requirements of local people for services? How far should central government tell local authorities how to spend their money? Each question would today be considered part of the ‘economics of social policy’. As a one-time local councillor, Cannan was suspicious of central ‘experts’ and of the trend to a ‘new Chadwickianity’ that he observed in the growth of central government power, although he did favour some kind of central support for areas with particular social problems. It is difficult to read this lively account without being continuously reminded of current debates.

This LSE interest in public finance, taxation and its distributional consequences continued. Post-First World War lectures on the theory of public finance were given by Hugh Dalton, later Chancellor of the Exchequer in the Attlee government,¹ and published under the title *Principles of Public Finance* in 1922. As he put it in his Preface: ‘It has been customary with the majority of economists to adopt a somewhat negative attitude towards public expenditure. I have attempted to treat this part of the subject more positively and to exhibit the parallelism which exists between the theory of public expenditure and that of taxation’ (ibid.: vii).

¹Attlee had been recruited as a Lecturer in the new Department of Social Science and Administration at LSE. Dalton had been rejected. It was something Dalton never forgot! (Bew 2016: 71–72).

The chapter headings in Dalton's textbook sound familiar today—'the characteristics of a good tax system', 'the incidence of taxation', 'the distribution of the burden of taxation from the point of view of equity'. So, too, do many of the arguments, including the need to balance the efficiency gains from collective activity against possible disincentives. Dalton produced a scathing dismissal of the whole notion that there was some given 'taxable capacity' of a nation.

This public finance tradition continued in the work of Nicholas Kaldor, Alan Peacock and Alan Prest through the 1940s, 1950s and 1960s. Prest's *Public Finance in Theory and Practice* was published in 1960 and went to seven editions. The interest in local government finance continued too. Three colleagues, Foster et al. (1980), produced an outstanding follow-up to Cannan's work, a study of local government finance that has never been outdone. It was entitled *Local Government Finance in a Unitary State*. In their introduction, they pointed out that 'Local government finance has become an esoteric mystery, with its own jargon, with which very few are really familiar' (ibid.: 4). That unfortunately remains true today despite their best efforts and the considerable changes since. The fiscal unitary state they described has disappeared. That is something they would probably have welcomed, if not necessarily the form it has taken.

3 Income Distribution

A second related strand of work began somewhat later but has also persisted. It was the attempt to measure the combined distributional impact of taxes and public spending. How much did each income group pay in tax and receive back in benefits? What was the overall distributional impact? The pioneer in this work was Tibor Barna, a Hungarian PhD student. He was supervised by another Hungarian, Nicholas Kaldor, during the Second World War. Some attempt to tackle this distributional question had been attempted in work by an American academic (Stauffacher 1941). The idea to do it for the UK probably came from Kaldor, but the meticulous and inventive detective work was Barna's. (He was later to teach Julian Le Grand at Sussex, such are the linkages in academic life.)

Barna worked with public expenditure data for 1937. He soon concluded that the benefits of some spending, for example on defence, could not be attributed to any particular section of the community. But other categories could be. It was possible to estimate which income groups used State schools, which group received State pension income and public

assistance, etc. There were heroic assumptions to be made, some crude guesses and remaining problems. But each was discussed with great care and appropriate caveats.

Barna's PhD was approved and then published by Oxford University Press (Barna 1945). Subsequently, various academic attempts were made to replicate his work but in the late 1950s, J.L. Nicholson, of the UK Central Statistical Office (CSO), recognised that it would be feasible to substantially improve the accuracy of Barna's analysis. The new Family Expenditure Survey, introduced to calculate weights for the consumer price index, made it possible to estimate how much indirect taxation affected net family resources in each income group. Linking the sample's family characteristics to other studies of service take-up would give a more accurate picture of service usage than Barna had been able to achieve.

The first official attempt to extend Barna's work in this way was published in 1962 (CSO 1962). From then on, the CSO and its successors have produced such a study annually—for example, see ONS (2016). A comparison of these surveys enables us to trace how far the tax and benefit system has reduced post-tax and benefit inequality over time. In the late 1930s, the Gini coefficient was probably reduced by about ten points. That equalising effect more than doubled after the Second World War and has been sustained for much of the period since (Glennerster 2006).

This approach was, and is, not without its critics. Alan Peacock, Kaldor's successor as Reader in Public Finance at LSE was one. How are we to value the benefits a household gains from free health care? What would the family be prepared to pay in a free market, 'in the absence of compulsion', as Peacock (1954: 11) put it? Perhaps they would have bought less health care, valuing it differently from its cost to the State. However, Peacock decided that the results of Barna's work told us something and were sufficiently interesting to replicate and to ask academics in other countries to try to do the same. The result, Peacock (*ibid.*), is the first example of an international study of the distributional impact of taxes and benefits.

Despite such caveats, these studies became a staple diet of social policy commentaries and indeed political debate. Sefton (1997) criticised and corrected the crude assumption that all families of given ages received the same value from services in kind like the NHS. Hills and others extended the approach into a life cycle or intergenerational analysis of social benefits gained and tax payments made over a lifetime (Falkingham and Hills 1995; Hills 2015).

4 TIDI

This interest in taxation and income distribution took on a much deeper focus with the arrival of 'Tony' Atkinson. He moved from University College London, to take a Chair at LSE in 1980. He stayed for twelve years before moving on to Cambridge and, two years later, to Oxford. An account of his contribution to economics is to be found in the first volume of this series, but his arrival at LSE was groundbreaking in a number of respects. He became Chairman of its then largest research centre, the Suntory and Toyota International Centres for Economics and Related Disciplines—STICERD as it was popularly known. No other leading figure in the Economics Department had ever shown the deep interest in social policy that Atkinson exhibited. He was unusual as an economist in his preparedness to master the detail of social security provisions and subject it to rigorous economic analysis.

Atkinson's first major publication was 'Poverty and the Reform of Social Security' (Atkinson 1969), written when he was an Assistant Lecturer in Economics at Cambridge. He said that he was prompted to write it by the publication of Abel-Smith and Townsend's *The Poor and the Poorest* (1965) which had rekindled a political and academic interest in the topic of poverty. Their study claimed that the scale of poverty, measured in terms of a minimum income that attracted National Assistance, had risen during what had been seen as an era of prosperity. In his study, Atkinson took each major recent change in social security policy in the UK to assess its impact on the incomes of the poor and on work incentives. Possible alternative policy options were compared, notably modelling the impact of a social dividend or citizen's income scheme. His interest in such questions continued throughout his career.

During his time at University College, he developed a collaborative research programme—Taxation, Incentives and the Distribution of Income (TIDI)—together with Mervyn King and Nicholas Stern then at Birmingham and Warwick universities, respectively. The programme was originally framed as a response to a call for bids on the topic from the Social Science Research Council (SSRC) which later became the Economic and Social Research Council (ESRC). Work began at University College in January 1978 but moved to LSE with Atkinson in 1980. Extended as an ESRC Programme Grant, it lasted for twelve years, throughout Atkinson's time at LSE. He was later joined at the School and in STICERD by his

two collaborators. This programme proved to be a major contribution to the theoretical and statistical analyses of income distribution, but it also added a completely new technical means of analysing the distributional consequences of changes in tax and benefit policy, namely microsimulation modelling. As the TIDI final report to the ESRC put it: ‘The programme has pioneered the development of user-friendly micro-computer software for the analysis of taxation and social security’ (*TIDI Programme Final Report 1991*: 1 and in more detail, pp. 23–27). This modelling approach became widely used in policy debates and was taken up by other research groups and government departments. It produced a range of models—PTAX, TRAP, TAXBEN and TAXMOD. The latter developed into EUROMOD to compare distributional changes across countries of the EU. This approach enabled researchers to examine how different types of household with differing incomes would be affected by tax and benefit changes. For example, it is now taken for granted as a way of thinking about the impact of a government’s annual budget plans.

The need for better analytical tools to study the impact of taxation had become evident from the work of the Meade Commission and had been advocated by a House of Commons Select Committee. This led to a suggestion that the then SSRC should put out a call for research proposals on the topic. Atkinson and his group were successful. When the project was initiated, only a relatively low priority was given to the microsimulation element. But, as the capacity of microcomputers was transformed, so was the significance of this part of the project. It is difficult to imagine the tax and benefit policy world today without this means of holding governments to account for the distributional impact of policy change.

There are, of course, criticisms of and limitations to the approach. It does not, or did not, attempt to take into account the longer-term behavioural effects of changes to tax and benefit rates. Will higher benefits announced in a given budget reduce the incentives to work of people affected? What impact will the tax changes announced have on households’ work behaviour? That was the focus of other work on the project. Separate estimates of these longer-term effects can be added.

The programme also gave rise to a range of more theoretical and technical contributions. Cowell and Atkinson worked on the comparative merits and limitations of different ways to measure inequality and poverty. In his Walras-Bowley Lecture at MIT, Atkinson (1985) linked Bowley’s work on poverty with later approaches to the analysis of income distribution. He distinguished two approaches. One began with the notion of a minimum acceptable standard of living measured by a basket of essential goods and

services. The other was derived from minimum rights to access a range of resources. Simple measures of the number of households below a single poverty line were deficient, Atkinson argued. It was necessary to weigh the depth of deficiency on each measure to assess how far the depth of one deficiency might dominate others. Cowell (1987) also argued that what happened within the 'poor' category mattered. Both moved the study of poverty nearer to a broader analysis of income distribution. Atkinson devoted his Presidential Address for the Royal Economic Society to the topic: 'Bringing Income Distribution in From the Cold' (Atkinson 1997).

Other works in the TIDI programme focused on labour supply, savings, asset markets and wider tax policy. A regular international Public Economics Seminar was begun. Several PhD students and young academics joined the programme and went on to major careers—John Micklewright and Holly Sutherland are two examples. All in all, the programme generated 173 working papers, ten books, six major review articles and 74 journal articles. It would have fared well in any current review of academic and policy impact!

5 The Welfare State Programme

TIDI was not the only research programme Atkinson initiated. The Welfare State Programme was a second. What resources were being devoted to social policy and to what effect? A post was advertised to direct this research alongside Atkinson. The shortlist was narrowed down to two applicants, Julian Le Grand and John Hills. The selection board found it difficult to choose between them. Atkinson decided to find the money to appoint both. The Atkinson, Hills and Le Grand partnership resulted in nearly 130 discussion papers and several books, notably *The State of Welfare* (Hills 1990). That study produced a consistent detailed time series on different kinds of social policy expenditure, and how far stated objectives in each policy area had been achieved in the last quarter of the twentieth century.

Subsequently, and largely as a result of this work, Atkinson was asked to undertake an official review of the way the productivity of public services was measured in the national accounts. Until that point, the output of public services had been assumed to be equivalent to expenditure on them. Productivity was, by definition, nil. In a world where these services were taking a growing share of resources, this was less than helpful.

Atkinson recommended a series of service-by-service reviews to see how far outcomes, quality and standards could be systematically measured and compared with inputs to better inform the national accounts

(Atkinson 2005), ‘Atkinson Reviews’ as they came to be called. These studies are still undertaken annually, for example ONS (2017).

Another major product of the Welfare State Programme was a micro-simulation model that estimated the life cycle distributional effects of state welfare (Falkingham and Hills 1995). This work strikingly illustrated the extent to which ‘the welfare state’ had succeeded in smoothing out the financial risks families faced through the life cycle. Over their lives, even higher-income groups received back in benefits most of what they had paid in taxes. The model made it possible to measure the impact that changes in policy would have, and had had, on different groups over a lifetime. It made it possible to do this for each service and part of a service. For example, the way higher education was then financed proved to be massively advantageous to higher-income groups. It also charted gender differences and the impact of child-rearing on women’s lifetime income. It was a major analytical contribution.

6 Markets, Competition and the State

The tension between economics and social policy naturally stems from most economists’ belief in the virtues of markets and social policy academics’ scepticism of them. The LSE Economics Department’s leading figures from the outset favoured free markets wherever they could be achieved. They were critical of monopolies of any kind (Plant) and worried how to price utilities in the absence of proper competition (Coase). Meade favoured some kind of market pricing alongside State planning. Durbin favoured competition within a large public sector.

In the early part of the twentieth century, unemployment was seen primarily as a problem of wage rigidities, i.e. wages did not adjust downwards sufficiently fast to adjust to declining or changing demand. William Beveridge, then Director of LSE, changed his mind on this question. He was influenced by Lionel Robbins who acted as his research assistant and then advisor on a revision of Beveridge’s early work, *Unemployment: A Problem of Industry* (Beveridge 1909; 1930). Having originally dismissed wage rigidity as a mere ‘paradox of the lecture room’ (Beveridge 1914: 251), Beveridge, under Robbins’s influence, came to accept the idea in the 1930 edition of his book (Harris 1997: 312).

Robbins, working in the Treasury during the Second World War, was worried by Beveridge’s advocacy of a major extension of the State’s welfare role, especially the cost of universal pensions which Beveridge (1942) was

advocating (Harris, *ibid.*: 400–401). However, it fell to Friedrich Hayek to sound the loudest alarm against the growth of the State in his *The Road to Serfdom* (Hayek 1944, 1949). Expanding State power would end in tyranny, according to Hayek. He was dismissive of the notion that the term ‘social justice’ had any meaning (Hayek 1976). It implied that any distribution of rewards that arose from the decisions of many actors could have any moral judgement applied to it. Once the State began to try to influence such outcomes, it would get drawn into a steady and irreversible growth of power as it had done in Nazi Germany.

Hayek was to be one of the founding fathers of modern neoliberalism, and his work is evaluated in more detail in Chapter 16 of this volume. Another member of the School’s professoriate, the sociologist T.H. Marshall, who was attached to what was to become LSE’s Social Policy Department, had a different interpretation of post-war European and British history (Marshall 1950). Marshall had spent the First World War in a German prison camp, arrested while staying in Germany at the outbreak of war, spoke German fluently and during the Second World War had served in the German Section of the Foreign Office (Harris 2010).

Full equality of rewards, Marshall argued, was impossible and undesirable in a market economy. But if inhabitants were to feel fully part of a society—‘share in the social heritage’—they had to be able to enjoy the essentials of a civilised life—an equality of ‘citizenship status’. Without that, the very rule of law which made markets possible could be undermined by popular resentment and class warfare. Markets were made possible and framed by laws enacted by parliaments. Their results were therefore susceptible to moral judgements about their consequences. It will be clear what side this author takes in this debate but it was, and is, a fundamental one.

Several prominent members of the Economics Department shared Robbins’s and Hayek’s suspicion of the emergent ‘welfare state’. Peacock’s *The Economics of National Insurance* (1952) was the first serious study of the topic by a British economist. He questioned the need for a separate National Insurance Fund, and indeed of national insurance at all, arguing that the insurance element was tenuous and largely fictional. He argued for its replacement by what was, in effect, a negative income tax scheme combining a wide range of State benefits. He could be seen as the originator of the present government’s universal credit scheme but going well beyond it!

He and Jack Wiseman formed an unlikely duo. Peacock was charming, polite and willing to listen, though ruthless in discussion when he disagreed with you. Wiseman was a north-country extrovert, blunt and argumentative. In 1953, Peacock joined a group of Liberal politicians and academics called

the Unserving State Group to rethink the Liberal Party's approach to the welfare state after Beveridge. The outcome was a volume called *The Unserving State: Essays in Liberty and Welfare* (Peacock 1957). Peacock's position is summed up by his British Academy biographer (Peden 2015):

[N]o person's opportunities to develop should be frustrated by personal circumstances, and that therefore there was a good distributional argument for the state providing individuals with financial support for access to health, housing and education. However, for the most part these services need not be provided by the public sector. For example, state provision of hospitals and public health services did not require a state monopoly for routine medical and dental services. In general he [Peacock] hoped to encourage alternative sources to provide competition for existing public providers of social services. In particular he advocated the introduction of some kind of tax relief to parents who chose to educate their children privately (ibid.: 503).

These ideas were similar to those of Milton Friedman in many respects but were developed in advance of many of them. They were shared by others at the Institute of Economic Affairs, where Peacock had become a member of the Advisory Council. They have a contemporary ring.

As a duo, Peacock and Wiseman were effective communicators with a clear political position, but they also made a major original contribution to our understanding of the history of public expenditure in the UK and the reasons for its growth. Their study, *The Growth of Public Expenditure in the United Kingdom* (1961), pieced together a careful and consistent time series of public spending and its purposes from the end of the nineteenth century to the mid-1950s. It remains a key source. They went on to consider how well the trends they found supported existing theories of public expenditure growth, notably that advanced by the German economist Adolph Wagner. They differed.

The pattern of UK public spending that Peacock and Wiseman uncovered showed periods in which there had been relatively stable shares of the GDP devoted to public spending followed by peaks after the two world wars followed again by periods of stability. Those periods of stability were broken, they argued, when voters were persuaded to accept the higher taxes demanded by war. When the war was ended, those wartime tax rates could be reduced, but not to previous levels. Voters could—temporarily—enjoy both an expanded role for the State and lower taxes than they had grown used to.

The duo were, of course, writing at the end of a period in which the Conservative government of the day had managed to stabilise public

spending as a share of GDP in the early 1950s. Public spending's share was about to increase again. But the 'war' explanation supplements rather well the story told by Titmuss (1950), though they disagreed with him on almost everything else.

Peacock left LSE in 1956 and took a Chair at Edinburgh, moving on to York when the new university was founded, Wiseman following him. But his and Wiseman's work in the 1950s gives a good indication of the views held about social policy by those in the Economics Department at LSE at the time who took it seriously.

The next major figure to take on the teaching of public finance was Alan Prest. He was no enthusiast for the traditional welfare state. With Peacock and Wiseman, he submitted evidence to the Robbins Committee advocating student loans (Prest 1962; Peacock and Wiseman 1962). However, he mentored a young Nicholas Barr who helped with and co-authored the sixth and seventh editions of his textbook *Public Finance in Theory and Practice* (Prest 1960, first edition). As Barr put it to me: "The questions on which he mostly worked were 'How can government finance what it does?' and "What are the effects of different ways of financing what it does?" rather than "What should governments do, and why?" (personal correspondence).

Subsequently, the very different departmental traditions of work on social policy at LSE moved closer together. Both the Economics Department and the Social Policy Department contained a wide spectrum of opinion, but there was a growing area of mutual agreement. This was made possible in part because of economists' developing theories of market failure and (some) social policy academics' awareness of limits to the effectiveness of State action. In the period after the Second World War, the share of national spending devoted to social policy in the UK had risen from 10% to over a quarter. It was therefore understandable that economists should devote more attention to it.

7 An Emerging 'Economics of the Welfare State'

The acknowledgement that there were reasons why markets did not perform well in certain situations began with Arrow's (1963) discussion of uncertainty and health care. It continued with Akerlof's (1970) work on the imbalance of information between buyer and seller in certain markets and the related work of Rothschild and Stiglitz (1976). This phase of theoretical

work was followed by behavioural economics—a recognition that individuals do not always act in their own long-run best interests, notably in the way they make decisions or, more accurately, fail to. This was particularly pertinent when thinking about pensions or the funding of long-term care in old age. None of these key theories or approaches began at LSE, but their combined significance for social policy was appreciated by Nicholas Barr (see Chapter 33 in this volume).

In 1971, like many newly appointed staff in the Economics Department, he was given a job most other people did not want—in his case, teaching social policy students economics. ('They are suspicious of economics but they ask good questions' (personal recollection).) Barr continued to teach them for many years at his own request. He came to see that such approaches could be put together to produce a microeconomic foundation for social policy and a framework for his teaching. He enthusiastically reviewed the joint text produced by Le Grand and Robinson entitled *The Economics of Social Problems: The Market Versus the State* (1976) and in 1979 began a 25-lecture course with Le Grand on the 'Economics of the Welfare State'. Barr dealt with cash benefits, and Le Grand covered benefits in kind. When Le Grand left for Bristol, Barr took on the whole series. That led to the first edition of a book with the same title (Barr 1987) and consequently a sixty-page article in the *Journal of Economic Literature* (Barr 1992).

The central theme of this last contribution was 'the importance of the literature on imperfect information in establishing an efficiency case for various types of state intervention' (ibid.: 742). Barr set out the objectives he saw exemplified in social policy legislation: the avoidance of poverty, the protection of accustomed living standards and income smoothing over a lifetime. These aspirations were aimed at avoiding unnecessary stigma and maximising social solidarity. Markets themselves were not able to fully achieve these goals for reasons that were becoming more fully understood. As behavioural economics came on stream, it was incorporated into the model. Clearly, there are objections to this approach that economists of a different persuasion will bring. However, the forthcoming sixth edition of the textbook, *The Economics of the Welfare State*, attests to the power of this framework.

Barr's contribution was not confined to the classroom. Unlike many economists, he relished political debate and getting things done. This applied to two particular areas of policy: higher education finance and pensions.

Barr was an early convert to the idea of income-contingent student loans, suggesting in *The Times* that the national insurance contribution mechanism might be a collection device (Barr 1988). White Papers in 1987 and

1988 managed to produce proposals for funding British universities and students that combined the worst possible policy mix. They increased central government interference, ran down the means-tested system of student support and replaced it with a mortgage-type loan administered by banks. In an article in *The Independent* Barr launched a fierce attack: 'Mr. Baker's [then Education Secretary] statement set out a better class of drain down which to tip large volumes of taxpayers' money' (Barr 1989: 21).

For the next two decades, Barr hounded both Conservative and Labour governments on the subject. He was not afraid to appear before House of Commons committees, see ministers, have long discussions with officials in the Treasury, use the good offices and skills of LSE's publicity officer to write newspaper articles and appear on TV and radio to make his case. He gained the most sympathetic hearing from Charles Clarke, Blair's Education Secretary. The Labour government introduced fees, set at about a quarter of the tuition costs of universities. But they were to be repaid through the income tax system later in life and only by those earning above a threshold. This legislation embodied most of Barr's ideas. Indeed, he was in regular communication with Blair's and Clarke's offices as the legislation took shape. They sought his responses to new proposals and took note of his criticisms, unlike the following coalition government! (personal communication). He was highly critical of the big hike in fees after 2010 and the elimination of all direct Exchequer support for social science and humanities tuition costs (see Barr 2012). (For a collection of papers on the earlier 'campaign', see Barr and Crawford (2005).) Barr also advised the Australian, New Zealand and Hungarian governments on loan schemes.

Later in his career, from 2004 on, he became more involved in pension reform, especially in China and Chile, working in close collaboration with Peter Diamond (Barr and Diamond 2008). There was no one optimal pension model. It depended on the sophistication of a country's capital markets, its stage of economic development and its government's objectives. But there were a set of questions that designers of pension systems should try to answer.

Barr had had earlier battles with the World Bank over what he saw as their overenthusiastic advocacy of individual funded accounts, especially in developing countries. He was also involved in advising Eastern European nations emerging from the Soviet era on labour market and social policy strategies. (Later on, he joined the European Institute.) In both areas of public policy, higher education finance and pensions, Barr has made major contributions to social policy practice, using economics to help diagnose what the policy problem actually is and to generate options in helping to solve it.

8 The Economics of Education and Housing

The wider economics of education has been a consistent and important focus of attention for the Department of Economics well beyond the finance of higher education. R.H. Tawney saw the essence of the subject in his Hobhouse Memorial Lecture in 1938. Education was, he pointed out, a way of ‘investing in human beings’ (Tawney 1938: 16; also see Hobhouse Memorial Lectures, 1930–1940 (1948)). As Mark Blaug put it:

Fifteen years before economists turned their attention to what he calls “that repulsive hybrid”, the economics of education, Tawney grasped most of the issues: “the benefit of education to the boys and girls concerned is weighed against the loss to employers of their services as errand boys or little piecers, and to their parents of their earnings”; England must learn to “depend less on cheap coal and more on trained intelligence” (Blaug 1970: 8).

Blaug had been recruited to found the Economics of Education Research Centre at the London Institute of Education but spent a good deal of his time down the road at LSE in the Higher Education Research Unit. It had been founded in 1963 by Professor Claus Moser from the staff that had worked with the Robbins Committee on Higher Education. Maurice Peston, then the latest in the line of Readers in Public Finance at the School, was also a member. This was a period when the human capital approach to education was erupting and Blaug brought each significant new paper to be discussed by those working in the Unit. (The author was a junior member of the research staff at the time.) Richard Layard, who was Deputy Director of the Unit, decided he needed a proper training in economics, and later began the Centre for Labour Economics which would become the Centre for Economic Performance (CEP).

Given its origins, the economics of education has always been a significant element in that CEP’s work. A Centre for the Economics of Education was later created within it. This was followed by a programme of work on ‘Education and Skills’ led by Professor Sandra McNally and a new collaborative Centre on Vocational Education, a neglected area of concern. Much of this work has been policy directed. It has examined the outcomes of particular policy changes such as greater school choice and competition (Gibbons et al. 2008) and the new phase of ‘academies’ (Eyles et al. 2015). There have been several studies examining what impact the expansion of higher education has had on graduate earnings and how these vary between universities—a lot is the answer.

Education policy is often driven by a set of preconceptions that politicians bring to the topic, often drawing on their rose, or mud, coloured memories of their own schooling. What the Centre for the Economics of Education has consistently done is to approach this minefield dispassionately. What works has been its driving question, not economic theory.

Like Nicholas Barr, another intellectual lone wolf has been Christine Whitehead. She has made a significant contribution by bringing economics to bear on the housing market and has done so, almost alone, for forty years. Whitehead also held a position at the Department of Land Economy in Cambridge. In the end, she was made Professor of Housing Economics—not Economics!—at LSE. Whitehead brought hard-headed economic analysis to the subject from the beginning (Whitehead 1974). In particular, she challenged the notion of a long-run competitive equilibrium in housing markets: ‘The concept of equilibrium in the conventional sense is particularly inappropriate in the urban housing market because such factors as transaction costs and information costs are of more than usual importance’ (Whitehead and Odling-Smee 1975).

Since her early work, there have been profound shifts in housing policy. Public-sector building in England has been almost eliminated. Planning restrictions affect this market in major ways (Burgess et al. 2010). Cash subsidies to mitigate the cost of housing to the poor have grown in scale and are withdrawn as income rises in ways that impose high implicit tax rates on those households. Also, restrictions on labour mobility that are a consequence of housing rigidities and shortages can have serious implications for the wider economy. Whitehead concluded a lecture at the Geffrye Museum of the Home in London, surveying these issues in 2015 by saying, ‘Yes, we have a dysfunctional housing system’ (Whitehead 2015). Given its importance in the wider economy, the housing market might have been the focus of more attention by more people.

9 Quasi-markets

The use of competition to drive performance in the social service sector has not been a major focus of attention in LSE Economics Department, though in the tradition of Evan Durbin (see Chapter 19 in this volume) it might have been. It was only after Julian Le Grand left LSE that he began to focus on the issue (Le Grand 1991; Le Grand and Bartlett 1993; Bartlett et al. 1998). As the title of the last book in this list suggests, he saw a range of policies introduced in the 1990s as constituting a ‘revolution in social policy’.

They had been implemented as a remedy for what Le Grand saw as a series of ‘government failures’ in the delivery of social policy akin to market failures on which economists had previously concentrated. His Inaugural Lecture on gaining a Chair in Social Policy at LSE was expanded to become the volume, *Motivation, Agency, and Public Policy: Of Knights and Knaves, Pawns & Queens* (Le Grand 2003). Just what does motivate those who work in hospitals and GP surgeries and those who employ them? Pure altruism, as social policy traditionalists have claimed, or pure self-interest, as the neoliberals imply? Neither, completely, Le Grand claims but a complex mixture of both. Different people occupying different roles act differently, and people in the same roles act differently in response to different incentives, too. Some are knights, others less than saints, but most a mixture of the two. We need to understand this and work out how far financial incentives, competition and other drivers of professional action affect service quality and responsiveness. This is the right set of questions to ask, in the author’s view, but the mix of questions does not sit easily in either the economic or social policy tradition.

More recently, some in the Departments of Economics and Social Policy have combined with those in the CEP and at the University of Bristol to test the impact of introducing patient choice into the NHS. The conclusion that, in particular cases, it *did* improve health outcomes and the quality of hospital management may not have been a surprise to other economists, but it infuriated many in the public health tradition (Pollock et al. 2011; for a reply, see Bloom et al. 2011). It is entirely understandable that many economists would rather steer clear of such controversy. But with very tight spending margins for health care and education that is a pity.

Finally, political economy, or the economics of politics, has provided a new way into public policy. The question is not so much what ought policy to be but why do governments come to take the policy decisions they do? Here, Professor Tim Besley at LSE has taken a leading role, but both the question and the topic are much broader than social policy.

10 Conclusion

This review has left out any discussion of development economics, which contains a great deal of work on poverty in developing countries as well as on education and health care delivery. Development economics has had a long tradition in the Department of Economics. This review has also said nothing about the economics of global climate change which again overlaps with social policy, and here, Professor Nicholas Stern has played a leading role.

Looking back over more than a century of economics at LSE makes one aware of large changes in theoretical interest and practical policy concern. As the role of the State, especially that related to social policy, has grown (absorbing 2.5% of UK GDP in 1900 and 26% today), so economists' interest in it has grown. The original outright hostility to social policy has moderated as theories of market and information failure have come to be accepted, particularly in health care and pensions markets.

Debates remain. How far should public-sector providers exclusively undertake social service delivery and to what extent should they compete? What role should the State continue to play in pension provision and regulation? For some, the gulf between Friedrich Hayek's and T.H. Marshall's worldviews remains as wide as ever. But not for most.

It is also clear from this retrospective view that the Economics Department at LSE has engaged with questions of social policy more than many, perhaps because of the very existence of an interdisciplinary Department of Social Policy 'next door'. But often those specialising in it have tended to move on or have worked in a relatively isolated position. (Neither Barr nor Whitehead gained Chairs in 'economics'.) Perhaps that was inevitable given the wider academic approach to economics. Perhaps that is changing. Perhaps not.

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6

Economica and LSE Economics

Jim Thomas

1 Introduction

Economica was launched in 1921 and continues to be published in its reformulation as *Economica* (New Series), so that it will celebrate its centenary in 2021. Most journals that were founded at around that time or earlier were one of two types. First, there were those that were founded by some society or association, such as the *Economic Journal*, founded by the Royal Economic Society in 1891 or the *American Economic Review* (*AER*), founded by the American Economic Association in 1911. Alternatively, new journals were started and edited by the academics at a particular university, such as the *Quarterly Journal of Economics* (*QJE*), founded at Harvard in 1886, or the *Journal of Political Economy* (*JPE*), which started at Chicago in 1892. All these journals were ‘generalist’, in the sense that they covered all areas of economics and, in some cases, other related subjects, such as economic history, accounting or statistics. As economics has become more specialised, many new journals that have appeared have concentrated on research in particular areas of economics, such as the *Journal of Monetary Economics*, the *Journal of International Economic Law* or the *Journal of Economic Dynamics and Control*. The increasing number of economic journals is to be expected, as a growing body of academic economists require that extra space to

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accommodate the number of articles they must 'publish or perish', but the increasing number of specialist journals has set up an interesting dynamic in terms of the competition between generalist and specialist journals, as we shall see below.

2 The Launch of *Economica* (Old Series)

The first issue of *Economica* appeared in January 1921. It began with an unsigned Foreword setting out the purpose of the new journal¹:

The existing facilities for the publication of contributions to economic or political science have, it is believed, become inadequate; for the only journals devoted to any parts of this range of study are those of the Royal Economic and Royal Statistical Societies, and these are to a considerable extent specialist and are readily filled with weighty and important matter. There is a considerable output of work in the various departments of the London School of Economics and Political Science, which for want of an accessible vent, must be published in the inconvenient form of pamphlets or be delayed till it has reached the dimensions of a book, or remain unknown and wasted, and it is expected that this output will increase with the rapid development of the School. It may take the form of special lectures delivered by members of the staff, or of material prepared for lectures, or of preliminary studies which will ultimately have their place in a treatise; or it may be the result of work in seminars or of directed researches in one or other of the departments of the School, or may be the outcome of investigations made on their own initiative by post-graduate students. The range is as wide as that of the School's activities, from mediaeval history to modern statistics, and from political philosophy to railway operation. The purpose of *Economica* is to make public work of this character. It is proposed to issue this journal three times a year, in January, May and October, but in a new venture, it may not always be possible to attain perfect regularity. No doubt its form and the nature of its contents will be modified, as experience or the material available is obtained. It is intended to deal with new books, not by including reviews of important publications individually as they appear, but by articles on recent works in selected subjects, such

¹Although the editors are not mentioned in the first issue, Hayek (1946: 21) wrote: 'It [*Economica*] was started as a journal devoted to all the subjects taught at the School and was issued at first three times a year under the supervision of an editorial board whose original members were Professors Cannan, Wallas and Bowley with T.E. Gregory as secretary'.

as that by Mr. Laski in the present number. Lists and accounts of new books already form important features of the *Economic and Statistical Journals*, and it is unnecessary to duplicate them. Some pages will be devoted to notes on developments of the School which may have more than domestic interest, but apart from this, accounts of current affairs will be the concern of the old-established *Clare Market Review*, with which this journal does not compete and which it does not in any way supersede. It will be some time before *Economica* can become as well known as the *Journals of Studies* and other serial publications of the Faculties of Economics of America and other Universities, but it is hoped that it will receive a welcome from those who issue or are interested in these important contributions to current economic theory and history, and that like them, it will find its place on the tables and shelves of libraries and common rooms (*Economica* 1921: 1).

This is a very modest statement of intent, with the overall message being that *Economica* is to be the house journal of LSE, aiming to publicise the work that is going on there. It seems parochial to suggest that the *Economic Journal* and the *Journal of the Royal Statistical Society* were the only journals available for publication by LSE economists. This ignored the existence of the *QJE*, the *JPE* and *AER*. The omission is all the more surprising as Edwin Cannan, whom Hayek lists as one of the original editors, had already published two articles in the *QJE* (see Cannan 1905, 1921) and Herbert Foxwell, who taught economics at LSE from 1895 to 1922, had published in the *QJE* in 1887 (see Foxwell 1887).

Forty issues of *Economica* were published between January 1921 and November 1933, at which point its structure was changed. Some statistics concerning these issues are presented in Appendix 1. The journal was to present work from all the School's disciplines and columns 3–5 analyse the distribution of articles, grouped as 'Economics', 'Statistics' and 'Other' (the remaining disciplines). The number of articles per issue varied between four and nine, with an average of 6.8 per issue. In total, 286 articles were published, of which 84 (29%) were in 'Economics', 26 (9%) were in 'Statistics' and 176 (62%) were in 'Other'.²

²During this period, a large percentage of articles published in *Economica* were in-house: an analysis presented to the Editorial Board in 1937 showed that 69% were written by (past or present) staff or students at the school. The corresponding percentage for the 16 issues of *Economica* (New Series) between 1934 and 1937 was 63%, while for the corresponding issues of *Politica* (of which more details below) the figure was 53% (see BLPES (c)).

With respect to book reviews, the 'Foreword' quoted above suggests that individual reviews would not be appearing, but from Issue No. 8 on, a considerable number of reviews did in fact appear in *Economica*. Column 7 lists the number of book reviews in each issue, the range being between 5 and 26 at an average of 13.6 reviews per issue. Column 8 lists the percentage of pages devoted to book reviews out of the total available for articles plus reviews. The range is from 15% to 41%, with an average of 25.8%. These data suggest that during this early period *Economica* published a relatively small number of articles per issue and devoted about a quarter of its space to reviews.

While the early issues did attract some overseas contributions, such as a public lecture given by Cassel at LSE on 'The Restoration of the Gold Standard' (Cassel 1923) and Schumpeter explaining the business cycle (Schumpeter 1927), and did also contain some theoretical articles, such as Robertson on 'Economic Incentive' (Robertson 1921), more typical were survey articles, such as Dalton (1921, 1922).

Economica changed over time, and initially, this was due to generational changes in LSE's Economics Department: Foxwell retired in 1922 and Cannan in 1926, the same year that John Hicks arrived as an Assistant Lecturer. R.G.D. Allen joined the School in 1928 and, while he was officially in the Statistics Department, he was very much involved with the Economics Department. Among the undergraduates in the Department were Nicholas Kaldor (1927–1930) and Abba Lerner (1929–1932). While Hugh Dalton continued teaching at LSE, he was becoming increasingly preoccupied with his political role within the British Labour Party.

Although Hayek does not mention him as being connected with the Editorial Board of *Economica*, it is clear that the LSE's Director, William Beveridge, was very much involved with the journal from the outset and the Beveridge Archives held at the School contain a number of letters that are clearly written in an editorial capacity.³ In 1923, he proposed that Dalton and Harold Laski should be the editors, with Eveline Burns to act as assistant editor. When he wanted to give up his editorship in 1926, Dalton nominated Lionel Robbins, who had just returned to LSE from New College, Oxford, as a possible replacement. Beveridge responded to this suggestion in a letter dated 1 July 1926:

³For example, see BLPES BEVERIDGE/2B/22/5, which contains correspondence for the period June–July 1923. The membership of the LSE Director on the Editorial Board of *Economica* continued until at least the 1970s.

Dear Dalton, There is one matter which you mentioned to me regarding the future arrangements about which I am not clear, and that is the editorship of *Economica*. Admirable as Robbins is, I think he is a little young to put forward for the position of editor at once. Can you manage to keep it on, or can we use Gregory? Personally I prefer the former. Yours sincerely, WHB (BLPES (a)).

Dalton disagreed that Robbins was too young, but accepted the suggestion and Theodore Gregory agreed to become an editor.⁴ However, on his return to LSE in 1929 as a Professor, Robbins joined the Editorial Board of *Economica* and contributed a number of articles (Robbins 1930a, b, c, 1931).

In the 1929–1930 session, Robbins began running a seminar in economic theory for research students. In 1931–1932, he was joined by Hayek and from 1932–1933 to 1936–1937. Arnold Plant joined the other two.⁵ The discussions in the seminar led to a number of articles being published in *Economica*: for example, Hicks (1931), Allen (1932, 1933) and Edelberg's mathematical restatement of Ricardo's theory of profits (Edelberg 1933).

In the early 1930s, *Economica* contained a number of articles reflecting the growing economic disagreements between LSE (Robbins and Hayek) and Cambridge (Keynes). In August 1931, Hayek published the first instalment (Hayek 1931a) of a two-part critical review of Keynes's *Treatise on Money* (Keynes 1930 [1971]). The November 1931 issue of *Economica* included a one-page erratum (Hayek 1931b), correcting one diagram from his article in the August issue, a reply to that article by Keynes (1931) and a rejoinder from Hayek (1931c). In his reply, Keynes begins with Hayek's article of August 1931, but then extends the discussion to include Hayek's *Prices and Production* (Hayek 1931d) and expresses a low opinion of its merits:

The reader will perceive that I have been drifting into a review of Dr. Hayek's *Prices and Production*. And this being so, I should like, if the Editor will allow me, to consider this book a little further. The book, as it stands, seems to me to be one of the most frightful muddles I have ever read, with scarcely a sound proposition in it beginning with page 45, and yet it remains a book of some interest, which is likely to leave its mark on the mind of the reader. It is an extraordinary example of how, starting with a mistake, a remorseless logician can end up in Bedlam (Keynes 1931: 394).

⁴Theodore Gregory (1890–1970) had been Cassel Reader in international trade at LSE from 1920 and became Sir Ernest Cassel Professor of Economics in the University of London in 1927, so was much more senior than Robbins at that point in time.

⁵See Howson (2005) for an extensive discussion of the 'Robbins Seminar' and the research that came out of it.

The second part of Hayek's review (Hayek 1932a) appeared in the February 1932 issue of *Economica*. Keynes did not respond further to Hayek's criticisms, but the March 1932 issue of the *Economic Journal* contained a critical review by Piero Sraffa (1932a) of *Prices and Production*.⁶

In 1932, Robbins proposed that instead of expanding the journal, it should be split into two, *Economica* and *Politica* (see Howson 2011: 223). Readers were notified of the forthcoming changes in the August 1933 issue of *Economica*:

The pressure of material on the space at present available in *ECONOMICA* has made it necessary to contemplate an enlargement. It has been thought convenient at the same time to arrange for dividing the material and to issue two periodicals, to be known as *ECONOMICA* and *POLITICA* respectively. The first of these will deal with economic questions, including Economic History⁷ and Statistics, and allied subjects, and will be issued as heretofore four times in the year, about February, May, August and November. The second of these will deal with Political Science, Sociology, International Relations, Law in its bearing on these studies, and allied subjects, and will be issued in the first instance twice a year, at the same time as the February and August numbers of *ECONOMICA* (*Economica* 1933: iv).⁸

⁶Hayek's reply to Sraffa's review (Hayek 1932b) ends: 'That Mr. Sraffa should have made such a suggestion, indeed, seems to me only to indicate the new and rather unexpected fact that he has understood Mr. Keynes' theory even less than he has my own' (ibid.: 249). At this point, Keynes re-entered the fray by adding a footnote to Hayek's concluding sentence: '[With Prof. Hayek's permission I should like to say that, to the best of my comprehension, Mr. Sraffa has understood my theory accurately. – J.M. KEYNES.]' (ibid.: fn. 2). See also Sraffa's follow-up (Sraffa 1932b).

⁷Although Economic History is specifically mentioned and Eileen Powell was one of the two editors, only nine articles in the discipline were published between 1934 and 1940, one of them being Clapham's obituary notice following the death of Powell in 1940 (see Clapham 1940).

⁸The first issue of *Politica* stated that: 'POLITICA is issued twice yearly, in February and August, by the London School of Economics and Political Science. It is devoted to Political Science, Sociology, International Law and Relations and allied subjects, and is under the direction of an Editorial Board composed of Sir William Beveridge, Dr. W. Ivor Jennings, Mr. A.V. Judges, Professor H. Laski (Acting Editor), Professor C.A.W. Manning and Professor C.K. Webster (Acting Editor), with Mr. H.R.G. Greaves as Assistant Editor. The Journal is intended primarily to afford a means to the public of becoming acquainted with the results of investigations, or other work both by the staff and students (past and present) of the School, and by contributors in Great Britain and other countries' (*Politica* 1934: Inside front cover). The publication of *Politica* was discontinued at the outbreak of the Second World War.

3 *Economica* (New Series)

The editors of *Economica* (New Series) were Lionel Robbins and Eileen Power as acting editors with Frank Paish as assistant editor.⁹ The first issue got off to a good theoretical start, as Robbins noted in a letter to Fritz Machlup in January 1934 describing recent events at the School. In the Robbins Seminar, there had been ‘some very fruitful discussions on the pure theory of value. Hicks and Allen have discovered some new formulae which have really path-breaking significance and should do much to unify views on this fascinating subject. You will see the first fruits in the first number of the new *Economica* [Hicks and Allen 1934]’. Paul Rosenstein-Rodan, who was ‘a great success’ as a lecturer at University College London, also had an article in that issue (Rosenstein-Rodan 1934) (see Howson 2011: 249–250).

In 1935, political developments in Europe were reflected in *Economica* when Wilhelm Röpke, a future economic advisor to Chancellor Konrad Adenauer, published ‘Fascist Economics’ (Röpke 1935), and a critical analysis that concluded:

Another thing is much more striking in Germany than in Italy. There is a pathetic contrast between the vigour with which the Old (“Liberal”) Economics are decried by the new set of economists and the exceedingly poor crop of new or even newly polished ideas which are to constitute the New Economics ... Almost everything that is presented as something new and revolutionary reveals itself, on closer inspection, as old wine in new bottles with eye-catching labels (ibid.: 99).

When Keynes’s *General Theory of Employment, Interest and Money* (Keynes 1936 [1973]) appeared, the review in *Economica* was not written by an LSE economist, but by A.C. Pigou, Keynes’s noted Cambridge colleague who had been subject to a great deal of criticism in the book. It was highly critical of Keynes’s criticisms of other authors and begins:

When, in 1919, he wrote *The Economic Consequences of the Peace*, Mr. Keynes did a good day’s work for the world in helping it back towards sanity. But he did a bad day’s work for himself as an economist. For he discovered then, and his sub-conscious mind has not been able to forget since, that the best way to

⁹From 1934 on, the names of the members of the Editorial Board were listed on the cover of the journal. Until 1966, editors were listed as ‘Acting Editors’ no matter how long they held the position. Then, in 1966, they were finally referred to as ‘Editors’ and the ‘Assistant Editor’ became the ‘Review Editor’. A complete list of the editors of *Economica* from 1934 to 2017 is presented in Appendices 2(a) and 2(b).

win attention for one's own ideas is to present them in a matrix of sarcastic comment upon other people. This method has long been a routine one among political pamphleteers. It is less appropriate, and fortunately less common, in scientific discussion (Pigou 1936: 115).

One notable theoretical article that was published in this period was 'The Nature of the Firm' by Ronald Coase (1937). Although it was quoted as one of the two major contributions that earned him the Nobel Prize in Economics in 1991, Coase later reported that it was not an instant success at LSE, as neither Robbins nor Hayek ever made a point of discussing it with him (see Coase 1988: 6).

4 Second World War

With the outbreak of the Second World War, LSE, including *Economica*, was evacuated to Peterhouse, Cambridge. As the war developed, members of the Economics Department left the School to work for the government, reducing the number of staff remaining in Cambridge. One economist who was not invited to partake in the war effort was Hayek. He had become a British citizen in March 1938 and was not interned, but when in September 1939, he wrote to the Ministry of Information suggesting that his 'exceptional experience and somewhat special position might enable me to be of considerable help in connection with the organization of propaganda in Germany' his offer was ignored (Hayek quoted in Wapshott 2011: 190). Hayek remained in Cambridge, where he undertook a considerable teaching load and, when Robbins resigned to undertake government duties, Hayek became an editor of *Economica* together with Frederic Benham (see BLPES (d)).¹⁰

Paper rationing was introduced in Britain in February 1940, and in June 1940, a case had to be made to the Ministry of Supply that *Economica* was deserving of an allocation. The application was successful and a supply of paper was obtained, though it was limited and the minutes of the *Economica* Editorial Board (EEB) for 2 October 1941 report that the font size would be reduced to save paper. The minutes of the EEB for 18 June 1942 note further reductions in the font size, to Times New Roman 9 point for articles

¹⁰While the minutes of the EEB for 12 July 1940 state that, following the resignation of Robbins, the acting editors will be Hayek and Benham, Benham is not listed on the cover of the journal which, from 1941 to 1945, lists Hayek as the sole acting editor and Frank Paish continuing as assistant editor. From 1946 to 1957, the economic historian, T.S. Ashton, served as a second acting editor.

and Garamond 9 point for reviews. The same minutes show that the members of the EEB were thinking ahead to a successful outcome of the war and renewed academic linkages when they concluded that:

The information on the considerable increase in the circulation of *Economica* was noted with satisfaction. It was realised at the same time that only a limited further increase would be desirable in war-time, since a certain minimum reserve should be kept to supply libraries, etc. in enemy and enemy occupied territories after the war (BLPES (d)).

Betty Barron, the Secretary to the EEB, provided figures on the journal's subscriptions. The war had led to a considerable fall in numbers. From a circulation of about 1100 in 1939, subscriptions were down to 629 in November 1942. The distribution was mainly to Great Britain (345) and the USA (125), with Imperial support from India (43), Canada (22), South Africa (17), Australia (16) and New Zealand (seven). Neutral Switzerland accounted for 12 subscriptions (see *ibid.*).

With so many British economists involved in the war effort and many foreign economists isolated by the conflict, Hayek found space to publish a number of his own long articles that show his movement away from trying to solve problems in economic theory and towards more political and philosophical arguments.¹¹ In a review article of two books, Lippincott (1938) and Dickinson (1939), Hayek (1940) offers a detailed criticism of the pricing system under socialism as a mechanism for the efficient allocation of resources.

Next came a three-part article on 'The Counter-Revolution of Science' (Hayek 1941a, b, c) in which Hayek devotes 100 pages to presenting a somewhat discursive account of the life and teaching of Henri de Saint-Simon, who 'conceived some of the earliest and most fantastic plans for the reorganisation of society' (Hayek 1941a: 19), and his widespread influence, which included Marx, Engels and Lenin, among many others. Hayek concentrates on Saint-Simon's influence on Comte and explores 'the conviction which had led to his break with Saint-Simon: that the political reorganisation of society could be achieved only after the spiritual foundation had been laid by a reorganisation of all knowledge' (Hayek 1941c: 297). Following a detailed critique of Comte's ideas, Hayek notes that there are a number of points in which 'Comte's system resembles the later authoritarian

¹¹In *The Road to Serfdom* (Hayek 1944 [2001]: 15, fn. 1), Hayek wrote: "The author has made an attempt to trace the beginning of this development in two series of articles on "Scientism and the Study of Society" and "The Counter-Revolution of Science" which appeared in *Economica*, 1941-4'.

socialism which we associate with Prussia rather than socialism as we used to know it. In fact, in some passages, this resemblance with Prussian socialism, even down to the very words used, is really amazing' (ibid.: 315). Having given a range of examples, Hayek concludes that 'we might match almost every sentence with identical statements of recent German theoreticians who laid the intellectual foundations of the doctrines of the Third Reich' (ibid.: 316).¹²

There followed another three-part article entitled 'Scientism and the Study of Society' (Hayek 1942, 1943, 1944) that provided a further 68 pages of criticism of any attempt to use science to control society or the working of the free market. Here, in a style that anticipates much of *The Road to Serfdom*, Hayek writes in almost mystical tones about the operation of social processes:

The fact that no single mind can know more than a fraction of what is known to all individual minds sets limits to the extent to which conscious direction can improve upon the results of unconscious social processes. Man has not deliberately designed this process and has begun to understand it only long after it had grown up. But that something that not only does not rely on deliberate control for its working, but has not even been deliberately designed, should bring about desirable results, which we might not be able to bring about otherwise, is a conclusion the natural scientist seems to find difficult to accept (Hayek 1944: 37).

5 Recovery After the Second World War

Editorial Arrangements: Hayek and Paish continued their editorial roles after the war, with Ashton becoming a second editor from 1946 to 1957 and Paish being replaced by Alan Peacock in 1949. Hayek ceased to be an editor in 1950 and was replaced by Richard Sayers, who was an editor until 1960, when he was replaced by the then assistant editor, Basil Yamey, who served as an editor until 1973.¹³

Given the professorial structure in the Economics Department in the 1960s, the increasing use of mathematics in economic articles put a severe strain on refereeing within the School. At an EEB meeting on 5 March

¹²In a footnote, Hayek states that the 'German theoreticians' that he had in mind in particular were Spengler and Sombart (see Hayek 1941c: 316, fn. 3).

¹³For an account of his time as editor, see Yamey (2000).

1962, it was agreed that the editors should be able to use referees from outside LSE (see BLPES (i)). To strengthen the editorial team, Denis Sargan became a second editor from 1969 to 1973, with Harry Johnson added as a third editor from 1970 to 1973. In 1972, Yamey indicated to the EEB that he would like to retire from being an editor and the Board. He proposed a new arrangement such that editors need no longer be professors. This was accepted by the EEB (see BLPES (l)) and from 1974 junior members of the Economics Department served as editors of *Economica*. While there was a steady turnover of editors,¹⁴ one exception was Frank Cowell, who was an editor from 1982 until 2015, when the editorial arrangements were changed once again (see Appendix 2(a)).

Circulation: On 21 May 1951, Barron provided the EEB with the latest figures for subscriptions (in parentheses) for 1946 (1420), 1947 (1705), 1948 (1752), 1949 (1841) and 1950 (1964). The main contributions to the increase in subscriptions over this period came from Great Britain (up from 636 to 811), the USA (up from 220 to 321) and India (up from 76 to 113). Subscriptions from European countries were also growing again and, over the period, the number of subscriptions from Belgium, France, Germany, Holland, Italy, Poland, Spain and the USSR combined for each year between 1946 and 1950 was 142, 111, 133, 164 and 165, respectively (see BLPES (g)).

What was published? In November 1945, *Economica* published Richard Radford's article 'The Economic Organisation of a P.O.W. Camp', in which he wrote an account of his experience of a monetary system based on a cigarette currency.¹⁵ The analysis of price fluctuations as the supply of cigarettes changed over time, in the intervals between the arrival of Red Cross parcels containing cigarettes with the stock declining as some proportion was smoked, seemed to demonstrate the operation of the quantity theory of money. This became a very popular article and was much cited.

In contrast to the lack of initial interest in Coase (1937), Phillips's 1958 article (Phillips 1958), with its 'Phillips curve' showing an apparent trade-off between inflation and unemployment, attracted considerable attention.¹⁶

¹⁴As is clear from Appendix 2(a), the term of office for some editors was quite short. In many of these cases, the editor resigned when moving from LSE to a position at another institution. In other cases, editors were promoted to being professors while in post.

¹⁵Radford was in a P.O.W. camp containing about 2400 prisoners from 1941 until his liberation near the end of the war. At the time of its publication, authors were paid for their contributions and an EEB minute dated 16 January 1946 notes that Radford was paid £3.15.7 for his article (see BLPES (e)).

¹⁶For an account of the refereeing process through which this article was accepted, see Yamey (2000).

When Lipsey (1960) provided a simple method of ordinary least squares approximation to the original Phillips curve, an industry was set in motion to estimate curves for any country for which the relevant data were available.

In 1958, the EEB decided to publish a Consolidated Index to mark the publication of the first 25 volumes of *Economica* (New Series) and this appeared in 1959 (*Economica* 1959). While it is useful to have a listing of the names of authors and the titles of articles and books reviewed, the listing combines authors and titles in the same list, so that it is difficult to get a clear impression of any trends or concentrations of articles published in the different areas of economics.

The decision of another EEB to publish an update as *Economica: Index to Volumes 26–48 (1959–1981)* (*Economica* 1982) provided more information, as in addition to an index of authors and titles of articles, book reviews and other items, this book also contained a detailed Analytical Subject Index. This provided data on what was being published during this period.¹⁷

The number of items in two particular categories reflects certain distinctive characteristics of LSE: first, it was agreed at the separation in 1934 that *Economica* (New Series) would continue to publish material on economic history, and there are 265 items listed under this heading. Second, given the interest of Cannan and Robbins in the history of economic thought, it is not surprising to find 217 items in this category.

The category with the largest number of entries (351) was 'Country Studies', a heterogeneous collection of items over a wide range of topics that focused on a particular country, a collective institution or a geographical region. While the original Phillips curve article was published in 1958 and is therefore outside this period, it was still of interest and there were 39 items under this heading. On a smaller scale, *Economica's* contribution of items to ideological debates were somewhat mixed: on the one hand, Keynesian economics (six) versus monetarism (ten), but on the other, adaptive expectations (eight) versus rational expectations (four).

¹⁷The Analytical Subject Index provided information on articles, review articles, book reviews and public lectures listed together by categories. It occupied a total of 182 pages. To obtain some idea of the number of items published during the period, I selected 20 pages at random from the index and counted the number of entries. Entry numbers were relatively stable, with the 20 counts lying in a range between 20 and 27, with a modal value of 24, a median of 23 and an arithmetic average of 23. Taking this latter figure and the number of pages in the index, I calculated a total of 4186, or 4200 items as an approximation. I have used the term 'item' as it has not been possible with available resources to distinguish between articles, book reviews and any other items.

6 Cutting-Edge Technology: The T-Card Revolution

Accurate record-keeping in the pre-microcomputer age was difficult and there were some embarrassing moments when potential authors were kept waiting too long after an editor lost track of a submission and failed to chase up a referee for a report. These lapses occurred despite the determined efforts of three long-serving secretaries, Betty Barron, Irene Anderson and Jean Doran, to keep the *Economica* office running efficiently. The problem was greatly eased when *Economica* adopted a T-Card system in the 1970s. The shape of the cards explains the name of the system: T-Cards were 125 mm long and 80 mm wide at the base, but 15 mm from the top of the card, it widened out to 90 mm, so that it looked like a capital T, but with an exaggerated vertical section. The wider strip at the top of the card could be used to show the name of the author and title of the paper and information on the progress on the paper could be typed below. The cards could then be fitted into slots in a metal frame, with columns allocated to the various stages in the process from arrival, through refereeing to rejection, accept subject to revision, or publication. By colour coding the cards according to the time of arrival, it was now easy to see if a submission had begun to lag behind in the editorial process and take appropriate action. Similar colour coding reminded the review editor to chase up laggardly reviewers.

7 *Economica* Ceases to Be a House Journal

So far, the discussion has concentrated on what was published in *Economica* and the contribution LSE economists made to that publication. One may also ask the question: How important has *Economica* been for LSE economists? For many years, each of the annual issues of the *LSE Calendar* contained a listing of the publications by members of the academic staff in the previous session and this provides information on the articles published by LSE economists in journals other than *Economica*. As such, it is possible to see how much of their research output was being published in that journal. This information is summarised in Appendix 3. The listing of the publications of academic staff was suspended from the 1915/1916 session and only resumed for the 1931/1932 session. It was suspended again from 1941/1942 to 1945/1946 and finally suspended from 1997/1998.

For each of the sessions for which the information is available, Appendix 3 summarises: (1) the number of LSE economists publishing articles in a particular year, (2) the total number of articles published, (3) the number of these articles published in *Economica* and (4) the percentage of articles published in *Economica*. There are many year-to-year fluctuations in the percentage published in *Economica*, but the ‘Summary by Decades’ at the end of the table shows a clear trend. The average for the 1930s was 30.4%. The period immediately after the end of the Second World War, with its disruption of academic life and shortages of paper, led both to a reduction in the number of articles published and a reliance on LSE authors. As a result, the average for the period 1946/1947 to 1949/1950 went up to 47.1%. For the 1950s, the average of 30.5% was back to the level of the 1930s, and it then fell in each decade until in the 1990s there were years in which no LSE economist was listed as having published an article in *Economica*.¹⁸ Clearly, the journal had moved a long way from being the house publication of LSE, with its success depending more on its ability to attract research from non-School economists. Some idea of its appeal may be gauged by a number of studies that have been made of the ranking of various economics journals.

8 The Ranking of *Economica* Among Other Journals

Some independent evaluations of *Economica*’s ranking compared with other journals is provided by a number of studies. An early study by Eagly (1975) took a sample of 18 journals and constructed a square matrix showing citations, taking articles published in two periods (1961–1964 and 1970–1971). The row for a journal shows the number of its articles cited in other journals (sending information) and the column for that journal shows the number of articles it cites from other journals (receiving information). Eagly constructs a sending–receiving ratio for each journal and argues that

High values [of the ratio]—greater than 1.0 for example—suggest that the journal is a “feeder” of network information, while low values (less than 1.0)

¹⁸The decline in the number of LSE economists publishing articles in the 1990s reflects the fact that some of them were publishing material in the magazines issued by commercial banks, special supplements to newspapers, publications of the Institute of Economic Affairs and other such bodies; I did not count these as journal articles. Much research material also appeared in the form of the working papers published by the various research centres at LSE before being published as chapters in books or in volumes of conference papers.

suggest that the journal is a “storer” of network information. The high values may perhaps be interpreted as indicative of the journal’s innovative role as a wellspring of seminal ideas in the discipline as well as an index of the journal’s relative prestige (*ibid.*: 880).

On this measure, *Economica* is ranked fourth for 1961–1964 period and eighth for the 1970–1971 period. Based on this and other criteria, *Economica* is included with six other journals (*American Economic Review* (*AER*), *Economic Journal* (*EJ*), *Econometrica* (*Ecom.*), *Journal of Political Economy* (*JPE*), *Quarterly Journal of Economics* (*QJE*) and *Review of Economics and Statistics* (*R.E. Stat.*)) as the core journals feeding the rest of the network.

A.W. (Bob) Coates published two sociological studies of economics journals which included *Economica*. In Coates (1971a [1993]), the author’s sample of journals consisted of the *AER*, *Economica* (*Eca.*), *EJ*, *JPE* and *QJE*; articles published between 1886 and 1959 were analysed. Two rankings are of interest: in terms of journal articles chosen for inclusion in the American Economic Association (AEA) ‘Readings’, the rankings were *AER* (37), *QJE* (32), *EJ* (30), *JPE* (29) and *Eca.* (24). The second examined the number of citations from a number of journals that appeared in the AEA’s two *Surveys of Contemporary Economics* and the three AEA *Surveys of Economic Theory*. The sample of journals was expanded to include *Econometrica* (*Ecom.*), *Oxford Economic Papers* (*OEP*), *Review of Economics and Statistics* (*R.E.Stat.*) and *Review of Economic Studies* (*R.E.Stud.*). The total for each of the *Surveys* and the overall totals were: *AER* (157 + 117 = 274), *EJ* (84 + 163 = 247), *R.E.Stud.* (52 + 152 = 204), *R.E.Stat.* (89 + 112 = 201), *JPE* (91 + 100 = 191), *Ecom.* (87 + 102 = 189), *QJE* (76 + 73 = 149), *Eca.* (45 + 60 = 105) and *OEP* (10 + 40 = 50). That *R.E.Stud.* should rank so highly in the *Economic Theory* surveys is perhaps not surprising given its objectives, but what is striking is that the *EJ*, a generalist journal, should also score so highly in this category.

In his second article, Coates (1971b [1993]) presents a comparison between the *EJ* and *Economica* for articles by British-based authors published by decade between 1920 and 1969. This showed that over this period relatively few (less than 10%) of the articles published in *Economica* were written by Cambridge or Oxford authors and the majority (70% initially but dropping to 58% by the end of the period) were by London authors, confirming a finding reported earlier in this chapter. This article is also of interest in reporting what Hicks and Robbins had to say about the choice of journals available in the 1930s. Hicks reported:

Looking back over my own articles, I would say that one naturally began by trying to get something published in the *EJ*, as that was a source of prestige (I suppose I got what the Americans call “tenure” as a result of my first *EJ* article). But after that it was natural to publish in the place where discussions on the matter [one] was writing [on] were going on. There was a great deal that came out of LSE seminars and that naturally went in *Economica* until it got too bulky and then *RES* [*Review of Economic Studies*] came in as an overflow (ibid.: 194).

Robbins noted the importance of payments to impoverished academics:

[T]he *EJ* paid far more than we did per page and, although by 1934 or thereabouts this objective began to dominate, I would still feel obliged in conscience to tell lecturers at the School who brought me articles that, if they were hard up, they had better go first to the *EJ* because they paid better (ibid.).

Further information on the ranking of journals based on citations is presented in Liebowitz and Palmer (1984), Laband and Piette (1994) and Ritzberger (2008). Some results based on these studies are summarised in Appendix 4.¹⁹

The article by Liebowitz and Palmer provides an explanation of the various ways in which journal citations can be weighted and presents seven rankings based on different criteria for 107 or 108 journals. In the seven rankings, *Economica* is ranked eight, 11, 20, 28, 30, 35 and 44, with an average ranking of 25. The *AER*, *EJ*, *JPE*, *QJE*, *R.E.Stud.* and *Econometrica* tend to come out well on all the rankings and, on average, are in the top 20, but other generalist journals do less well, with the following averages: *Kyklos* (52), *Manchester School* (54), *OEP* (46) and *Scandinavian Journal of Economics* (32). What is striking is how newer journals devoted to specialised branches of economics, such as the *Journal of Finance*, *Journal of Monetary Economics*, *Journal of Economic Theory* and *Journal of Financial Economics*, appear high in the rankings.

The same pattern is confirmed in the other two studies, which include rankings based on more recent citations: the six journals listed above (*AER*,

¹⁹Liebowitz and Palmer present seven rankings based on three periods and five measures of citations; Laband and Piette offer six rankings based on three periods and two measures, while Ritzberger presents several rankings based on his analysis and summaries of earlier studies. This quantity of information is difficult to summarise, so in Appendix 4, I have quoted the ranking from each study based on the most recent period and using the authors' preferred method of ranking. For each study, the top 20 journals are listed, together with the ranking of *Economica* and seven other UK and European journals for comparison.

etc.) all appear in the top 20, with the remaining entries being journals specialising in particular branches of economics or areas outside economics.

This development is also illustrated by the results presented in Han Kim et al. (2006), which presents a list of 146 articles with over 500 citations as of June 2006. While it is sad to report that no articles published in *Economica* appear on this list, the distribution by journals is interesting. Articles from 18 journals appear in the ranking: *Econometrica* has the most, with 31 citations, followed by *JPE* (27) and *AER* (18). But the fourth-ranked journal is *Journal of Financial Economics* (12) and six other specialist journals contribute a further 24 citations combined, bringing the number up to 36, or 25% of the total.²⁰

Looking at the ranking for the UK and European journals in Appendix 5, it is clear that the *Review of Economic Studies* is the outstanding journal, appearing in the top ten in all three rankings reported. This is followed by the *Economic Journal*, with *Economica* either third or fourth.

9 Conclusion

As a gauge of the current links between LSE economists and *Economica*, I visited the LSE website on the 18 June 2017 and selected the entry for the Department of Economics. There were 66 economists listed there as members of the Department and for 57 of them it was possible, by checking their 'Personal Websites', to discover the articles they chose to list on their CVs and the journals in which they were published. The result of this survey was that the 57 economists listed 906 articles published in 124 different journals. The number of articles per journal was collected and the results for the 37 journals with six or more citations are presented in Appendix 5. As one might expect, the prestigious generalist journals (*AER*, *EJ*, *QJE*, *Econometrica* and *R.E. Stud.*) figure high on the list, but specialist journals are also well represented. Nineteen articles were published in *Economica*, which appears as equal thirteenth in the ranking. The number of articles covered in Appendix 5 is 717, and the remaining 189 articles were published in 87 other journals, most of which were specialist by nature. These results confirm the earlier finding that *Economica* is no longer a house journal as

²⁰The *EJ* has four citations, including Davidson et al. (1978), a well-known piece of LSE research, with 537 citations.

most of the articles published are not written by LSE economists, who now tend to publish in other journals.

In January 2016, there was an editorial reorganisation at *Economica* and Volume 83 appeared under new editorial management, with the following mission statement:

This is the first issue edited by a new Editorial Team comprising Oriana Bandiera, Tim Besley, Francesco Caselli, Maitreesh Ghatak, Stephen Machin, Ian Martin, Gianmarco Ottaviano and John Van Reenen. The Chair of the Board of *Economica* is now the Head of the Department of Economics (currently Professor Leonardo Felli) and the Professors of Economics at LSE are the journal board. *Economica* will remain an international journal covering research in all branches of economics published on behalf of the LSE Economics Department. The *Economica* Editorial Team welcomes high-quality contributions from all parts of the international research community which are of interest to general readers, as we work towards increasing the journal's standing and reputation in the future (*Economica* 2016: Inside front cover).

Only time will tell whether a journal aimed at the general economic reader will succeed in attracting interesting high-quality papers, but Barro and McCleary's statistical analysis of the making of Catholic Saints between 1590 and 2012 (Barro and McCleary 2016) suggests that such papers do exist.

Appendix 1: Distribution of Articles and Reviews in *Economica* (Old Series)

Date	Issue no.	Distribution of articles				Reviews	
		Economics	Statistics	Other	Total	Number	Pages (%)
Jan. 1921	1	3	0	6	9	0	0
May 1921	2	2	2	5	9	0	0
Oct. 1921	3	2	2	4	8	0	0
Jan. 1922	4	0	3	5	8	0	0
Jun. 1922	5	2	0	6	8	0	0
Oct. 1922	6	1	2	5	8	0	0
Jan. 1923	7	0	2	6	8	0	0
Jun. 1923	8	4	1	3	8	11	25
Nov. 1923	9	2	1	6	9	16	25
Feb. 1924	10	2	1	6	9	19	18
Jun. 1924	11	4	0	5	9	21	26

(continued)

Date	Issue no.	Distribution of articles				Reviews	
		Economics	Statistics	Other	Total	Number	Pages (%)
Nov. 1924	12	3	0	6	9	10	15
Mar. 1925	13	3	1	5	9	16	20
Jun. 1925	14	1	3	5	9	15	15
Nov. 1925	15	1	0	7	8	14	20
Mar. 1926	16	1	1	5	7	14	20
Jun. 1926	17	4	0	5	9	12	24
Nov. 1926	18	1	0	6	7	14	26
Mar. 1927	19	2	0	5	7	18	26
Jun. 1927	20	3	0	5	8	13	29
Dec. 1927	21	2	1	3	6	13	22
Mar. 1928	22	2	1	3	6	9	15
Jun. 1928	23	1	1	3	5	9	30
Dec. 1928	24	0	2	3	5	11	16
Apr. 1929	25	1	1	5	7	19	32
Jun. 1929	26	1	0	5	6	15	27
Nov. 1929	27	0	0	6	6	15	26
Mar. 1930	28	2	0	3	5	16	39
Jun. 1930	29	3	0	2	5	11	36
Nov. 1930	30	1	0	4	5	26	41
Feb. 1931	31	2	0	3	5	9	17
May 1931	32	2	0	2	4	12	19
Aug. 1931	33	1	0	4	5	7	36
Nov. 1931	34	4	0	3	7	17	38
Feb. 1932	35	3	0	3	6	8	17
May 1932	36	2	0	3	5	15	27
Aug. 1932	37	4	0	2	6	5	24
Nov. 1932	38	2	0	2	4	16	35
Feb. 1933	39	2	0	4	6	12	29
May 1933	40	3	1	2	6	12	26
Aug. 1933	41	2	0	3	5	13	37
Nov. 1933	42	3	0	2	5	13	26
	Total	84	26	176	286	476	n/a
	Average	2.0	0.6	4.2	6.8	13.6	25.8
	% of total	29	9	62	100		

Appendix 2(a): List of Editors of *Economica* (New Series) by Date

Date	AssEd/RE	
1934	Robbins	Powell
1935	Robbins	Powell

(continued)

Date				AssEd/RE
1936	Robbins	Powell		Paish
1937	Robbins	Powell		Paish
1938	Robbins	Powell		Paish
1939	Robbins	Powell		Paish
1940	Robbins	Powell		Paish
1941	Hayek			Paish
1942	Hayek			Paish
1943	Hayek			Paish
1944	Hayek			Paish
1945	Hayek			Paish
1946	Hayek	Ashton		Paish
1947	Hayek	Ashton		Paish
1948	Hayek	Ashton		Paish
1949	Hayek	Ashton		Paish
				Peacock
1950	Hayek	Ashton		Peacock
1951	Sayers	Ashton		Peacock
1952	Sayers	Ashton		Peacock
1953	Sayers	Ashton		Peacock
1954	Sayers	Ashton		Peacock
1955	Sayers	Ashton		Peacock
1956	Sayers	Ashton		Peacock
1957	Sayers	Ashton		Peacock
				Yamey
1958	Sayers			Yamey
1959	Sayers			Yamey
1960	Sayers			Yamey
	Yamey			Peston
1961	Yamey			Peston
1962	Yamey			Peston
1963	Yamey			Corry
1964	Yamey			Corry
1965	Yamey			Corry
				Peston
1966	Yamey			Peston
				Corry
1967	Yamey			Corry
1968	Yamey			Corry
				Thomas
1969	Yamey	Sargan		Thomas
1970	Yamey	Sargan	Johnson	Thomas
1971	Yamey	Sargan	Johnson	Thomas
1972	Yamey	Sargan	Johnson	Thomas
1973	Yamey	Sargan	Johnson	Thomas
1974	Miller	Richardson	Wallis	Thomas
1975	Miller	Richardson	Wallis	Thomas
1976	Miller	Richardson	Wallis	Thomas
1977	Jackman	Richardson	Smith	Thomas

(continued)

Date					AssEd/RE
1978	Jackman	Shorrocks	Smith		Thomas
1979	Jackman	Shorrocks	Smith		Thomas
1980	Jackman	Shorrocks	Glaister		Thomas
1981	Jackman	Pissarides	Glaister		Thomas
1982	Cowell	Pissarides	Davidson		Thomas
1983	Cowell	Pissarides	Davidson		Thomas
1984	Cowell	De Meza	Davidson		Thomas
1985	Cowell	De Meza	Davidson		Thomas
1986	Cowell	De Meza	Davidson		Thomas
1987	Cowell	De Meza	van der Ploeg		Thomas
					Webb
1988	Cowell	De Meza	van der Ploeg		Webb
		Webb			
1989	Cowell	Webb			Webb
1990	Cowell	Webb	Schankerman		Webb
1991	Cowell	Webb	Schankerman		Webb
1992	Cowell	Webb	Schankerman		Webb
1993	Cowell	Webb			Webb
1994	Cowell	Webb			Webb
1995	Cowell	Webb			Webb
1996	Cowell	Webb	Manning		Webb
1997	Cowell	Webb	Manning		Webb
1998	Cowell	Webb	Manning		Webb
1999	Cowell		Manning		Witztum
2000	Cowell	Ellingsen	Manning		Witztum
2001	Cowell	Ellingsen	Manning		Witztum
2002	Cowell	Ellingsen	Manning		Witztum
2003	Cowell	Ellingsen	Manning		Witztum
2004	Cowell	Ellingsen	Manning		Witztum
2005	Cowell	Ellingsen	Manning		Witztum
2006	Cowell	Ellingsen	Manning	Michaelides	Witztum
			Caselli		
2007	Cowell	Ellingsen	Caselli	Michaelides	Witztum
2008	Cowell	Sørensen	Caselli	Michaelides	Witztum
2009	Cowell	Sørensen	Caselli	Michaelides	Witztum
2010	Cowell	Sørensen	Caselli	Michaelides	Witztum
2011	Cowell	Sørensen	Sheedy	Michaelides	Witztum
2012	Cowell	Sørensen	Benigno	Michaelides	Witztum
2013	Cowell	Sørensen	Benigno	Edlund	Witztum
2014	Cowell		Benigno	Edlund	Witztum
2015	Cowell		Benigno	Edlund	Witztum
2016	A new editorial arrangement was launched with eight Editors, 20 Associate Editors 'and all Members of the LSE Economics Department'				
2017	As for 2016				

Key: AssEd/RE = Assistant Editor/Review Editor

Appendix 2(b): Alphabetical Listing of Editors of *Economica*

Name	Role(s)
Ashton, T.S.	E: May 1946–May 1957
Benigno, G.	E: Feb. 2012–Dec. 2015
Caselli, F.	E: Nov. 2006–Oct. 2010
Corry, B.A.	AssE: Feb. 1963–May 1965; RE: Nov. 1966–Aug. 1968
Cowell, F.	E: Feb. 1982–Dec. 2015
Davidson, J.E.	E: Feb. 1982–Nov. 1986
de Meza, D.	E: Feb. 1984–Aug. 1988
Edlund, L.	E: Feb. 2013–Dec. 2015
Ellingsen, T.	E: Nov. 2000–Nov. 2007
Glaister, S.	E: Feb. 1980–Nov. 1981
Hayek, F.A. von	E: Nov 1941–May 1950
Jackman, R.A.	E: Feb. 1977–Nov. 1981
Johnson, H.G.	E: Feb. 1970–Feb. 1974
Manning, A.	E: Nov. 1996–Aug. 2006
Michaelides, A.	E: Nov. 2006–Dec. 2012
Miller, M.H.	E: Feb. 1974–Nov. 1976
Paish, F.	AssE: Feb. 1934–May 1949
Peacock, A.T.	AssE: Nov. 1949–Feb. 1957
Peston, M.H.	AssE: May 1960–Nov. 1962; AssE: Aug. 1965–Aug. 1966
Pissarides, C.	E: Feb. 1981–Nov. 1983
Powell, E.	E: Feb. 1934–May 1940
Richardson, R.R.	E: Feb. 1974–Nov. 1977
Robbins, L.	E: Feb. 1934–May 1940
Sargan, J.D.	E: Feb. 1969–Feb. 1974
Sayers, R.S.	E: Aug. 1950–Feb. 1960
Schankerman, M.	E: Feb. 1990–Nov. 1992
Sheedy, K.	E: Jan. 2011–Dec. 2011
Shorrocks, A.	E: Feb. 1978–Nov. 1980
Smith, M.A.M.	E: Feb. 1977–Nov. 1979
Sørensen, P.N.	E: Feb. 2008–Dec. 2013
Thomas, J.J.	RE: Nov. 1968–Aug. 1987
van der Ploeg, R.	E: Feb. 1987–Nov. 1988
Wallis, K.F.	E: Feb. 1974–Nov. 1976
Webb, D.	RE: Nov. 1987–Nov. 1999; E: Nov. 1988–Nov. 1999
Witztum, A.	RE: Feb. 1999–Dec. 2015
Yamey, B.S.	AssE: May 1957–May 1960; E: May 1960–Feb. 1974

Key: AssE = Assistant Editor; E = Editor; RE = Review Editor

Appendix 3: Articles Published in *Economica* by LSE Economists by Year (1931/1932–1997/1998)

Year	1	2	3	4(%)	Year	1	2	3	4(%)
1931/1932	7	14	4	28.6					
1932/1933	9	18	9	50.0	1980/1981	18	35	2	5.8
1933/1934	8	17	5	29.4	1981/1982	16	41	5	12.2
1934/1935	11	24	9	37.5	1982/1983	22	45	5	11.1
1935/1936	14	30	10	33.3	1983/1984	26	60	7	11.7
1936/1937	9	20	5	25.0	1984/1985	26	54	3	5.6
1937/1938	14	43	8	18.6	1985/1986	21	45	1	2.2
1938/1939	10	21	3	14.3	1986/1987	23	49	1	2.0
1939/1940	8	20	10	50.0	1987/1988	25	61	4	6.6
Publications not reported during this period					1988/1989	28	75	2	2.7
1946/1947	5	5	2	40.0	1989/1990	21	49	3	6.1
1947/1948	8	12	6	50.0	1990/1991	23	51	1	2.0
1948/1949	8	11	5	45.5	1991/1992	25	88	1	1.0
1949/1950	4	6	3	50.0	1992/1993	29	54	4	7.4
1950/1951	11	17	5	29.4	1993/1994	18	35	0	0.0
1951/1952	10	18	8	44.4	1994/1995	14	35	0	0.0
1952/1953	15	29	11	37.9	1995/1996	11	28	0	0.0
1953/1954	14	19	4	21.1	1996/1997	8	22	0	0.0
1954/1955	19	33	7	21.2	1997/1998	8	36	0	0.0
1955/1956	15	26	6	23.1	Reporting of publications by staff discontinued				
1956/1957	12	17	7	41.2					
1957/1958	16	26	9	34.6	Summary by Decades				
1958/1959	18	31	10	32.3	1931/1940	–	207	63	30.4
1959/1960	15	33	9	29.0	1946/1950	–	34	16	47.1
1960/1961	17	29	5	17.2	1951/1960	–	249	76	30.5
1961/1962	27	46	11	23.9	1961/1970	–	314	77	24.5
1962/1963	21	39	12	30.7	1971/1980	–	401	44	11.0
1963/1964	12	20	4	20.0	1981/1990	–	514	33	6.4
1964/1965	16	31	7	22.6	1991/1998	–	349	5	1.4
1965/1966	19	25	6	24.0					
1966/1967	12	25	6	24.0					
1967/1968	13	18	4	22.2					
1968/1969	15	36	12	33.3					
1969/1970	18	45	10	22.2					
1970/1971	16	43	4	9.3					
1971/1972	16	33	7	21.2					
1972/1973	16	42	4	9.5					
1973/1974	19	41	2	4.9					
1974/1975	20	46	6	13.0					

(continued)

Year	1	2	3	4(%)	Year	1	2	3	4(%)
1975/1976	23	47	4	8.5					
1976/1977	26	51	6	11.8					
1977/1978	18	39	6	15.4					
1978/1979	18	34	4	11.8					
1979/1980	15	25	1	4.0					

Source: Volumes of the LSE Calendar from 1931/1932 to 1997/1998

Key

1. Number of LSE economists publishing articles that year
2. Total number of articles published
3. Number of articles published in *Economica*
4. Percentage of articles published in *Economica*

Appendix 4: Ranking of Journals Based on Citations

	Studies		
No. Journals	Liebowitz and Palmer (1984)	Laband and Piette (1994)	Ritzberger (2008)
Citation period(s)	107	130	261
Top 20 journals	All articles ever published by journals + 1975–1979	1965–1969; 1975–1979 and 1985–1989	2003–2005
	Based on impact-adjusted citations to articles published 1975–1979	Based on impact-adjusted citations to articles published 1985–1989	Based on the invariant method (Ritzberger: 407). For 2003–2005
1	<i>AER</i>	<i>J. Financial Economics</i>	<i>Econometrica</i>
2	<i>JPE</i>	<i>Econometrica</i>	<i>QJE</i>
3	<i>Econometrica</i>	<i>JPE</i>	<i>R.E.Stud.</i>
4	<i>J. Monetary Economics</i>	<i>J. Monetary Economics</i>	<i>JPE</i>
5	<i>J. Economic Theory</i>	<i>QJE</i>	<i>Inter. Econ. Review</i>
6	<i>R.E. Stud.</i>	<i>R.E. Stud.</i>	<i>Journal of Finance</i>
7	<i>Inter. Econ. Review</i>	<i>AER</i>	<i>J. Monetary Economics</i>
8	<i>Bell J. Economics</i>	<i>Bell J. Economics</i>	<i>AER</i>
9	<i>J. of Finance</i>	<i>J. Economic Theory</i>	<i>J. Economic Theory</i>

(continued)

Studies			
10	<i>J. of Econometrics</i>	<i>J. of Finance</i>	<i>J. Financial Economics</i>
11	<i>Scand. J. Economics</i>	<i>J. Economic Literature</i>	<i>Rev. Financial Studies</i>
12	<i>Brookings Papers</i>	<i>J. Acc. Economics</i>	<i>J. Economic Growth</i>
13	<i>J. Public Economics</i>	<i>J. Economic Perspectives</i>	<i>J. of Econometrics</i>
14	<i>J. Financial Economics</i>	<i>J. of Business</i>	<i>J. Internat. Economics</i>
15	<i>R. Econ. Stats.</i>	<i>J. Mathematical Econ.</i>	<i>Game Econ. Behav.</i>
16	<i>J. Amer. Stats. Assoc.</i>	<i>J. of Econometrics</i>	<i>R. Econ. Stats.</i>
17	<i>QJE</i>	<i>Brookings Papers</i>	<i>J. of Labour Econ.</i>
18	<i>J. of Human Resources</i>	<i>J. of Labour Economics</i>	<i>J. of Bus.Econ. Stats.</i>
19	<i>J. of Economic Literature</i>	<i>J. of Finan. Quant. Anal.</i>	<i>J. of Public Economics</i>
20	<i>EJ</i>	<i>Inter. Econ. Review</i>	<i>J. Risk & Uncertainty</i>
Other journals			
	<i>Economica</i>	28	45
	<i>EJ</i>	20	28
	<i>Euro. Economic Review</i>	37	49
	<i>Kyklos</i>	38	66
	<i>Man. School</i>	42	72
	<i>OEP</i>	49	64
	<i>R.E. Stud.</i>	6	6
	<i>Scan. J. of Econ.</i>	11	51

Appendix 5: Journals Cited in LSE Economists' CVs in 2017

Ranking	Journal title	Number of articles
1	<i>American Economic Review</i>	80
2	<i>Economic Journal</i>	54
3	<i>Quarterly Journal of Economics</i>	52
4=	<i>Journal of Econometrics</i>	39
4=	<i>Journal of the European Economic Association</i>	39
6	<i>Econometrica</i>	32
7	<i>Review of Economic Studies</i>	31

(continued)

Ranking	Journal title	Number of articles
8=	<i>European Economic Review</i>	27
8=	<i>Journal of Public Economics</i>	27
10	<i>Econometric Theory</i>	24
11=	<i>Journal of Labour Economics</i>	22
11=	<i>Journal of Monetary Economics</i>	22
13=	<i>Economica</i>	19
13=	<i>Journal of Political Economy</i>	19
15	<i>American Economic Journal: Microeconomics</i>	16
16=	<i>Journal of Development Economics</i>	15
16=	<i>Journal of Economic Dynamics and Control</i>	15
16=	<i>RAND Journal of Economics</i>	15
16=	<i>Review of Economics and Statistics</i>	15
20	<i>Journal of Economic Theory</i>	13
21	<i>Scandinavian Journal of Economics</i>	12
22=	<i>American Economic Journal: Macroeconomics</i>	10
22=	<i>British Journal of Industrial Relations</i>	10
24=	<i>American Economic Journal: Economic Policy</i>	9
24=	<i>Annals of Statistics</i>	9
24=	<i>Journal of Economic Perspectives</i>	9
24=	<i>Journal of International Economics</i>	9
24=	<i>Labour Economics</i>	9
29=	<i>American Economic Journal: Applied Economics</i>	8
29=	<i>Econometrics Journal</i>	8
29=	<i>Fiscal Studies</i>	8
29=	<i>Oxford Economic Papers</i>	8
33=	<i>Oxford Review of Economic Policy</i>	7
33=	<i>Theoretical Economics</i>	7
35=	<i>Journal of Applied Econometrics</i>	6
35=	<i>Journal of Law and Economics</i>	6
35=	<i>Quantitative Economics</i>	6
	Total	717

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Archival Material

Though not listed on the website of the British Library of Political and Economic Science (BLPES), the LSE Archive contains material on the meetings of the *Economica* Editorial Board. (I am extremely grateful to Sue Donnelly, the LSE Archivist, for drawing this material to my attention.) The relevant references (and the periods covered) are:

- BLPES (a). LSE/CENTRAL FILING REGISTRY/122/25/A(Box 239) [1920–1933].
 BLPES (b). LSE/CENTRAL FILING REGISTRY/122/25/B(Box 239) [1933–1935].

BLPES (c). LSE/CENTRAL FILING REGISTRY/122/25/C(Box 239) [1935–1937].
 BLPES (d). LSE/CENTRAL FILING REGISTRY/122/25/D(Box 240) [1938–1945].
 BLPES (e). LSE/CENTRAL FILING REGISTRY/122/25/E(Box 240) [1944–1949].
 BLPES (f). LSE/CENTRAL FILING REGISTRY/122/25/F(Box 240) [1949–1951].
 BLPES (g). LSE/CENTRAL FILING REGISTRY/122/25/G(Box 240) [1951–1954].
 BLPES (h). LSE/CENTRAL FILING REGISTRY/122/25/H(Box 240) [1954–1958].
 BLPES (i). LSE/CENTRAL FILING REGISTRY/122/25/I(Box 241) [1958–1963].
 BLPES (j). LSE/CENTRAL FILING REGISTRY/122/25/J(Box 241) [1965–1968].
 BLPES (k). LSE/CENTRAL FILING REGISTRY/122/25/K(Box B15) [1966–1969].
 BLPES (l). LSE/CENTRAL FILING REGISTRY/122/25/L(Box B15) [1970–1972].

Within the boxes, the items are neither listed in order nor numbered.

There is also:

BLPES (c2) LSE/UNREGISTERED/32/33/ [Minutes (2/10/1938+ Agenda (2/1/1938)].

BLPES BEVERIDGE/2B/22/5 Title: Correspondence, June–July 1923.

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Part II

Some LSE Economists



7

Edwin Cannan (1861–1935)

Keith Tribe

1 Introduction

When I first met him at the Ipswich meeting of the British Association in 1895, he was with two other bicyclists, Edgeworth and Gonner. There was no sign of his earlier illness; he was at any time prepared to ride a hundred miles, and the story is told that when he failed to find a book at the Bodleian, he merely said “–then I must go on to the British Museum” (Bowley 1935: 392).

As we talked I gradually got to know you were a cyclist of a different order from myself; but it was not till the evening that I learnt from Edgeworth how supreme a master of the craft you are ... I had heard of you as delicate in the chest. I am very grateful you are able to take such strong exercise. I trust it promises a long life of great work for economics (Marshall to Cannan, 3 July 1896, reprinted in Whitaker 1996: 170).

There is every indication that Alfred Marshall is here writing of the first time he and Edwin Cannan met socially, and out of context it might read as a comment from the Cambridge Professor of Political Economy

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addressed to a young man just finding his way in the world (see Kadish 1983: 62). Marshall was nearly twenty years older than Cannan, but at the time, Cannan was 35-years-old. Their first contact had come three years earlier, when Marshall had sent a short note thanking Cannan for the copy of *History of the Theories of Production and Distribution* that he had sent (Marshall to Cannan, 5 May 1893, reprinted in Whitaker 1996: 94). That book was a slow starter, just like its author: initially, it sold very slowly, did eventually go into a second edition in 1903, and then the third edition of 1917 was reprinted in quick succession, in 1920, 1922, and 1924. Cannan's scholarly career was in fact just about to blossom when he met Marshall, for he had been working on his ground-breaking edition of Adam Smith's *Lectures on Justice, Police, Revenue and Arms* (Smith 1896), on the back of which he was asked by Methuen to produce a modern edition of *Wealth of Nations*; published in 1904 (Smith 1904), a work that remained the standard edition until the appearance of the Glasgow edition in 1976 (Smith 1976). Academically, it was the same story: Cannan was appointed London University Professor of Political Economy in 1907 in his mid-forties and then for more than twenty years taught 'Principles of Economics' at the London School of Economics (LSE) to second- and third-year undergraduates. He retired from the School in 1926 and was succeeded by Allyn Young, whose early death led to the appointment of Lionel Robbins as Professor of Political Economy. Together with Arnold Plant, Professor of Commerce, Robbins dominated the School through the 1930s and 1940s—both of them having been taught their 'principles' by Cannan.

Nonetheless, Cannan was, and remained, an Oxford economist from the generation of the 1880s, like his contemporaries William Ashley and E.C.K. Gonner; all of them marked by the teaching of Arnold Toynbee, and by his early death.¹ By 1891 Gonner was the founding Brunner Professor of Political Economy at Liverpool, and in 1892 Ashley was appointed founding Professor of Economic History at Harvard. Although it took Cannan a great deal longer to find his defining part in the story of British economics, his role was eventually more significant than either, for he retained an allegiance to a formal, Jevonian, approach to economic argument that he took with him to LSE. Always sceptical, sometimes scornful, of Marshallian economics, with its emphasis on an empirical engagement with the 'ordinary

¹On Toynbee, see Kadish (1986).

business of life', Cannan's robustly liberal approach to economic analysis came to define LSE economics from the 1920s onwards.²

2 Biography

Edwin Cannan was born in Funchal, Madeira, on 3 February 1861. His father David Cannan, originally from Kirkcudbrightshire, had retired in 1858 as agent for an Australian manufacturer; his mother Jane suffered poor health, which had in the later 1850s brought about a move first to Bournemouth, and then to Madeira, where she died shortly after giving birth to Edwin. Returning to Bournemouth, Edwin was initially brought up by his father's sister, Agnes, who also kept house for his elder brother Charles and his father. When in 1864 Agnes died, another sister, Margaret, took her place. Then, in 1868, David Cannan remarried, to Eliza Weekes, and the family moved to the school she owned, Aschan House. Although Eliza died the following year Edwin attended Aschan House before moving in 1876, at the age of fifteen, to Clifton College. Then, in 1877, David Cannan died, and since Edwin's mother had inherited a significant legacy in 1854, the teenage brothers Charles and Edwin now became young men of independent means. Charles (1858–1919) would make his mark in the world as Secretary to the Delegates of Oxford University Press; the brothers remained Oxford men to the end. Both were City councillors. Charles hardly ever left Oxford after he was elected Fellow of Trinity College in 1884; Edwin's appointment at LSE was always part-time, and he commuted to the School from his home in Oxford.

Edwin entered Balliol College, Oxford in January 1881 as a Commoner; already nearly twenty, but with a frail constitution, he lived with his aunt Margaret in rooms at 24 St Giles. During his second year at Oxford his health deteriorated and he abandoned plans to sit for an Honours degree. He took a Pass Degree in 1884, and then went on a long sea voyage with his aunt (see Kadish 1982: 10). Once back at Oxford he wrote an essay on the Duc de Saint-Simon (1675–1755), winning the Lothian Prize in 1885. Then his attention turned to political economy and he entered another essay for the Cobden Prize in 1886. Here he was unsuccessful, but he revised the

²Apel (1961: 1) counts T.E. Gregory, Lionel Robbins, Arnold Plant, Frederick Benham, William H. Hutt and Frank Paish as the foremost students of Cannan, citing as authority an article by Hayek (1951: 335). My thanks to Maxime Desmarais-Tremblay for alerting me to this source.

first part of the essay and then published it as *Elementary Political Economy* in 1888. The following year he presented some of his ideas in a paper to the Fabian Society, and it was through meeting Sidney Webb that he was asked in 1895 to lecture at the newly founded LSE.³ By this time he was a dedicated ‘bicyclist’, as the above epigraphs attest, and the vigour he subsequently brought to his writing and teaching belies the fragility of his youth.

3 Political Economy in the 1880s

In the 1880s political economy was in Oxford primarily taught in the context of the Modern History degree, and this provides some explanation of why Cannan’s Lothian essay should have been devoted to a historical biography. Nonetheless, Oxford was at this time at the heart of political economy in England⁴; after the death of Arnold Toynbee in 1883 Alfred Marshall moved there from Bristol, lining himself up as successor to the Drummond Professor of Political Economy, Bonamy Price. Born in 1807, elected to the Drummond Chair in 1868, Price was not expected to occupy it much longer; but then in 1884 Fawcett, the Cambridge Professor of Political Economy, died, and instead Marshall moved in 1885 to Cambridge and built the immediate future of English economics there (see Kadish 1983: 64–65).

One element of this immediate future was the occultation, perhaps deliberate on Marshall’s part, of the influence of Stanley Jevons. During the 1880s Marshall was still slowly detaching himself from the economics of John Stuart Mill, who was not a ‘classical economist’ in the sense usually presumed today and was a clear influence on Alfred and Mary’s *Economics of Industry* of 1879. Given some of the internal incoherency of *Economics of Industry* (see Tribe 2014: 54–56), there are strong grounds for suspecting that Alfred Marshall’s later efforts to suppress that work were related more to the evidence it provided of how slowly he had detached himself from Mill than to any supposed defects of Mary Marshall’s contribution, the motive usually attributed to him. Jevons had died in 1882, but before doing so had sparked an interest in political economy in his Hampstead neighbour,

³See Hayek (1946) which remains a useful guide to the improvisational nature of the early years of the School, when all teachers were part-time and courses were being rapidly developed in response to demand from students.

⁴Kadish (1982) remains a very reliable guide to this period.

Francis Edgeworth, and *Theory of Political Economy* would also be the point from which Philip Wicksteed developed his own understanding of marginal utility (his own usage).⁵ Throughout the 1880s Jevons's formal and analytical approach to the problems of political economy was accepted as the current standard; this was true both of Fabian circles in London and of those in Oxford who took an interest in political economy (see Kadish 1983: 66). Evidence for the latter point can be found in Gonner's own introductory textbook, published in the same year as Cannan's *Elementary Political Economy*, and also Wicksteed's *Alphabet of Economic Science*.⁶ Likewise, William Ashley's later criticisms of contemporary economics hinged on a grudging acceptance of the Jevonian principles he had acquired in the 1880s, however limited they might be, and a refusal to see any merit in any work done since:

They all begin, at any rate, by laying stress on the doctrine of marginal or final utility, some as the key to the whole problem of value, some as the key to the demand side of it. And what has one to say to it? Of course, in the first place, it is quite true, so far as it goes; and, in the second place, it is pedagogically of some use (Ashley 1907: 476).

It has often been presumed that Ashley was an advocate of a more historical approach to economic analysis than the approach he encountered in his contemporaries, and that his failure to prevail over their arguments eventually led to the splintering of economic history from economics.⁷ The evidence rather suggests a different view: that Ashley was an Oxford historian whose appreciation of economic analysis remained Jevonian to the end of his life, failing to see in the principles of economics as he understood them anything that might illuminate historical processes. This is obvious from his design of the Birmingham Commerce degree: initially, there was one course only on 'Economic Analysis', in the second year as the fifth of five compulsory subjects; this was soon moved back into the first year, presenting a 'rapid survey of the whole of the wealth-producing and wealth-distributing activity

⁵[T]he exchange value of an article is simply its marginal utility measured in the marginal utility of the commodity selected as the standard of value' (Wicksteed 1888: 81).

⁶Discussing price formation, Gonner describes the market price as the value possessed by the last portion of the commodity that is worthwhile producing, 'in other words, commodities will exchange according to their *final utility*, and *the final utility* is that which appertains to the last portion of the commodity demanded and supplied' (Gonner 1888: 99; italics in original).

⁷For a demonstration of the defects of this argument, see Cook and Tribe (2016).

of society', and so evidently not a course in economic analysis as would have then been understood in Manchester, Cambridge or London.⁸

We can see the same attitude in Cannan, albeit reversed. He showed little interest in the economic history being developed in Germany, nor in contemporary English argument about Henry Maine's account of social evolution as one from status to contract, an argument that influenced both Ashley and Marshall. He was instead committed to a conception of 'modern economics' as a set of formal principles that were employed to demonstrate the errors of earlier economic principles.⁹ He did show a profound knowledge of these earlier economic principles, but the errors that the 'abstract method' there revealed were employed to demonstrate the viability of modern theory.¹⁰ This approach also illuminates the divergence with Marshall. For while the Jevonian approach served Cannan well as critique, for a critical assessment of past systems of economic theory, it works rather less well as an engine of practical economic analysis. The Marshallian approach had exactly the opposite problem, especially in the hands of Marshall: for while he developed a novel and flexible engine of practical economic analysis, he obstinately maintained an allegiance to a spurious theoretical heritage reaching continuously back to Ricardo. Whatever the strengths of Marshallian analysis, Marshall himself is entirely unreliable as a guide to the political economy on which he had built. He talked up Ricardo while implying that Jevons was superseded, although Jevons had in 1879 famously dismissed Ricardo and Mill, writing that:

When at length a true system of Economics comes to be established, it will be seen that that able but wrong-headed man, David Ricardo, shunted the car of Economic science on to a wrong line—a line, however, on which it was further urged towards confusion by his equally able and wrong-headed admirer, John Stuart Mill (Jevons 1871: li).

Jevons partially absolved Malthus and Senior from such condemnation, itself a questionable judgement, but one which does serve to demonstrate

⁸University of Birmingham, *Calendars for the Session* (1904/1905: 337) and (1906/1907: 361). See also Kadish (1991).

⁹Judged then, by what we may, perhaps, using the term in a sense which has very often, though not very accurately, been given to it, call the "abstract method", the theories of production and distribution arrived at in the first half of the nineteenth century must be visited with almost unqualified condemnation' (Cannan 1924: 383).

¹⁰As such, Cannan validated modern theory through an essentially teleological process. All the same, his exposition of early English political economy is full of insights today often lost to view, given the subsequent influence in the later twentieth century first of Sraffa's edition of Ricardo, and then of Marxian political economy.

the filiation from Jevons to Cannan: for Cannan shared with Jevons an idiosyncratic, but nevertheless quite explicit understanding that the economic theory of earlier times was a lumber room to be explored not for its own sake, but in order to advance contemporary thinking.¹¹

4 Cannan's Economics

This was the core principle of his teaching at LSE, as Lionel Robbins noted: 'There can be no doubt that the sixty-lecture course with its broad conspectus of the best of economic thought up to Marshall with Cannan's own positive and critical comments, was a magnificent training in general economics' (Robbins 1935: 396). This course had developed from Cannan's 1893 *Theories of Production and Distribution*,¹² and the resulting evolutionary outcome would be printed in 1929 as *A Review of Economic Theory*. In 1914 he also published *Wealth*, an introductory text based upon first-year undergraduate lectures that he had given since 1898 and which anticipates, in its focus upon wealth, incomes and population, Hicks's later *Social Framework*, itself developed from his first-year lectures at Manchester.¹³ *Wealth* went into a second edition in August 1916 which was reprinted every year up to 1924 save 1921, and then followed by a third edition in 1928. As with *Theories*, the publishing history of these key works testifies to the significance of Cannan's broader teaching in the 1920s, belying the tendency to regard him, by that time, as an economist of a former age. If that were true, then in the LSE during the 1920s the economic principles of that former age were undergoing a remarkable revival: an argument that is plausible and could in fact be extended to include Robbins's *Nature and Significance of Economic Science*—but there is no space to pursue this line of thought here.

Cannan's publishing career got off to an inauspicious start. His Lothian Prize essay was published in 1885, but at this distance, it is difficult to see

¹¹Jevons attached importance to an understanding of past theoretical work in political economy, as demonstrated by his valuable 1879 Preface to the second edition of *The Theory of Political Economy*. He also shared with Cannan a particular admiration for Smith's *Wealth of Nations*, having in 1878–1879 begun work on a new edition; see Tribe (2002: 43).

¹²See the comments by Gregory (1935: 367).

¹³Hicks, like Cannan, set out to write an introductory text that neither began with theory (supply and demand) nor with descriptive economics (see Hicks 1942: v). Of course, Hicks's book is chiefly notable for the systematic use of a national accounting framework, but the substantial filiation to Cannan is nonetheless striking.

any specific merit in it (Cannan 1885). As noted above, failing to win the Cobden Prize,¹⁴ Cannan reworked the first part of the essay he had submitted into *Elementary Political Economy*, ostensibly an introductory text similar in scope to the much later *Wealth*, but a work of baffling obtuseness. His reluctance to use the term ‘capital’ led to circumlocutions such as, ‘It is useless to attempt to divide any actual stock of useful material objects into objects used in the production of other useful material objects on the one hand, and objects used in the production of comfort directly on the other hand’ (Cannan 1888: 9). Nor did he see any point in distinguishing between useful natural objects (such as land) and those useful objects produced by human labour, leading to a subheading: ‘The Productiveness of Industry Increased by the Accumulation of Useful Material Objects’ (ibid.: 14). Things did not improve very much when it came to describing exchange: ‘The value of a given quantity of a commodity in some other commodity is the quantity of the second commodity for which the given commodity of the first commodity is exchanged’ (ibid.: 66).

The only named economist was Adam Smith, and no additional reading was given, and it is no surprise that this work should have sold so slowly.¹⁵ Recalling his developmental path at this point, Cannan later wrote that:

The method of direct attack on what was erroneous seemed likely only to add further sterile and unnecessary controversy to the literature of the subject, and the plan of writing a short textbook ignoring a great part of current doctrine had turned out quite ineffectual. It struck me that the most useful thing to do was to trace the development of general theory, showing it in its early crudeness as well as in the more plausible refinements of later times, and explaining its connection with the circumstances in which it grew up (Cannan 1912: 17).

As a result, in 1889 Cannan set to work on what became *Theories of Production and Distribution*, also travelling regularly through the autumn and winter of 1891–1892 to Cambridge so that he could sit in on Marshall’s lectures.¹⁶

¹⁴Political Economy and Socialism: What is the Teaching of Political Economy as to the Effects of Private Property and Free Exchange on the one hand, and of State Property and Regulated Contracts on the other hand, on the Production and Distribution of Wealth?. Unsuccessful Cobden Essay, 1886, Cannan Papers, BLPES Archives: File 898, handwritten foolscap ms. fn. 141.

¹⁵The book cost £130 to produce, but by 1890 sales had brought in just £60 4s. 4d. Lyttleton Gell (Clarendon Press) to Cannan, 8 January 1890, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 1.

¹⁶Notes on Alfred Marshall’s Lectures on Political Economy, 1891–1892’, Cannan Papers, BLPES Archives: File 909, lined quarto notebook, fn. 42. Cannan attended lectures and the Advanced Class on

A History of the Theories of Production and Distribution (1893) was a very different book to *Elementary Political Economy*, not least for an insistence on giving exact references to the literature discussed; a practice not usual at the time, as Cannan argues in his Preface (Cannan 1893: x). The literature that he reviews lies between the first publication of *Wealth of Nations* in 1776 and Mill's *Principles of Political Economy* of 1848, this latter forming the end date because of Cannan's belief that discussion stagnated for 20 years after the first appearance of Mill's book, newer developments being however too recent to deal with 'in an historical spirit' (ibid.: v). Not only does he regard *Wealth of Nations* as a foundational text, he sees it as a blend of 'indigenous economics with the system of Quesnay' (ibid.), not a common perspective in the early 1890s when the writings of Quesnay, Mirabeau, Turgot and Du Pont were little read and barely registered in discussions between English economists.

Cannan finished the manuscript in the spring of 1892 and sent it off to Macmillan, who smartly rejected it on the grounds that,

As we understand it, the two greatest developments of Political Economy since the time of Mill consist in a generalization of the doctrine of rent, formerly treated as a matter of land tenure, and in the sharp distinction in the function of the employer or entrepreneur, and the passive function of the capitalist, the owner or lender of capital. You appear however to have entirely neglected the study of modern Economic literature which is saturated with these ideas, and everywhere identify profit and interest, the capitalist and employer, and as a great part of your book is taken up with a critical account of the theory of distribution, this defect would inevitably prevent your work from being accepted as authoritative by Economists of the present day.¹⁷

In response, Cannan pointed out quite reasonably that the book was intended to end with Mill,¹⁸ so that the lack of coverage of modern literature was no omission; but this objection was quickly overruled: 'You are mistaken in supposing that our adviser overlooked your Table of Contents.

an irregular basis: six times in October 1891, six times in November, twice in January 1892, five times in February, attending for the last time on 22 March 1892.

¹⁷Macmillan to Cannan, 5 May 1892, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 2.

¹⁸Cannan to Macmillan, 7 May 1892, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 3.

It was in fact this very Table of Contents that led him to look with special care into your critical comments on Distribution'.¹⁹

Macmillan had been advised by Herbert Foxwell, whose lengthy comments demonstrate nonetheless that it was Foxwell whose understanding of 'modern economics' was at fault, not that of Cannan. His rambling memorandum begins decisively enough: 'It would seem that Cannan knows nothing of the development of Political Economy since Stuart Mill—i.e. in the last 40 years. In fact, he has yet to learn the modern science'.²⁰

Foxwell then proceeds to characterise 'modern economics' as a 'generalisation of the doctrine of rent, formerly treated as merely a matter of land tenure, and in the sharp distinction of the active function of the employer or entrepreneur, and the passive function of the capitalist, the owner or lender of capital' (ibid.)—an idiosyncratic view in the early 1890s, to say the least. Worse, Foxwell went on to remark that he had met Henry Higgs at Toynbee Hall, who had complained to him of two submissions Cannan had made for the Palgrave Dictionary: 'I asked Higgs whether after reading the Dictionary articles (which I have not seen) he thought the writer could possibly be qualified to write a book on the development of theory. He said that he had not a single moment's hesitation in saying that he could not be competent' (loc. cit.).

So Foxwell, not having seen the articles to which Higgs referred, recruited to his report Higgs's negative opinion about a manuscript that *he* had not seen. The report blunders on, eventually acknowledging some of its merits and suggesting in conclusion that lecturers would find it a good textbook, thus leaving any reader wondering about the unequivocal way in which he had begun.

In July 1892 Cannan sent the manuscript to Percival & Co., with whom a contract was signed in August.²¹ Following some slight revision, the book was ready for publication by May 1893. As already noted, it sold slowly—of 400 copies printed, 169 were sold in 1893, 81 in 1894 and 21 copies until May 1895, totalling 271 in two years.²² But by then Cannan was engaged

¹⁹Macmillan to Cannan, 12 May 1892, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 5.

²⁰H.S. Foxwell, Reader's Report on Cannan, 'English Political Economy', 6 May 1892, Macmillan Archives, British Library: Add. Ms. 55946, fn. 63.

²¹Percival & Co. to Cannan, 10 August 1892, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 9.

²²Percival & Co. to Cannan, 28 May 1895, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 16.

upon another project that would, this time, make his name—editing a set of student’s notes from Adam Smith’s Glasgow lectures.

5 The Smith Editions

In the spring of 1895 a chance encounter with Charles Maconochie, an Edinburgh barrister, brought into his hands a set of student notes from Smith’s Glasgow lectures of 1763–1764. Smith’s executors had (on Smith’s instructions) destroyed most of his papers when he died, and the discovery of this notebook contributed significantly to an understanding of the construction of *Wealth of Nations*, since it recorded lectures from the period immediately preceding his departure for France with his tutee, the Duke of Buccleuch. In the latter half of the nineteenth century, *Das Adam Smith Problem* had been elaborated by German scholars, positing a distinction between the ‘sympathetic’ orientation of *Theory of Moral Sentiments* (1759) and *Wealth of Nations* (1776). In its crudest formulation, this discrepancy was explained by Smith’s change of view following his encounter with French philosophers and economists in 1764–1765, and so evidence of what Smith had taught during his final lectures in Glasgow, and its evident filiation to arguments found in *Wealth of Nations*, was of major importance to an understanding of Smith’s work in general. Publication of Cannan’s edition of the lectures demonstrated the non-existence of any *Problem*; all the same, the arguments developed by German scholars provided the initial foundation for serious discussion of Smith’s work, which had not happened in Britain for decades, at the very least.²³ Cannan had already, in the Preface to the first edition of *Theories of Production and Distribution*, complained of the lack of ‘even a tolerably good edition’ (Cannan 1893: xi) of *Wealth of Nations*, despite the regular appearance of new editions.²⁴ In 1895 Cannan himself seemed oblivious to the extensive discussion among German academics of Adam Smith’s ‘method’, but he had already demonstrated a good working knowledge of both *Wealth of Nations* and the writings of the Physiocrats, and so would have been primed to grasp the significance of the manuscript that

²³I detail the development of the *Problem* in Chapter 5 of Tribe (2015).

²⁴Thirteen separate editions were published in Britain during the decade from 1891, including versions edited by McCulloch, Shield Nicholson, Belfort Bax and William Ashley—see Tribe and Mizuta (2002: 343).

was sent to him on 2 May.²⁵ He evidently read the manuscript immediately, understood the significance of what he was reading, and wrote a report for Maconochie proposing the editing and publication of the notes. One week after the notes had originally been sent to Cannan, Maconochie wrote: ‘Reading yr. report has finally proved to me that any publication of the M.S. should be edited and commentated upon by an expert in Economics, and certainly no one is better entitled to do it than you’.²⁶

Cannan approached Thomas Raleigh, Reader in English Law at All Souls and also a delegate of Oxford University Press, who reviewed the manuscript, sounded out the other delegates, wrote back suggesting how Cannan should approach the Press, and offered his help with the annotation of the text.²⁷

Direct contact with the Press was then quickly made and there evidently followed some wrangling over property in the manuscript, copyright and payment,²⁸ for Cannan wrote to the Press in early June emphasising the significance of the manuscript:

A.S. has always been a popular writer. Of the Wealth of Nations there are 5 editions at least in print in this country alone. The exhausted editions are numbered by the score. I should think at least 20 or 30 copies are sold for every one of Ricardo’s Principles ...

Since I wrote last I have done some work with the MS and I find it will not be difficult with the aid of several recent works especially the Catalogue of AS Library to produce an edition which will be absolutely necessary to any serious student of the Wealth of Nations at any rate till some moderately respectable edition of that work itself appears.²⁹

²⁵Maconochie to Cannan, 2 May 1895, Cannan Papers, BLPES Archives: File 1020, Correspondence 1889–1899, fn. 28. Maconochie’s family had come into possession of the lecture notes many years previously in a manner that could no longer be reconstructed. A careful account of the manuscript and its provenance, endorsing Cannan’s assessments, can be found in Smith (1978: 5–9).

²⁶Maconochie to Cannan, 9 May 1895, Cannan Papers, BLPES Archives: File 1020, Correspondence 1889–1899, fn. 30.

²⁷Raleigh to Cannan, 26 May 1895, Cannan Papers, BLPES Archives: File 1020, Correspondence 1889–1899, fn. 34.

²⁸While the delegates agreed to publish the manuscript at their own risk, they queried Maconochie’s property in it and so were not prepared to pay him anything—Gell (Secretary to the Delegates) to Cannan, 29 May 1895, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 17.

²⁹Cannan to Gell, 6 June 1895, Cannan Papers, BLPES Archives: File 1020, Correspondence 1889–1899, fn. 41 (underlining in original).

In the same letter, Cannan offered to edit the text for £25 plus 10% of revenue after the first 500 had been sold, with £25 for Maconochie. As it transpired, the Press were only prepared to pay £25 in total, making Cannan responsible for a payment of £20 to Maconochie, which terms were agreed, and by early July 1895, all this had been settled and Cannan set a deadline for delivery of 30 June 1896.³⁰

Work proceeded apace over the winter of 1895–1896, Raleigh supplying Cannan with a stream of suggestions and comments, so that when the work was published in October 1896 it was the first modern edition of any work of Adam Smith.³¹ Very positively reviewed, translated into Japanese in 1926 and German in 1928, the work set a new standard not only for editions of Smith but also for all later editions of the works of early political economists. As Cannan also noted in his Introduction, the existence of the lecture notes enabled a comparison to be made between *Wealth of Nations* as it appeared in 1776 and Smith's thinking at the time he left for France (see Smith 1896: iv). The following year he turned to *Wealth of Nations*,³² and applying the same careful and analytical approach produced in 1904 the first modern edition of the book, which became the standard English-language version until 1976, when the Oxford edition was published (see Smith 1976).

Cannan based his edition on the last edition printed in Smith's lifetime, the fifth of 1789, presuming that any scholarly edition should be based on the final version of a text in the author's lifetime. However, the fourth and fifth editions had been set from their respective previous editions and were not revised by Smith; changes from the third edition of 1784, which Smith had worked on extensively, are therefore almost entirely attributable to variations introduced by the setter. Nonetheless, this problem does not vitiate Cannan's approach, since he also collated the fifth edition with the first and so was able to remove the more obvious errors, while also revealing in footnotes the changes that Smith had introduced into the third edition.³³ Coupled with his Introduction and explanatory footnotes, this 1904 edition of *Wealth of Nations* was a true landmark, since all previous editions had simply reprinted an unexamined text.

³⁰Contract with Clarendon Press for *Lectures*, 6 July 1895, Cannan Papers, BLPES Archives: File 1015, Publishers' Agreements, fn. 15.

³¹For a summary of editorial work and reception, see Tribe and Mizuta (2002: 43–47).

³²Studman to Cannan, 19 July 1897, proposing that Methuen publish a scholarly edition of *Wealth of Nations* edited by Cannan, Cannan Papers, BLPES Archives: File 1018, Correspondence with Publishers I 1890–1916, fn. 33.

³³For more detail on this edition, see Tribe and Mizuta (2002: 47–48).

These points are still worth making, for Cannan's edition has undergone an unforeseen revival in the twenty-first century. The 1976 Oxford edition of *Wealth of Nations* was based on the third edition of 1784, in conformity with modern editorial standards, and this remains the standard edition, albeit available only in hardback from the Press and not as an e-book. However, financial problems with the edition experienced by the Press in the later 1970s led to the Liberty Fund making a financial contribution to the completion of the publication of Smith's writings; in return, they gained the right to publish the Oxford edition in paperback format (which remains available and is the cheapest, and best, edition of *Wealth of Nations*). With the subsequent development of the digital Library of Liberty, this edition was also available for many years online; but this arrangement was terminated by the Press in February 2014, and a digital edition of Cannan's edition substituted. Hence, the only reliable electronic version of *Wealth of Nations* legally available today is Cannan's 1904 edition. His scrupulous work on the text ensures that, as an edition, it is still quite usable, and while his long Introduction is by now rather dated, it remains superior to most such introductions by virtue of his detailed knowledge of the relevant contemporary literature.

6 At LSE

By the time that his edition of *Wealth of Nations* was published Cannan had been teaching at LSE for several years, and evidently its demands had slowed the completion of his work on Smith. In the early 1890s, Cannan had become a regular contributor on legislative and administrative matters to the new Oxford journal, *Economic Review*, published by the Oxford University branch of the Christian Social Union, and so a very different kind of journal to the *Economic Journal* that first appeared a few months later. He developed an interest in local government taxation, and this formed the subject of his first course of lectures at LSE in 1895, then published in 1896 as an account of the early modern statutory foundation of local systems of taxation (Cannan 1896). By 1897, Cannan was offering 'The Meaning and Use of Economic Terms and the Leading Principles of Economic Science', together with an advanced course on 'Recent Additions to Economic Theory' which the following year became 'History of Economic Theory' (see Caine 1963: 54). This foreshadowed the later

development, by 1904,³⁴ of the cycle of second- and third-year courses written up as *A Review of Economic Theory*.

The LSE *Calendar* for 1902/1903 already had Cannan down as ‘Teacher of Economic Theory in the University of London’,³⁵ and as President of Section F of the British Association in 1902 Cannan had delivered an Address arguing for the utility of ‘economic science’ in approaching social and political issues, remarking in passing on the absence of a textbook for economics which ‘commands any really wide approval’ (Cannan 1902: 460). Whether this was intended as a deliberate snub to Marshall is not clear, but the Address certainly raised his hackles:

I will not argue in favour of “economics” versus “political economy”: though as to your reason I stand half way between you and Ashley; who, if I understand him rightly, holds that a grasp of the principles of business is becoming so essential to the broader problems of State policy, that even from the public point of view there is much to be said for a temporary diversion of the attention of economists from public affairs to private. But I must splutter against your adoption of the London phrase “economic theory” to represent what had hitherto been called by a name which seems to me perfect viz “General economics”; as contrasted with “Special branches of economics” ... You imply that economic theory gives a sense of proportion. I should say that economic theory is that (vital) part of economics which exercises the analytical and ratiocinatory faculties but not educates a sense of proportion.³⁶

Although the ‘science’ that Cannan had described in his Address was largely descriptive, more like the approach he takes in *Wealth* than in *A Review of Economic Theory*, we can see here already a division opening up between London and Cambridge about the scope, method and terminology of economics. Shifting our attention on from Marshall to his students Pigou and Keynes, we can see in them an engagement with real economic issues informed by theory that was always absent in the London approach, which increasingly worked in a dogmatic fashion from theory to issues. This is most evident of course in the work of Robbins, culminating in his extraordinary *The Great Depression*, where events are supposed to bend to theory.³⁷

³⁴London School of Economics and Political Science, *Calendar (1904/1905)*: 113): Principles of Economics, including the History of Economic Theory—60 lectures over two years for second and third year students by Cannan, Tuesdays at 4 p.m. Repeated at 7 p.m. for evening students.

³⁵London School of Economics and Political Science, *Calendar (1902/1903)*: 6).

³⁶Marshall to Cannan, 22 September 1902, reprinted in Whitaker (1996: 397).

³⁷For an example of Robbins’s overweening grandiosity in the early 1930s, see Howson (2011: 192).

Cannan's saving grace here was his extensive knowledge of past economic theory and his demonstrable commitment to high standards of historical scholarship that Robbins never really had. It is for this reason that *Theories of Production and Distribution* can still be read with great profit today, because in the first place, it is based on very wide reading and, in the second, the 'robust common sense' to which he submits past arguments cuts through many of the received ideas built up in the course of the twentieth century. Cannan remained very active as a teacher well into his 60s, engaging with the First World War as a critic, writing acerbic reviews about wartime administration rather than engaging in the practicalities of economic management—the highest level at which he ever exercised the latter was as an Oxford City Councillor from 1896. As he writes in the Preface to his collection of occasional writings from 1914 to 1926, if asked what he had done in the Great War (he was 53 when it started), his response would be, 'I protested' (Cannan 1927: v).³⁸ As such, comparison of his wartime writings with those of Pigou, collected as *The Political Economy of War* (Pigou 1921), highlights again the gap between the 'practical theory' of Cambridge and the deployment of theory as critique that would become the hallmark of LSE.

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³⁸But: 'It would be no use to say that immediately on the outbreak I offered my services as a harvester, for the young man who took my name evidently regarded me as far too old...and nothing ever came of it. It would be no better to say that I sat for quite a long time on several committees, because everyone did, and nothing ever came of that' (Cannan 1927: v).

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8

Arthur Lyon Bowley (1869–1957)

Adrian Darnell

1 Introduction¹

Arthur Lyon Bowley was born on 6 November 1869, in Bristol; he died at Haslemere on 21 January 1957. He was the son of the Reverend James William Lyon Bowley and his second wife, Maria Johnson; James's first wife, Ann Jackson, had died in 1861 having borne three children. At the time of his death (of colitis) in 1871 aged 44, Bowley's father was the vicar of St Philip and St Jacob, Bristol. The young Bowley was then brought up by his mother in a household comprising the three half-siblings from his father's first marriage and four siblings from his marriage to Maria. The Mayor and local businessmen of Bristol collected a benefaction of £2100 for the family, and this generated an annual income of £200 for the household of a widow and seven children. Arthur was educated first in Bristol but, in 1879, he moved to Christ's Hospital, London, having met the primary criterion for entry: that his widowed mother required assistance towards Arthur's education. He left Christ's in 1888 having demonstrated early

¹For general accounts of Bowley's life and work, biographies and assessments, see, for example, Allen and George (1957), A.H. Bowley (1972a), Maunder (1972), Darnell (1981) and the more recent and comprehensive Dale and Kotz (2011).

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mathematical abilities, winning the Tyson Gold Medal for Mathematics (1886), the Montefiore Prize for Mathematics and Classics and the Thompson Gold Medal for Mathematics (1888). But he was a sickly child and did not enjoy good health. He left as the 'top Grecian' (awarded for achievements in Classical studies) and entered Trinity College, Cambridge, in 1888 with a Mathematics scholarship. By virtue of being 'top Grecian', he secured further financial support, and while an undergraduate he also won scholarships. But the ill health he had suffered as a young boy returned and, although he successfully sat his finals in 1891 and was placed tenth in the First Class, he was not qualified to graduate due to his periods of illness and absence. Such misfortune, however, was to prove positively life-changing: in order to complete and take his degree, he was required to stay at Cambridge for a further period, and from October 1891 to March 1892, he studied courses in physics, chemistry (in the Cavendish Laboratory) and, most significantly, he followed a reading course in economics under Alfred Marshall. Bowley was introduced to Marshall by his Cambridge tutor, Reverend Richard Appleton (himself an 'old Grecian'), and Bowley and Marshall thus began a long friendship.

Marshall's own work was that of a powerful economic theorist, though he dismissed the claims of pure theory to be economics proper (see O'Brien 1981: 39). While it was his view that nineteenth-century economics 'had completed the main qualitative tasks...[and that]...the need was now for quantitative analysis' (O'Brien, *op. cit.*), his own empirical work was highly limited. He sought measurable regularities in economic phenomena ('laws') but the quantitative developments he anticipated were to be the work of others, and none more so than Arthur Bowley. In *Money, Credit and Commerce* (Marshall 1923), for example, Marshall had asserted that 'the progress of economic science depends largely on the stock of trustworthy and appropriate statistics at its command' (*ibid.*: 273) and Marshall would have recognised in Bowley a student well capable of putting some quantitative clothing on his qualitative theorising.

However, Bowley was to achieve much more than to stimulate and implement a programme of empirical economics: he also pioneered, and sought to popularise, the use of mathematics in economics and he was a pioneer in the theory and practice of sampling. If Bowley's applied statistical work might have been viewed as building upon and developing the Marshallian tradition, his innovative work in statistical theory and mathematical economics did not always generate favourable reactions from his mentor. The doubts Marshall had regarding the use of mathematics in economics are well known: '(1) use mathematics as a shorthand language, not as an engine of enquiry;... (3) translate

into English... (4) illustrate by examples that are important in real life... (5) [now] burn the mathematics', adding that if 'you can't succeed in [finding examples]...burn 3' (Marshall to Bowley, 27 February 1906, reprinted in Pigou (1966: 427)). This attitude extended also to statistical theory: on the publication of the first edition of Bowley's *Elements of Statistics* (1901) Marshall wrote: 'I told you I thought there was too much mathematics in your excellent book...having now brought out this great and successful book...leave the mathematics to one side' (Marshall to Bowley, 21 February 1901, reprinted in *ibid.*: 419). Thus, while Marshall, an accomplished mathematician himself, was content to see others collect statistics (i.e. data) relevant to economics (as distinct from applying statistical theory to the social sciences), he had little time for mathematics in economics and little time for statistical theory itself. Marshall's focus was on statistics in the sense of the data which describe the phenomena in question; he had no appreciation (contrary to Bowley) that data require careful collection and interpretation.

This aside, Marshall's positive influence on Bowley is not to be underestimated. It was at Cambridge and on Marshall's suggestion that Bowley entered and won the Cobden Essay Prize in 1892; his submission had the succinct title, 'Changes in the Volume, Character and Geographical Distribution of England's Foreign Trade in the 19th Century and their Causes'. Not only did this win the Prize but subsequently became a successful publication (1893, with the title: *A Short Account of England's Foreign Trade in the Nineteenth Century*), and is notable for its detailed, even forensic, analysis of the balance of trade.

On leaving Cambridge in 1892, Bowley first took employment as a mathematics school teacher, initially at Brighton College (1892–1893), then at St John's School, Leatherhead (1893–1899), and then in a temporary post for two terms at Clifton College, Bristol. His continuing friendship with Marshall, combined with his interests in statistical enquiry and in what was to become a lifelong interest in social reform, led him to write a paper analysing changes in wages. This was successfully submitted to Cambridge for the Adam Smith Prize in 1894 and an extended version under the title 'Changes in Average Wages (Nominal and Real) in the United Kingdom between 1860 and 1891' was read to the Royal Statistical Society (RSS) in March of 1895. For this paper, Bowley was awarded the prestigious Guy Medal in Silver (he was awarded the Guy Medal in Gold in 1935). Marshall, in the discussion, observed that as one of the examiners at Cambridge he had been 'struck by the brilliancy of the plan...to extract some information from the great mass of wage statistics which had hitherto been almost useless because of its fragmentary nature' (Marshall in Bowley 1895: 279).

Of the recurring themes in Bowley's work, interest in both socialism and social reform are evident. Indeed, it is worth noting that Marian Bowley—daughter of Arthur Bowley who was herself an economist and historian of economic thought, becoming Professor of Economics at UCL—took exception to Robbins's throwaway line (Robbins 1971: 74) describing her father as 'almost certainly a conservative', proposing the alternative that he was a 'genuine liberal of pre-1914 vintage after an early period of interest in Fabian Socialism' (Bowley 1972b: 808).

Of critical importance at this stage in Bowley's intellectual development is that in 1895 the London School of Economics and Political Science (LSE) was founded. The social and economic reformers Sydney and Beatrice Webb, the author and playwright George Bernard Shaw and the political scientist George Wallas, all Fabians, established LSE using a bequest from the will of Henry Hutchison (see Hayek 1946). The LSE *Calendar 1895–1896* records that,

The growing importance of social and economic subjects has drawn attention to the need of further provision for systematic training in economic and political science, and the promotion of original investigation and research ... It is now proposed to attempt to remedy this deficiency [by establishing the London School of Economics and Political Science].

The first Director of LSE, William Hewins, then aged 30, faced early challenges not only to enrol students but, perhaps more importantly, to engage staff. In the original prospectus, the named lecturers were all part-time (though the Director was not). LSE was established with the deliberate intention to 'represent important aspects of economic science and practical investigation whether they were in agreement or not with orthodox economics ... [N]ew views of policy had to make their way against established political and economic vested interests' (Hewins quoted in Hayek 1946: 4, fn. 1). In order to encourage and promote empirical studies in the social sciences, a course in statistics was established. At that time, no British university taught statistics as a distinct discipline and Hewins, 'probably on Marshall's advice' (ibid.: 7), turned to Bowley. On appointment to this part-time post, Bowley himself turned to Edgeworth for advice on the 'nature and literature of statistics' (Bowley 1934: 119) and thus began a friendship which lasted until Edgeworth's death.

In 1895, Bowley, then, became a part-time Lecturer in Statistics at LSE and he taught there continuously until his retirement over 40 years later in 1936. His LSE teaching in the early years took the form of an evening lecture, on Wednesdays, attended by civil servants and advanced students. Until 1900, he combined this part-time post with a succession of appointments in

schools. On leaving Clifton College in 1900, he successfully applied for a post as Lecturer in Mathematics at Reading College where, seven years later, he became Professor of Mathematics and Economics. At Reading, he met Julia Hilliam, ‘Instructor in Wood Carving’, and they married in 1904. Julia was one of the most accomplished carvers in the country; Arthur and Julia had three daughters (Ruth, Agatha and Marian) born in 1907, 1909 and 1911.

While at Reading, Bowley maintained his part-time appointment at LSE and was promoted to Reader in 1908 having added mathematics to the curriculum. The School was part of the University of London, but it did not proceed rapidly to appoint either full-time staff or chairs. In 1915, he was awarded the title of Professor at LSE and four years later, in 1919, he was appointed full-time to the newly created and first ever Chair in Statistics in the social sciences. The post attracted an annual salary of £1000 and was supplemented by a research assistant on a further £150–200. The former enabled him to resign his Reading post; the latter enabled him to pursue his research with renewed vigour.

An overview of his later work would suggest that Bowley was not so much a pioneer of economic theory as a pioneer of applied economics, and especially a pioneer of statistical techniques and their application to economics and social enquiry more generally. In this sense, Bowley was a pioneer in Marshall’s tradition through his collecting and generating statistics (and techniques) by which economics could be both tested and given quantitative substance. He was also a most important pioneer of sampling theory and methods, and particularly in their application to social studies.

He was awarded countless academic honours, including a DSc from Cambridge in 1913 but perhaps his most notable honours were the award of the Silver and Gold Guy Medals by the RSS (1895 and 1935, respectively). The Silver Medal is awarded to Fellows of the RSS who have communicated papers of special merit to the society or have published such papers in the journals of the society while Fellows who have made significant and innovative contributions to the theory or application of statistics are considered for a Gold Medal.

Bowley retired from LSE in 1936, was made Professor Emeritus, and in 1937 he was awarded the CBE; in 1950, he was made Knight Bachelor. Notwithstanding his retirement, he was persuaded to become the Acting Director of the Oxford Institute of Statistics in 1940 and, even though in his 70s, was far from being a nominal appointment: he took on the role with energy and retired (finally) in late 1944 and ‘left the Institute a vigorous and going concern’ (Burchardt and Worswick 1957: 2). He retired (for a second time) in December 1944 and died just over 12 years later.

Bowley was a most conscientious enquirer in all that he studied. He spread his talents widely as a teacher and researcher in economics, statistics and economic statistics. His reputation and abilities led to numerous positions of importance and influence on national and international bodies, including: Fellow of the Royal Economic Society from 1893 (and election to Council in 1901); member of the Council of the RSS from 1898 (President during 1938–1940); member of the International Statistical Institute from 1903 (Treasurer 1929–1936 and also 1947–1949); member of the Editorial Board of the newly founded journal *Economica* from 1921 (with Cannan and Wallas); Fellow of the British Academy (1922); editor and member of the Executive Committee of the London and Cambridge Economic Service from its foundation in 1923–1945 (his final contribution was in 1953); elected a member of the Senate of the University of London (1930); and a founder member of the Econometric Society in 1933 (and President 1938–1939). Also, as an acknowledged authority on the measurement of subsistence levels and the cost of living, William Beveridge (who had been Director of LSE from 1919–1937) invited him, in 1941, to membership of a small committee to advise him on the Report on Social Insurance and Allied Services (the ‘Beveridge Report’).

Bowley’s major contributions to economics were made as a collector and compiler of economic statistics (particularly wages and national income), as a pioneer of statistical techniques in the social sciences, in the development of mathematical economics and econometrics and, most notably, as a pioneer of sampling techniques.

2 Wages and National Income

Bowley’s skills as an economist and statistician had been confirmed in 1894 when he won the Adam Smith Prize. It is worth noting that at this point, Bowley’s first formal contact with the subject had only been three years earlier, in 1891. His rapid understanding and appreciation of the methods and approach of economics were remarkable and a great testimony to his intellectual abilities and to his ability to apply his mathematical skills to the social sciences. With Marshall’s support and encouragement, Bowley had begun the transformation from a mathematician and schoolmaster to an economic statistician.

Bowley’s method was novel. The ‘brilliancy of the plan’ (as noted by Marshall) was to construct ratios of movements and indices, and not use the actual wages. Bowley, ever the pragmatist, described his aim as not to

analyse wage rates with ‘mathematical accuracy’ for that ‘would have been impossible’; his method was only ever to compare two figures of wages at different points in time when they had been ‘given by the same authority’ (Bowley 1895: 227) and this enabled him to construct the best estimates of the rate of wage change. His argument for this approach was straightforward: ‘Without this...the inquiry would break down at the start for want of comparable figures; and I see no other way of making use of the figures that do exist, other than that I have chosen’ (ibid.: 230). The data at his disposal were patchy and of varied quality, but he extracted the very most information from them. His concern with the potential errors of his method was noteworthy; he had, for example, constructed a weighted index number with which to examine movements in average national wages and had used proxies where the weights were unavailable. He defended this approach and observed that ‘It appears at first sight as if this must vitiate the result, but *both by experiment and theory* it is found that the error introduced into the result is exceedingly small’ (ibid.: 236; italics added). Even in this very early work, Bowley was seeking justification for his method from both a theoretical and an empirical perspective. The theoretical question of the accuracy of weighted averages, when there is potential error in both ‘quantity’ and ‘weight’, was the subject of a paper he published in 1897, in which he was able to conclude that ‘in general the total error due to weight is less than that due to quantity...[but] their relative importance depends on the special circumstances of each investigation’ (Bowley 1897: 861).

The paper of 1895 is remarkable for several features; it was original in approach, focusing on *changes* in comparable data on wages; it used a weighted index, and analysed the potential sources of error; it was painstaking research and Bowley’s examination of the mass of primary data would have been conducted by himself without significant assistance (though the pupils at Leatherhead copied some of the figures, he certainly worked without the use of any great technology).

The RSS enthusiastically welcomed Bowley’s paper for both its method and its results. The meeting was chaired by Frederick Hendriks, then Vice President of the society, who remarked that the paper ‘struck out a method by which an accumulation of statistical observations hitherto lying waste might be utilised’ (Hendriks in Bowley 1895: 285), thus identifying the potential of Bowley’s pioneering method in analysing data in a variety of other circumstances.

Bowley’s early forays with index numbers led him, in 1899, to propose (in the *Dictionary of Political Economy*, edited by Inglis Palgrave) the formula for what has become known as Fisher’s ideal index number (Bowley 1899: 641).

That the use of index numbers is today the norm and the accepted best practice owes its origins to Bowley (1895).

Bowley's work on wages became a large-scale project, and between 1898 and 1906, he published a 14-part study of 'The Statistics of Wages in the United Kingdom During the Last Hundred Years'. Of these papers, ten were his sole authorship, while the other four were in collaboration with G.H. Wood (who had been trained as an engineer, later became a student of Bowley and was later appointed to the statistical staff of the Labour Department of the Board of Trade). The early work can appear underdeveloped theoretically: for example, Bowley moves ambiguously between concepts of wage rates, earnings and the total national wage bill. However, the statistical techniques used, and the economic concepts employed, became more refined in the latter parts and in subsequent work. Thus, Bowley provided a causal economic analysis of changes in wages, concentrating on the changes in the demand for labour, the increasing market power of combined workers and labour's increasing efficiency. He also produced comparative results for the UK, the USA and France. Moreover, in a later and mature analysis of wages and prices under the conditions of war, *Prices and Wages in the United Kingdom, 1914–1920* (Bowley 1921a), he provided much more precise definitions of all the key concepts. This volume also provides an analysis of the divergence between the growth of earnings and that of wage rates and, although he does not use the term 'wage-drift', Bowley was the first to draw attention to this important phenomenon. While his work on wages is primarily to be seen as statistical in nature, the economic content of Bowley's work was also significant.

Bowley's extensive work on wages led him quite naturally into three related areas: the theory of index numbers, national income statistics, and investigations into poverty, unemployment and social change. His work on the theory of indices was overshadowed by that of Fisher and Edgeworth, but paradoxically the index which Frisch christened the 'Bowley Index' had in fact been proposed earlier by both Marshall and Edgeworth, while the index known as Fisher's 'ideal index number' of 1926 had, as noted above, been introduced some 27 years earlier by Bowley in his entry in Palgrave's *Dictionary of Political Economy*.

Bowley's concern with wages became a motivating force in his interwar work on national income estimation. 'The Definition of National Income' (Bowley 1922) is a landmark study, containing as it does the distinction between market price and factor cost evaluations, the term 'transfer payments' was explicitly introduced, and the treatment of taxation was much clarified. This work culminated in the seminal 1927 joint publication with

his one-time postgraduate student, Sir Josiah Stamp, *The National Income, 1924*. The first official estimates of national income, made during the Second World War, relied heavily upon Bowley and Stamp's original work, and their methodological sophistication has shaped all later studies of national income.

In 1920, Bowley speculated that the share of wages in national might be constant. Evidence for this was presented in *The National Income* for the years 1911 and 1924. Bowley became the first to clearly assert the constancy of the wage share in his 1937 book *Wages and Income in the United Kingdom Since 1860*. This volume synthesised and brought up to date much of Bowley's earlier work; in a review, Pollak (1938) remarked 'even today, when considerable attention has been directed to studies of wages, standards of living, national income and its distribution, Professor Bowley's work stands out as pre-eminent in the sphere he has so peculiarly made his own' (ibid.: 79). On the basis of this work, Samuelson honoured Bowley by coining the expression Bowley's Law in 1964 (in the sixth American edition of his famous textbook *Economics*) to describe the stylised fact of a constant wage share, a finding which was wholly at odds with the view of the classical economists who perceived the factor shares of land, capital and labour to be inherently flexible.

3 Sampling Methods in Practice

Wholly in accordance with LSE's founding principles, the Ratan Tata Foundation was established at the School in the early 1910s through a gift from Sir Ratan Tata of Bombay and Twickenham. The new Foundation was established 'to promote the study and further the knowledge of the principles and methods of preventing and relieving destitution and poverty' (Hayek 1946: 17).

Bowley had published a study in 1913 of 'Working-Class Households in Reading' (Bowley 1913a) and, with the support of the Ratan Tata Foundation, he was able to extend this in 1915 with a survey of other towns of England: Northampton, Warrington and Stanley. The report, published as *Livelihood and Poverty* (1915) (with Burnett-Hurst), was notable (as is so often the characteristic of Bowley's work) as much for its method as for the results themselves. The critical innovation in this work was the use of random samples.

Bowley was one of the early pioneers of the sampling technique, and had first drawn attention to the method in his 1906 Presidential Address to

Section F of the British Association for the Advancement of Science (BAAS) when he observed:

The simple method of samples...for which all the materials have existed for at least twenty years...has been completely ignored ... [P]rogress in the development of theory has...been rapid...but there has been remarkably little application to practical statistical problems. The attention of mathematical statisticians has been mainly directed to theory ... [I]t is time that it was brought to bear on the...analysis of existing industrial statistics (Bowley 1906: 548–549).

Bowley's great success in the field of sampling extended from questionnaire design to the analysis of the results, and he set exemplary high standards. His first sample survey, of Reading (Bowley 1913a), provides a first class model of socio-economic investigation and it provided the pilot study from which *Livelihood and Poverty* developed.

At the turn of the century, the common view was that precision in demographic studies could only be obtained by the method of census investigations. Bowley was a vocal critic of this view and sought, by example, to demonstrate the virtues of sampling. The five towns study deliberately focused on towns with very different populations. Their different sizes implied the use of different sampling rates: Northampton, with a population of around 90,000, was sampled at the rate of one household in 23; Warrington's population of around 72,000 was sampled at the rate of one household in 19; Reading's population of around 88,000 was sampled at the rate of one household in 21; and Stanley's much smaller population of around 24,000 was sampled at the rate of one household in 17. The method was ahead of its time: the use of sampling allowed a full discussion of the precision of the results, and in focusing on evidence of poverty, the family size became a critical causal variable. To analyse this, Bowley ensured a family of any given size was made comparable to any other family by the innovative use of equivalence scales. While low wages were found to be the most important cause of poverty, income was analysed relative to the family size and composition, using equivalences. The concept of the poverty line and the lack of any moral judgements on those in poverty were major elements of the work. Bowley and Burnett-Hurst concluded that, '[O]ur figures show that...poverty exists in certain places on a scale which is really appalling' (Bowley and Burnett-Hurst 1915: 46) and '[I]t is often implied that the causes which bring men into poverty are within their own control, that they are the masters of their fate and the creators of their misfortunes. In many cases this may be so, yet the extent to which it is true is exaggerated' (ibid.: 47). The impact of *Livelihood and Poverty* was lessened, inevitably, by the

fact that it was published in the war years; however, the sequel, *Has Poverty Diminished?*, written in collaboration with Margaret Hogg, was published in 1925 and firmly established Bowley's reputation and standing as a serious social investigator. His conclusions were not obviously in tune with those of a conservative (cf. Robbins's view of Bowley) and were more in tune with those of a Fabian Socialist. The critical issue for Bowley was that the coincidence or otherwise of his conclusions with his political views was an irrelevance; his conclusions were based on firm evidential foundations and constructed upon properly collected and reliable statistics.

Bowley's concern with the analysis and measurement of social change, illustrated by *Livelihood and Poverty*, had been evident in his earlier 1915 *The Nature and Purpose of the Measurement of Social Phenomena*. This work, essentially a compilation of lectures he had presented at LSE in the previous year, had an underlying methodological approach, both positive and normative. On the positive side, his objective was to describe society as it is, to classify and investigate cause and effect. On the normative side, he sought to understand the position of society with a view to modifying it, to construct 'a society more in accordance with some ideal' (Bowley 1915: 7). The influence of his earlier seminal analysis of wage changes can also be seen here, where Bowley seeks to examine changes in national income. His concern is with changes and with precision. Hence, he eschews simple averages and adopts the methods of deciles because they are more readily determined with a minimum of error and because they are more readily used in deriving rates of change. Of critical importance are Bowley's attention to the distinction between that which can be measured and that which cannot, and to the dependence of a statistic's usefulness on its precision. In *Nature and Purpose*, Bowley's greatest achievement was perhaps to set out a template for the systematic and comprehensive research of the socio-economic dimensions of society with what was, for its time, an innovative emphasis on dynamics (the analysis of intergenerational mobility is of particular interest).

4 Statistical Theory and Official Statistics

Bowley's studies of wages, national income, unemployment and poverty were frustrated both by an underdeveloped relevant statistical method for social enquiry and, equally important, by the lack of suitable data. Elementary statistics was not commonly taught as a prescribed component of an economics degree in the nineteenth century; however, as noted above, from its foundation in 1895, LSE was unique and Bowley's lecture courses in statistics became the subject matter of his very successful *Elements*

of *Statistics* (Bowley 1901). This textbook, whose aim was to introduce the reader to the theory and practice of statistics, was well received and went through six editions (although most changes were minor, and the last edition was in 1937). While LSE was unique in that its economic curriculum included a course in statistics, other universities' curricula sometimes included statistics as an option but students were not 'pressed to go, and were encouraged in the belief that a little common sense could easily take the place of regular training with tables of numbers' (Darnell 1981: 146). The *Elements* was most important in the development of a profession of empirical economists and social scientists and wholly reflected the principles of LSE itself. Indeed, one could go as far to maintain that, in the absence of LSE's pioneering insistence on a statistics programme within the degree, not only would Bowley not have been appointed to the School, but his *Elements* would not have been written at the time it was. Many of his ideas can be seen as precursors to the concepts of exploratory data analysis, notably stem-plots, decile boxplots and the trimean.

Bowley's text amply illustrates his methodological perspectives on the role of the statistician and of evidence in the social sciences:

The statistician furnishes the political economist with the facts, by which he tests his theories or on which he bases them ... [I]t may be held to be the business of the statistician to collect, arrange and describe, like a careful experimentalist, but to draw no conclusions; even in an investigation related to cause and effect, to present evidence but not conclusions (Bowley 1901: 8, 9).

This view implicitly makes a distinction between the 'statistician' and the 'investigator': the statistician is a disinterested technocrat who amasses 'facts' (analogous to the fictional white-coated and 'neutral' laboratory scientist) while the political economist takes on the more creative investigative role. In practice, Bowley's distinction between 'statistician' and 'investigator' is best seen as being a distinction between *roles* as distinct from *individuals*: the 'statistician' and the 'investigator' might well be one and the same person but performing different roles at different times on a given project. Bowley's absolutist vision of 'facts' (with the implication that 'facts' are known with certainty) is also to be noted, as is his vision of the dual role of statistics: they may be used either as the basis of theory or as a test of theory. The confidence he shows in his equivalence between 'statistic' and 'fact' is intriguing, for he also wrote at length (and frequently) on the reliability and precision of statistics.

As a collector of statistics Bowley devoted considerable effort to the determination and accumulation of high-quality data; in *Statistical Studies*

Relating to National Progress in Wealth and Trade Since 1882 (Bowley 1904), he identified five stringent criteria for the acceptability of statistics, namely that statistics should be:

1. comprehensive;
2. correspond to theoretical constructs which are to be measured;
3. attention should be paid in time series to both trend and fluctuations;
4. measurements in units of money should be real not nominal; and
5. in examining the change in a variable which has a number of components, changes in individual elements should be analysed.

Bowley proceeded to test the then publicly available data against these criteria and concluded that,

It is humiliating to have to admit that our positive knowledge is so limited, and it is natural to ask whether more cannot be done in the way of official or private investigation ... The fault, if fault there be, must be attributed to the general public, who have made no effective demand for more complete information, and to the successive Governments, who have not recognised our stupendous ignorance of matters of vital importance as an evil calling for a remedy (ibid.: xii, xiii).

Bowley's criteria are not without their inherent difficulties: in previous work, he had proposed that 'the statistician furnishes the political economist with the facts', a view which failed to acknowledge that 'facts' are always relative to the theoretical window through which the world has been viewed. In *Statistical Studies*, his second criterion makes it explicit that statistics ('facts') are dependent on theory by virtue of their correspondence with theoretical constructs. This particular point was never developed further by the pragmatic Bowley.

Bowley returned to the theme of the quality of statistics in a paper read before the RSS in 1908 where, in acknowledging that his paper had an unusual object, namely an argument for further improvement of official statistics, he argued that it was necessary for public opinion to persuade governments of the need to improve official statistics (Bowley 1908). Bowley's confidence in the power of public opinion was not compelling and Leo Chiozza Money, in discussion, suggested that the RSS itself would provide a more effective lobby. In fact, the RSS had previously lobbied for better official statistics, but without success, due no doubt in some part because the lobby was seen by government statisticians as critical of their efforts. Bowley himself fuelled this defensive position by his remarks that 'the "official view" is that everything published under the [g]overnment's authority is accurate,

that the facts are just so ... Every statistician knows that the true meaning of published official statistics is quite different from their face meaning' (ibid.: 475). It is interesting to note Bowley's distinction here between a 'fact' (that which is 'just so') with a 'statistic' (an estimate to which a measure of precision can be attached).

Bowley said of official publications in his Presidential Address of 1906 to Section F of the BAAS: 'It is a sad reflection that, while so much care and labour are spent in accumulating and printing statistical tables, so few of them are of any real importance, and so few are intelligible, even to one who studies them carefully' (Bowley 1906: 542). In that same address, Bowley complained bitterly of the lack of coordination between the various government departments and declared: 'We need a central thinking department in statistics' (ibid.: 543); he also criticised the official view that no figure should be reported unless it was an ascertained 'fact' which might be defended as exact in a court of law. Bowley argued that the pursuit of accuracy delayed the publication of statistics and necessarily made their construction and collection more expensive (in both time and money) and proposed that the very field of statistics precludes numerical exactness while the pursuit of exactness led to the production of useless statistics which failed to correspond to any useful measure. The scientific enquirer is 'left in the position of a man who inquires a distance in France, and is told that it is 8.543 kilometres along the high road, and then some way along a path; the precision of the first measurement is useless to him' (ibid.).

It is interesting to note the paradox between Bowley's criticism of the pursuit of accuracy in official statistics and his vision of the statistician as a disinterested technocrat who amasses 'facts'. In his theoretical work, and in his practice, Bowley clearly knew that no statistic is a 'fact' and his common focus on 'the errors inherent in statistics' leads one to view his statements about 'facts' as less dogmatic than is apparent. His use of the word 'fact' should not be taken literally; by 'fact', he meant a reliable figure whose errors were in principle knowable, but in pursuing his goal of improving official statistics his rhetoric strayed from his intellectual principles.

In his second text on statistics, *An Elementary Manual of Statistics*, first published in 1910 and going through seven editions by 1951, Bowley again articulated his desire to see a national office of statistics established in the UK: 'There is urgent need for more systematic and more complete national statistics' (Bowley 1910: 5), and also warned against the potential dangers of statistical analysis which 'is dangerous in the hands of those who do not know its use and deficiencies. A knowledge of methods and limitations is necessary, if only to avoid being misled by unscrupulous or unscientific

arguments' (ibid.). That much of this problem (avoiding being misled by those who misuse statistics) could be addressed by the proper training of civil servants was very much within the founding *raison d'être* of LSE. Hayek (1946: 7) says that in looking for its first Lecturer in Statistics, the School sought to deliver 'not statistical theory, but statistics for junior civil servants'. Whether or not Bowley saw his crusade as within LSE's mission, he nonetheless pursued his cause with vigour over the years. His standing within the profession led to numerous invitations to take part in official inquiries, and his loud and clear voice proposing not only the creation of a Central Statistical Office (CSO) but also a professionally trained statistical class within the civil service was made manifest in a petition of 1919 to the government. This petition was signed by the President and members of the RSS and by members of other statistical groups, and called for a public inquiry into 'the system under which it is [civil servants'] misfortune to find their efforts frustrated' (Bowley 1921b: 302). The inquiry was established and in 1921 presented its findings in the *Report on the Collection and Presentation of Official Statistics*. The outcome was most disappointing in the short run and led to Bowley's critical response to the report (Bowley 1921b). The efforts (of many) to establish a CSO were maintained through the next two decades and, with the need to better understand the resources available to conduct hostilities (the added impetus of the Second World War), eventually saw the establishment of the CSO in 1941. The first official estimates produced by the CSO were of national income and expenditure for 1938 and 1940, based significantly upon the methods introduced by Bowley and Stamp.

5 Sampling Theory

In the 1930s, Bowley was to become an early pioneer of econometrics, but the foundations of his work as an econometrician were laid on his understanding and appreciation of the technical aspects of statistical theory. His work as an applied statistician, and especially his applications of sampling, has been discussed above. But he was not only an applied statistician but also a serious theoretical statistician.

By the mid-1920s, Bowley was recognised as a major applied statistician and this reputation resulted in his being invited to be a member of a committee, established in 1924 by the International Statistical Institute, to study the representative method in sampling. Bowley had been elected to the Institute in 1903 (the year in which the Institute had, at its Berlin Meeting, formally

endorsed the representative sampling method as proposed by Anders Kiaer at several earlier meetings). The 1924 committee recommended the use of sample surveys which were constructed so ‘as to allow of a mathematical statement of the precision of the results, and that with these results should be given an indication of the error to which they are liable’ (Jensen 1926: 378). The theoretical foundation of Jensen’s conclusion was a separate 62-page article by Bowley entitled ‘Measurement of the Precision Attained in Sampling’ (Bowley 1926). His extensive appreciation (Bowley 1928) of Edgeworth’s contributions to statistical theory is also significant. The importance of the 1926 paper cannot be overstated. Bowley’s concern was not only with simple summary statistics, but also with the functional form of distributions, a concern born of his Bayesian standpoint. Bayesians, required to mix prior information with data, have a particular concern with functional forms. Bowley owed his Bayesian perspective to Edgeworth; it was Edgeworth to whom Bowley turned in 1895 on his appointment to LSE and Edgeworth had given ‘careful consideration to the objective frequentist...view, but in the end adopted the inverse probability, or Bayesian view’ (see Stigler 1978: 296). The influence of Edgeworth on Bowley’s statistical theory is evident in his *Elements* (even though Bowley, ever modest, remarked that ‘there is little that is wholly original...in this book’ (Bowley 1901: viii)).

The ‘Measurement of the Precision Attained in Sampling’ is essentially Bayesian. In it, Bowley provides a theoretical demonstration that stratified random sampling is superior to simple random sampling when judged by the precision of the estimators; he also proposed a very weak prior distribution, weaker than the generally uninformative uniform prior. Finally, he recommended the use of purposive selection (which is similar to, but different from, representative sampling). In fact, this section of Bowley’s paper is seriously flawed and was severely criticised by Neyman (1934) who dismissed it as a special case of stratified random sampling by groups and showed that the estimators so obtained were neither consistent nor efficient. Neyman’s paper, read to the RSS, is of importance as it was the final, and fatal, word on purposive sampling (and it might be noted that Bowley, perhaps not wanting to be embarrassed, ventured that he had had his doubts about purposive sampling even at the time he was writing about the method). However, of much more importance, Neyman (1934) introduced the concept of the confidence interval to the statistical community. To appreciate the hostile welcome Bowley afforded the confidence interval, it is important to recognise that Neyman was a non-Bayesian. Bowley gave the vote of thanks, and remarked: ‘I am not at all sure that the “confidence” is not a “confidence trick”... Does it really lead us to what we need—the chance

that in the universe we are sampling the proportion is within these certain limits? I think it does not' (Bowley in *ibid.*: 609). Neyman, in a written reply, quite rightly observed that Bowley's question

contains the statement...in the form of Bayes...the solution of this problem *must* depend on the probability law a priori [but] the present progress is connected with...solving some other mathematical problem...which has a solution independent of...the probability law a priori ... Both [approaches] are dealing with probabilities, but these probabilities apply to different events' (Neyman 1934: 623–624; italics in original).

History tells us that Neyman's confidence interval is now the standard.

Bowley, the Bayesian, was equally acerbic towards Fisher's 1935 paper on inductive inference. Bowley did not appear to welcome any theoretical advances by non-Bayesians and employed sarcasm in thanking Fisher 'not so much for the paper that he has just read to us, as for his contributions to statistics in general' and concluded that Fisher's paper did not represent any advance: 'We are therefore left very much where we were' (Bowley in Fisher 1935: 55–56). Fisher replied that while Bowley may have failed to fully appreciate Neyman and Fisher's work, 'at least...Dr. Neyman and myself have not been left in his company' (Fisher 1935: 77).

Bowley was not to be moved by non-Bayesian thinking.

6 Mathematical Economics

Bowley was, not surprisingly given his undergraduate degree, an accomplished mathematician but a yet more accomplished mathematical economist. In 1913, whilst Lecturer in Mathematics at Reading, and having added mathematics to the syllabus at LSE, he published *A General Course of Pure Mathematics from Indices to Solid Analytical Geometry*, based upon his lectures. There were few texts on mathematics written for social scientists and no text in English on the subject of mathematical economics. Bowley's 1913 volume was written to include most of the results 'needed by those who use pure mathematics as an instrument in mechanics, engineering, physics, chemistry and *economics*' (Bowley 1913b: iii; italics added). The text was widely welcomed, but the great Cambridge mathematician, G.H. Hardy, was critical (Hardy 1914: 394); however, Dale and Kotz (2011) conclude that 'Hardy's assessment...may be seen as harsh and unjustified: we have found the book to be quite adequate in general' (*ibid.*: 399). Seen as an exposition of mathematical results to be applied outside mathematics per

se, Dale and Kotz's endorsement might be seen as half-hearted: it is arguable that rigour in deriving mathematical results is neither necessary nor sufficient for their proper application. Indeed, one might argue that Bowley's lack of rigour actually facilitated and accelerated the application of mathematics in economics.

In 1924, Bowley published *The Mathematical Groundwork of Economics*. This was the first text, in English, of economic theory in mathematical language and it represents a landmark: it synthesised previous work and brought a number of results to a wider and English-speaking audience. Moreover, as a result of this volume, a number of mathematical techniques came to be generally accepted. Bowley's intention was 'to reduce to a uniform notation, and to present as a properly related whole, the main part of the mathematical methods used by Cournot, Jevons, Pareto, Edgeworth, Marshall, Pigou and Johnson' (Bowley 1924: v). That this was achieved is evidenced by Margaret Tappan who said in review: 'Professor Bowley, needless to say, knows what he is about; and what others have been about becomes clear as the disguises...fall away in his hands' (Tappan 1925: 334).

At the time of writing, mathematical economics was in its infancy. The main achievement of *The Mathematical Groundwork* was to promote the inherent value of mathematical economics and to elucidate and popularise various concepts best approached through the use of mathematics. For example, the indifference curve, the contract curve and the derivation of properties of the demand curve, all appear in the first chapter alone. As evidence of the popularising role of *The Mathematical Groundwork*, it might be noted that Edgeworth had introduced his box—a now common tool in general equilibrium analysis which allows the study of the interaction of two individuals trading two different commodities, and relying on the use of indifference curves—in his book *Mathematical Psychics* in 1881. This was developed into the now familiar box diagram by Pareto in his *Manual of Political Economy* (first published as Pareto (1906) in Italian). However, following Bowley's exposition in 1924, the modern version of the diagram is commonly referred to as the Edgeworth–Bowley box.

Bowley's mathematical approach to the analysis of the demand curve enabled him to derive a version of the Slutsky equation (hardly known, if at all, at the time as it had only been published in an obscure Italian journal in the war years). In his treatment of duopoly, Bowley introduced both the reaction curve and the concept of conjectural variations (Bowley 1924); he also noted that, in contrast to Cournot (1927; first published in 1838), the variations may be nonzero. Bowley, quite typically, is concerned with the conditions of solutions to practical problems; this is illustrated most admirably by

his analysis of production and exchange under market structures from perfect competition to monopoly. It is a remarkable book and was well received by the profession in reviews, though its impact upon the use of mathematics in the common discourse of economics was very limited. Allyn Young remarked that *The Mathematical Groundwork* is the ‘best guide available to the student who seeks to acquaint himself with the methods and results of modern mathematical economics’ (Young 1925: 133); Edgeworth found the volume to be a ‘clear, concise and correct statement of the leading propositions *and methods* which mathematics contributes to Political Economy’ (Edgeworth 1924: 430; italics added) and Tappan remarked that by ‘selection, by improved variant forms of analysis, by the ordering of the matter of [previous] authors...he has made a whole which is more than the sum of its parts’ (Tappan 1925: 338). Wicksell (1925) provided, along with a review, a list of ‘errata’ which is the subject of close examination in Darnell (1982). Wicksell was certainly not wrong when he suggested that ‘the book would be better for those who are already engaged with these problems rather than beginners’ (Wicksell in Lindahl 1958: 209), but he could have been yet more accurate had he suggested that the book would be better for those who are *approaching these problems through mathematics* rather than those ‘already engaged with these problems’; it was not the problems per se which were the obstacle, but the use of mathematics at a time when the majority of the profession could not even speak the language, did not know its vocabulary nor its syntax. The role of mathematics in economics, its history, and Bowley’s contribution is further explored in (Darnell 1981, 1982, 1991). In summary, *The Mathematical Groundwork* had ‘little contemporary impact on the profession and the more advanced text of Evans (1930) was even less successful’ (Darnell 1991: xix).

7 Econometrics

Although Bowley recognised that the statistical method might be used to test theories, his own empirical work was more descriptive and exploratory. Bowley’s major written contribution to econometrics was the path-breaking text *Family Expenditure* (1935) (with R.G.D. Allen). Allen had joined LSE in the 1930s and was among a notable group of academics recruited by Lionel Robbins. (Robbins had been appointed Professor of Economics in 1916 at LSE following the sudden death of Allyn Young. He created one of the world’s leading centres for economics by recruiting a formidable group, including Kaldor, Hayek, Allen, Durbin, Lerner, Webb, Edwards and

Robertson; Hicks was already there when Robbins was appointed Professor, and Ronald Coase, originally a student, was persuaded to return as a member of staff.)

Family Expenditure is an exemplar of its time, whose purpose was 'to discover how far the expenditure of individual families... can be described by rules and formulae, to relate any rules that are found to the postulates of economic theory and to describe the variations from the averages that result from the different choices of individual families' (Allen and Bowley 1935: 1). The methodology was strictly 'measurement before theory', and wholly in accord with Bowley's exploratory data philosophy. A purpose of this work, then, was to identify the statistical and empirical relationships between variables and then use those observations (what Bowley sometimes referred to as 'facts') as the starting premises for theoretical work: 'In so far as these formulae are of general application, they should not only have immediate practical use but should also provide economists with fundamental material for theoretical analysis' (ibid.: 4) and having fitted Engel curves to cross-section data by eye 'we can formulate and examine these results objectively without any theoretical basis' (ibid.: 8). An example of using the results 'objectively' and as the basis for further theoretical work, Allen and Bowley observed that the linear Engel curve for food fitted poorly at high-income levels and that this was 'probably due to the larger numbers of children in this range' (ibid.). They then proceeded to examine this hypothesis by disaggregating their data into those expenditures which were more 'fixed' (identified as including, e.g., rent, heat and light) and those which are more variable and dependent on the family composition (identified as including, e.g., food and clothing). The analysis of the former expenditures was then appended by an additive linear term in family size but for items in the latter category the expenditures were scaled by dividing them, not by 'the actual number of individuals [in the household], but by a number based on a scale of needs in which allowance is made for age and sex' (ibid.: 19). Bowley had introduced the concept of equivalence scales in the five towns study of 1915, and with Allen he developed this into equivalent adult scales, one of the earliest uses in the new 'econometric' style of work. They recognised that the equivalence scales could be constructed by a variety of methods and, in the absence of theoretical criteria by which to choose any one, they reproduced their empirical analysis using each method in turn only to conclude that 'the various scales appear to give nearly the same results' (ibid.: 20).

An important development in their analysis of Engel curves was to test economic theory; for example, theory proposes that the sum of income elasticities of demand, weighted by budget shares, is unity. Allen and Bowley tested this, found general acceptance and, in yet more innovatory fashion,

employed diagnostic specification tests and a χ^2 goodness-of-fit test. They also examined in some detail the distributions of incomes and expenditures, finding support for log-normality. This study was instrumental in the development of applied econometrics and set some early standards, and most importantly on the question of the interaction between data and theory: while keen to use empirical observation to prompt theoretical investigations, Allen and Bowley were also prepared to test theory.

8 Conclusion

Arthur Bowley was an economic statistician of the highest rank; he made numerous pioneering contributions to statistical theory and practice and made very substantial contributions to the profession via his active involvement in the major national and international professional organisations. He pioneered the use of mathematics in economics and was an early econometrician. His work on the measurement of changes and his work which focused on the promotion of social welfare were of the highest order. There was nothing he did that was without practical, or at least potentially practical, application.

Bowley was a very modern economist whose work was significantly ahead of its time. He was a heavyweight in both applied economics and statistics and his seminal work in social investigations, where he pioneered sampling techniques, exemplifies his studious approach to serious and important questions par excellence. His work stands the test of time and today's reader will be enriched by the study of Bowley's contributions to our profession. Above all, his work, both theoretical and applied, advanced the development of an empirical evidence base in the social sciences.

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9

William Henry Beveridge (1879–1963)

Atsushi Komine

1 Introduction

This chapter deals with William Henry Beveridge (1879–1963). Generally, he has rightly been regarded as one of the founders of the modern welfare state. However, in most cases, as typically described by Robbins (1971: 158) and Hayek (1994: 83), he is often viewed as an amateur economist who made almost no contributions to the economic discipline. This chapter challenges the latter part of this view by re-evaluating Beveridge's contributions as an LSE economist from three standpoints. After briefly sketching his life in Section 2, the rest of the chapter addresses several key areas that challenge the view of Beveridge as insignificant as an economist. Section 3 examines his contributions to economic analysis in two regards, namely empirical works, and the modern theory of unemployment. Section 4 focuses on his involvement in the 'professionalization' of modern economics by paying special attention to LSE personnel affairs and the Association of University Teachers of Economics (AUTE), an academic society. Section 5 attempts to understand Beveridge's evolving yet coherent ideas in the intellectual history of economics from three perspectives, personal exchanges, a new type

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of economic idea and sublimated welfare ideas. Lastly, Section 6 reconsiders Beveridge's contributions as well his status as an LSE economist and concludes by considering his influence as a whole.

2 A Biographical Sketch¹

William Henry Beveridge was born in Rangpur, Bengal, on 5 March 1879. His father, Henry, was a judge in the Indian Civil Service, and his mother, Annette (née Akroyd), was a volunteer social worker. His parents were also experts on Hindi and Persian texts. Growing up in an atmosphere of both the modest and intellectual middle class within the British Empire during the late Victorian era, W. Beveridge was educated at the prestigious Charterhouse School. He later attended Balliol College, Oxford, a sanctuary of British idealism. Although he took first place in both Classical and Mathematical Moderations, he was still unsure of which path to take. Should he continue within academia, or should he turn to law for which he proved his exceptional abilities when he obtained the Stowell Civil Law Fellowship at University College, Oxford, that his father had highly recommended? In the end, he chose a rather rare vocation for men at that time and chose to become a social worker in the East End of London. Beveridge was scouted by Samuel Barnett for the position of sub-warden of Toynbee Hall, a university settlement house, where the young elite and people from the slums lived communally and were expected to influence each other. Instead of being touched by emotion or charity, Beveridge sought to identify realistic remedies to abolish casual labour, which was considered the primary cause of poverty typically observed in and around the London docks.

However, this part of Beveridge's career lasted for only three years. In 1906, he accepted a position as lead writer for the *Morning Post*, a rather conservative daily newspaper. Gradually, he became recognised as an expert on unemployment problems and was subsequently asked by lifelong friend Beatrice Webb to testify to the Royal Commission on the Poor Laws and Relief of Distress in October 1907. He worked to advance his career and his next position was that of a permanent civil servant at the Board of Trade, the President of which was then Winston Churchill. While serving in this capacity, Beveridge was put in charge of the Labour Exchanges Act 1909

¹This section mostly depends on *The Economist* (1963), *The Times* (1963), Beveridge (1953) and Harris (1977, 1997, 2008).

and National Insurance Act 1911, among others. Once the First World War broke out, he switched to the Ministry of Munitions, and later the Ministry of Food, where he became Permanent Secretary in 1919. Towards the end of his term in Whitehall, Beveridge caused two commotions. The first was to remonstrate against wrong figures in his salary as it was not £1500 but rather £1750 annually.² When Beveridge retired from the Ministry of Food, the Treasury asserted his salary as £1500, but Beveridge claimed it as £1750, the amount the Treasury finally agreed to. This episode tells us both about his deep concern over his salary as the basis of his living expenses and why he frequently changed jobs in pursuit of higher remuneration. The other was to claim a full pension payout for a ten-year officer before retirement.³ Taken together, these two episodes indicate that he was clearly focused on his salary, and that he perhaps realised why a comprehensive but not overlapping system for pensions became necessary for all.

From there, Beveridge continued to play an active part in academia, government and political circles, much like another prominent contemporary economist, John Maynard Keynes. However, there was a clear difference between the two: a sense of distance from business groups. Keynes was closely connected with both monetary and commodity markets and displayed a formidable talent as an investor, dealing with stocks, debts, futures, famous paintings and rare second-hand books, for himself, his friends, his academic institutions (King's College and the Royal Economic Society) and the companies who employed his services. On the other hand, Beveridge invested just twice in his entire life (Beveridge 1953: 93, 216) and had no assets to increase, no time to invest and no targets to spend his dividends on.

Disappointed with his life in Whitehall, Beveridge stopped working for the Ministry of Food and accepted an offer to become Director of LSE upon the request of the Webbs in 1919, with a salary of £2000. Beveridge and his Secretary (Janet Mair), later Lady Beveridge, were often accused of running a dictatorship rather than a Directorship.⁴ Early in LSE's development, their style was, however, useful: using sizeable external grants, Beveridge drastically changed LSE from a tiny local college in London into a great international university at the centre of the business and academic worlds in the UK.

²Three letters between George H. Roberts and Austen Chamberlain, February 1919, Papers Registered in 1919 (T 1/12286), The National Archive (hereafter TNA), Kew, London: Minute 2.a. 39675/18.

³Beveridge to the Secretary of the Board of Trade, 6 October 1919, Minute. Resignation of Sir W.H. Beveridge from the Post of Assistant Secretary, Board of Trade: Claim to Superannuation Allowance (45538/19), TNA: T 1/12399/45538.

⁴See MacKenzie and MacKenzie (1985: 372).

But that same style, no longer applied to the later stage of his Directorship. Between the late 1920s and the first half of the 1930s, Beveridge faced growing criticism from almost every member of LSE. Professors such as Harold Laski, Lionel Robbins, Friedrich Hayek, other staff, students and even Beatrice Webb complained of his authoritarian ways. Eventually, Beveridge was forced to resign from the Directorship, leaving LSE to become Master of University College, Oxford, in 1937.

Although his personality left him sometimes unlikeable, Beveridge's competence in his specialised fields, including unemployment benefits and workmen's compensation, kept him well known and led to governmental appointments such as Chair of the Unemployment Insurance Statutory Committee and as Commissioner for Man-Power Survey. Naturally, when the government sought a proper candidate to draft plans for workmen's compensation and social insurance,⁵ Beveridge was appointed Chair of an Inter-Departmental Committee on Social Insurance in 1941. This Committee was transformed into a broader body charged with developing a comprehensive post-war programme. The result was the White Paper, *Social Insurance and Allied Services* (the 'Beveridge Report') (Beveridge 1942), that would serve as a blueprint for the British welfare state. It was a symbolic effort of 'the other war' (Beveridge 1953: 273) and conceived as an imaginary plan to motivate common people by way of presenting a positive post-war vision, this in contrast to Churchill's Battle of Britain, a real plan for armed men.

Having been frustrated by the Beveridge Report, Churchill ignored most of its recommendations and instead published a different White Paper, *Social Insurance* (UK Government 1944a). In addition, the government, in a rush due to a forthcoming private report from Beveridge, decided to issue a White Paper on maintaining high levels of employment (UK Government 1944b). A few months later, Beveridge published his own report, entitled *Full Employment in a Free Society* (Beveridge 1944). After the publication of his two reports, Beveridge published a third private report, proposing a plan to deal with the modern and complex lives faced by ordinary people following the war, including voluntary action (Beveridge 1948). Additionally, he frequently discussed war and post-war problems in the 1940s (see Beveridge 1945).

Enthusiasm for the Beveridge Report gave Beveridge the popularity needed to win a parliamentary by-election in 1944, although this also

⁵Beveridge (1924) is one of the earliest manifestations of his support for the need for universal social insurance.

meant that he would lose his salaries of £1000 and £1800 as Chair of the Unemployment Insurance Statutory Committee and Master of University College, respectively. He was an M.P. for only ten months when he lost the next election at the same time as Churchill left office in July 1945. However, Beveridge accepted a proposal from the Liberal Party to become a member of the House of Lords in 1946. In the latter part of his career, Beveridge, now Baron Beveridge of Tuggal, mainly focused on two social problems: world peace, and the rebirth of community in urban life. Despite his hard work and devotion, his remedies for these two problems did not amount to specific solutions. In the final stage of his life, finding himself dependent on the very pension system he created, Beveridge became lonely. He lost his sister, Jeanette, in 1958 and his wife in 1959. His best friend and Jeanette's husband, R.H. Tawney, a Christian socialist and former Professor of Economic History at LSE, died in 1962. Beveridge described himself at this time as 'a busy and rather unhappy old man'.⁶

Lord Beveridge died on 16 March 1963, at his Oxford home at the age of 84. His last words, 'I have a thousand things to do' (as quoted in the *Oxford Mail*, 18 March 1963: 7), remind us of his busy and earnest life and his dedication to public service. A further example of this dedication was his selection of the title of his autobiography, *Power and Influence* (Beveridge 1953). Influence here means 'changing the actions of others by persuasion' and 'appeal[ing] to reason or to emotions other than fear or greed' (ibid.: 3). The title also reflected his belief in liberal European values and his opposition to dictatorial and excessive pecuniary powers.

3 Contributions to Modern Economic Analysis

Beveridge's analytical contributions to modern economics are divided into two primary areas. The first attempts to derive from empirical data general laws of macroeconomic phenomena, in particular movements in prices and wages. The second attempts to create a new epoch by creating a modern theory of unemployment, which stimulated subsequent scholars as a first step to the modern treatment of the unemployable, the unemployed and unemployment itself.

⁶Beveridge to Ethel Marjory Beveridge Gwilt, 14 September 1960, William Henry Beveridge Papers (hereafter BP), BLPES: IIa-112.

3.1 Empirical Works

Regarding the first contribution, collecting historical and current data easily connected with Beveridge's promotion of 'ideal' social sciences, including economics, which we will address later in Section 3. Beveridge was continually interested in empirical analysis from 1914 to 1960 (Beveridge 1914a, 1960a). While he considered this to be his life's work (Beveridge 1939), it was never fully realised in a sufficient way. However, his contemporaries acknowledged his vigorous engagement in empirical works. For example, Beveridge served as the President of the Royal Statistical Society (RSS) from 1941 to 1943. He also established the Association of Incorporated Statisticians Limited,⁷ later renamed the Institute of Statisticians, and served as its President from its inception in 1949 to his death in 1963.

Clive Granger, an eminent econometrician and a Nobel Laureate in Economic Sciences in 2003, places great value on Beveridge's empirical works concerning medieval and early modern Europe. He wrote that Beveridge (1922) provided 'not only the most sophisticated piece of analysis of economic historical data but also the most extensive calculation in the field of time-series analysis before the war' (Granger and Hughes 1971: 413). More recently, Baillie (1996: 44) describes the Wheat Price Index in Beveridge (1921a) as 'well-known', while Korotayev and Tsirel (2010: 1) explain that Beveridge's 1922 article is an 'important Kondratieff predecessor', as it discovered that one of the long-term cycles of wheat prices had an 'average periodicity of 54 years'.

While perhaps rather naively, Beveridge's zeal in his attempts to collect data is in fact one element of the foundation for subsequent developments in econometrics and statistics.

3.2 Modern Theory of Unemployment

Beveridge's second major and most conspicuous contribution to economics was creating a modern theory of unemployment.⁸ It was so inspiring that countless scholars have explored this theme based on Beveridge's

⁷The Royal Statistical Society and the Institute of Statisticians had similar missions, although the latter placed 'rather more emphasis on statistical administration' (*Nature* 1949: 605). The Institute of Statisticians was merged into the RSS in 1993.

⁸Unlike our evaluation, Garraty (1978: 136), Freedman (1978: 211) and Casson (1983: 25) are much less positive concerning Beveridge's originality in the theory of unemployment.

comprehensive analysis. *Unemployment: A Problem of Industry* (Beveridge 1909) is considered his magnum opus.⁹ It sold well and became one of the Tripos textbooks in Cambridge. At first, he transformed the unemployment problem from the nineteenth-century type of exceptional irregularity in casual labour and personal defects (such as laziness) into the twentieth-century one that incorporated issues such as regular fluctuations in industry and macroeconomic phenomena. Pigou (1913: 253), one of the earliest works on unemployment by an orthodox economist, observed that *Unemployment* was the ‘most elaborate British book’ and ‘a work deserving study by all interested in the subject’. The ‘friction of the labour market’ (Beveridge 1909: 81), owing to distance, ignorance or custom, results in casual labour and the reserve of labour. As a result, Beveridge advocated the decasualisation of labour by way of nationwide Labour Exchanges.

Beginning in the middle of the 1920s, Beveridge elevated his own ideas on unemployment. However, due to firmly established hostile views such as Robbins (1971: 158) and Hayek (1994: 83), it is currently widely accepted that Beveridge had little understanding of economic theories. On the contrary, he recognised the deficiencies in the latest theories on industrial fluctuations. To obtain a doctoral thesis, Beveridge published a revised book (Beveridge 1930), and it included a portion of his 1909 work as well as a newly written section. In this section, he critically pointed out that Hawtrey’s credit cycle had a crucial ambiguity as to how switching points (peaks or troughs) of the trade cycle should be explained (*ibid.*: 331). On a related point, Beveridge rejected the Treasury View,¹⁰ stating that, ‘Clearly the dogma...is untenable’ (*ibid.*: 414). Dennis Robertson came to the view that *Unemployment* was a masterpiece comparable to Smith’s *Wealth of Nations* and Bagehot’s *Lombard Street* thanks to its exquisite blend of abstract theory and empirical evidence (Robertson 1931: 74). In his *Causes and Cures of Unemployment*, which appeared in 1931, Beveridge suggested that his ideas were evolving. For example, he added historical explanations from the 1920s and pointed out the special characteristic of labour, noting that the price of it should not fall below a subsistence level, and accordingly, the minimum wage legislation contained in the Trade Boards Act 1909 was justified.

⁹Beveridge (1909) was ‘to become the classic text on unemployment for the next quarter of a century’ (Harris 1997: 166). Regarding the creative process of this work, see Komine (2004).

¹⁰The Treasury View was that almost no additional employment can be created by State borrowing and State expenditure. This dogma was supported by Churchill and became the key issue of the 1929 general election.

The antipathy towards Beveridge's Directorship of LSE among many of its staff was partly related to his own distaste for pure theorists. Thus, it might have been natural for him to reject Keynes's *General Theory* when he judged that its concepts, such as involuntary unemployment, were disconnected from reality.¹¹ However, it was also the case that Beveridge later accepted the *General Theory* after leaving LSE. Through an intimate exchange of letters and conversations with Keynes, he became convinced that they could apply such theory to economic policies to resolve unemployment. In turn, Keynes admitted that Beveridge's historical works fitted well with his own theory that fluctuations in investment were the prime mover in the cycle.¹²

Even while in the process of absorbing Keynes's demand-side macroeconomics, Beveridge advanced his own theory of unemployment in three respects. First, he uniquely defined full employment¹³ as 'more vacant jobs than unemployed men' (Beveridge 1944: 19). It was also such unemployment that did not last past the coverage of 'unemployment insurance without risk of demoralization' (ibid.: 20). His approach included both macroeconomics (vacancies) and microeconomics (demoralisation). Second, this characteristic brought a starting point for investigating the so-called UV curve (an inverse relation between unemployed workers and job vacancies) and job search theory: 'The number of vacancies...always exceeds considerably the number filled by them and the difference is greater in good than in bad years' (ibid.: 88). Beveridge's previous works and *Full Employment in a Free Society* provided a basis for later developed theories such as the UV curve and related topics. Thanks to both Malinvaud (1987) and Blanchard and Diamond (1989), this curve is now called the Beveridge curve, an acknowledgement of Beveridge's important contributions to this field.¹⁴ Third, structural unemployment was clearly defined by Beveridge as follows: '[T]he unemployment arising in particular industries or localities through a change of demand so great...as affecting the main economic structure of a country' (Beveridge 1944: 409). These three points are supplemental to the Keynesian approach to macroeconomics.

¹¹'Employment Theory and the Facts of Unemployment' by W.H. Beveridge, c. 1936, John Maynard Keynes Papers (hereafter KP), King's College Archive Centre, Cambridge University: GTE/2/1/5-21, 1-17.

¹²See Keynes to Beveridge, 2 February 1939, BP, BLPES: Iib-38.

¹³For Keynes, full employment meant unemployment without involuntary factors, yet included voluntary and frictional ones.

¹⁴Regarding the origin and development of the curve, see Yashiv (2008) and Rodenburg (2011).

Based on a comprehensive diagnosis, Beveridge advocated a package of three remedies for unemployment. The first was to sustain effective demand (*ibid.*: 29), similar to orthodox Keynesian theories. However, the concrete measure ‘by socialization of effective demand’¹⁵ (*ibid.*: 191), rather than by a usual deficit budget, was much closer to the position advocated by Keynes himself. The second was to control the location of industry to avoid the misdirection of demand. The third was to mobilise insufficient or ill-guided movements of labour by organising the labour market more efficiently (*ibid.*: 125). Here, Beveridge’s earlier supply-side diagnosis remained, though weighted differently, even after absorbing Keynes’s demand-side macroeconomics. Beveridge’s three remedies represented a comprehensive set of practical solutions in accordance with the observed diverse causes of unemployment.

By collecting empirical data and by absorbing the latest theories on trade cycles, Beveridge suggested a coherent diagnosis in order to, in turn, lessen the negative impact of industrial fluctuations by way of discretionary policies. The diagnosis seems eclectic, but it still applies to the complicated reality of modern economies.

4 Contributions to Professionalisation in Economics

Beveridge’s second primary contribution was to promote the ‘professionalization’¹⁶ of economics in Britain. Since 1885, Alfred Marshall had advanced the discipline by way of establishing an academic journal and a society, writing a textbook and finally, creating an Economics ‘Tripos’ at Cambridge (see Komine 2014: 80–81). After the Second World War, American universities promoted economics in the light of social engineering and rigorous mathematical models. These two movements have received significant attention (see, for example, Coats and Coats 1970; Tribe 1992). However, little attention has been given to the fact that between the wars, Beveridge attempted (and failed) to establish another direction for economics and the

¹⁵A National Investment Board should ‘have powers of obtaining intelligence, of giving assistance, and of regulating investment by public and private enterprise’ (Beveridge 1944: 177).

¹⁶This included five steps: (i) permanent installation of an academic subject in a university; (ii) permanent installation of a professorship for economics; (iii) establishment of an academic society, a journal and textbooks for economics; (iv) establishment of an independent degree; and (v) the production of numerous able economics graduates.

wider social sciences. His attempts inside and outside of LSE should not be forgotten, as they imply the possibility of diverse potential in economic thinking.

4.1 LSE: Personnel Affairs

As the new Director at LSE beginning in 1919, Beveridge had expanded the School's buildings by using major grants and establishing vibrant, autonomous academic bodies by offering high salaries (with educational allowances) (Beveridge 1953: 170). Two of his first appointments at LSE were Hugh Dalton and Harold Laski. Dalton was an influential pupil of Pigou's and later served as a Labour politician. He played a key role in bridging both the Cambridge School with LSE as well as connecting academic with political circles. Laski was a Labour activist and distinguished political scientist, and in the 1930s became one of the driving forces behind the purging of Beveridge from the Directorship. In addition, Lionel Robbins, previously an LSE undergraduate, became an assistant to Beveridge, a job he gained on Dalton's recommendation. He was later a lecturer, and finally a Professor at LSE and became a supervisor to Beveridge when he revised *Unemployment* and submitted it as a doctoral degree in 1930. Initially assisted by Beveridge and Dalton, Robbins became more independent particularly after Hayek came to London in 1931: their seminars became a symbol of LSE's openness and internationalism in contrast to Cambridge insularity. Before the publication of Keynes's *General Theory*, LSE attracted numerous promising economists, including Hicks, Kaldor, Lerner, Allen and Coase, among others (see Robbins 1971: 131). By creating an attractive academic environment, Beveridge encouraged the formation of a competitive nexus to counter the Cambridge School. The Cambridge members, including Keynes and Robertson, were in turn affected by Austrian and Swedish ideas directly or indirectly through young scholars coming and going between London and Cambridge.

Apart from the above, Beveridge consciously intended to create another direction for the academic development of economics, which linked up with his pursuit of an ideal version of the discipline (see Komine 2016). He established three goals to realise this ideal economics (Beveridge 1921b, 1937, 1953: 247).

The first goal was to establish economics as an inductive science, this a reflection of Beveridge's belief in the scientific method. His efforts at LSE, which ultimately proved fruitless, are understandable when we examine

the direct influence of Beatrice Webb.¹⁷ Like Jevons, Beveridge attempted to make political economy more ‘scientific’. Nevertheless, what it meant to be ‘scientific’ differed between the two: Jevons demanded that economics should be both *strict* (or rigorous) and *exact* by introducing mathematics into economic thinking. In contrast, Beveridge suggested only exactness, that is, to obtain accurate empirical data and predict subsequent phenomena exactly, rather than being strict, that is, to abstractly produce consistent and logical models in theory. As he himself put it (Beveridge 1953: 175), he just followed Beatrice Webb’s concept of ideal sciences, which was based on observation and analysis of the facts.

The second goal was to support new professions such as public administrators and business managers. Beveridge believed that they should absorb specific knowledge concerning modern economies and political phenomena in order to make private interests and public purposes compatible. The third goal was to place economic science as a central subject in the liberal arts. In the event of severe depressions and political crises, having a deep understanding of political and economic conditions was a necessary element of survival for everyone.

Based on the above goals, Beveridge helped promote closer ties between different academic fields, this despite a trend at the time towards specialisation and compartmentalisation within British universities (see Coats 1993: 376). Economics and political science were not sufficient to complete the circle: a third group was necessary, ‘forming a bridge between the natural and the social sciences’ (Beveridge 1960b: 88). According to a report submitted by Beveridge in July 1925 to the Trustees of the Laura Spelman Rockefeller Memorial (see Harris 1997: 280, fn. 8), in addition to expanding or establishing a new professorship, course, department or degree, such as in law, commerce, business administration or modern languages, Beveridge attempted to create new professorships in anthropology, social biology, physiology, economic psychology, public health, geography, agriculture and meteorology, among others. Of particular interest was the attempt to establish a Chair of Social Biology, with this encompassing genetics, population, vital statistics, heredity, eugenics and dysgenics. As a result of Beveridge’s 1925 report, a new subject entitled ‘Biological Factors in Social Evolution’¹⁸ was added to the Department of Sociology at LSE,

¹⁷‘I am sure it is good that people should become Economists as I did by studying some practical question under your guidance and inspiration’ (Beveridge to Webb, 9 May 1927, BP, BLPES: I1b-27).

¹⁸However, this subject seemed to be offered only for three years until 1927/1928. See London School of Economics and Political Science, *Calendar* (1925/1926: 290 and 1927/1928: 173).

with Beveridge inviting Lancelot Hogben to serve as the Chair of Social Biology in 1930. Despite Beveridge's enthusiasm, the Department was not a success and Hogben left LSE in 1936 (see Beveridge 1953: 250). Moreover, social biology (the Chair, the Department and the subject) was completely removed from LSE¹⁹ immediately after Beveridge left the School. In this sense, his attempt to make economics an inductive science had failed.

Beveridge's enthusiasm for establishing a new approach within the social sciences, in which economics and political science were still at the centre but related subjects supported the core, bore fruit in the form of employing two eminent scholars, Bronisław Malinowski and T.H. Marshall. The former accepted the position of Chair of Anthropology in 1927 while the latter was employed as assistant lecturer in social works in 1925²⁰ on Keynes's recommendation (Marshall had been Keynes's pupil at Cambridge).²¹ While Beveridge could not establish a new direction for the economics profession, founded on the greater use of empirical studies and closer links with kindred subjects, he did help to advance diverse disciplines such as anthropology and sociology.

4.2 Association of University Teachers of Economics (AUTE)

Outside LSE, the creation of the AUTE symbolised Beveridge's zeal for the economics profession. The Association attempted to challenge the cliquish nature of existing academic bodies such as the Royal Economic Society (RES)²² and Keynes's Political Economy Club. Initially, it was open to a relatively diverse membership, with its first annual conference being held in January 1924 at Balliol College, Oxford, Beveridge's alma mater. The second annual conference was held at Trinity College, Cambridge, in January 1925. Beveridge presided over a meeting at Cambridge aimed at drafting a constitution for the Association. Its aim was to afford to members the opportunity to meet and exchange ideas, including methods of teaching. Its membership was open to teachers of economics, economic history, sociology, commerce

¹⁹Social biology was offered just once in the normal curriculum. See the LSE *Calendar* (1930/1931: 21, 81, 194), (1931/1932: 198), (1932/1933: 205), (1933/1934: 215), (1934/1935: 223), (1935/1936: 226), (1936/1937: 229) and (1937/1938: 24, 88, 506).

²⁰Later, he became Chair in Social Institutions in 1944, and Chair in Sociology in 1954.

²¹On this matter, see Beveridge to Keynes, 14 February 1925, BP, BLPES: IIB-24.

²²Recent economists have described the AUTE as more akin to a proletarian organisation (Fourcade 2009: 147). See also Coats (1993: 142), Middleton (1998: 42, fn. 32) and Backhouse (2000: 68).

and ‘kindred’ subjects (see *Economic Journal* 1925: 154). Beveridge was at the centre of the dissemination movements in the economics profession, this underlined by his election as an executive member of the AUTE at four consecutive conferences (in 1925, 1926, 1927, and 1930).²³ The main academic subjects referred to in the AUTE’s constitution were similar to those contained in LSE’s curriculum.

However, Beveridge’s efforts petered out after 1930. Although Hogben presented a paper on biological aspects of the population in 1931, Beveridge did not appear to attend the conference. That same year, the constitution of the Association was amended to put university teachers at a slight advantage over others. In 1932, the organisation was renamed the AUTE; it was originally called the Association of Teachers of Economics. Beveridge seemed to have lost his leadership role during the early 1930s, whereas young economists, such as Robbins, Robertson, Henderson, Kaldor and others became more active. Therefore, not only at LSE but also at AUTE, Beveridge found that he could not stand in the way of more ‘academic’ economists, who oriented themselves more towards the ‘purification’ of the subject.

Overall, then, it can be argued that Beveridge contributed to the process of creating modern economics in two respects. First, in the 1920s, by collecting significant funds and capable scholars, he laid the foundation of an international intellectual group at LSE, differentiated from the Cambridge School. Second, until the first half of the 1930s, by taking what might be considered by some as an unpopular position in terms of trying to promote closer links between economics and other disciplines in the social sciences, Beveridge showed to younger generations a renewed political economy, based on empirical and natural elements. In this respect, his efforts should be regarded as a halfway point between Marshall’s initiation and development of the Tripos at Cambridge and the American domination of economics in the post-Second World War era.

5 Contributions to Evolving Economic Ideas

Beveridge contributed to the formation and the evolution of modern economic ideas in three specific areas. First, through personal exchanges with eminent scholars, he served as the driving force behind advancing innovative ideas. Second, he devised a new, dominant type of economic idea, or

²³The conferences for 1928 and 1929 were suspended.

‘management in bureaucracy’. Lastly, he played a role in being a node in the gathering together of three schools of economics, namely Oxford idealism, Cambridge practicality or utilitarianism and LSE scientism and internationalism, and sublimated them into his ideas for the welfare state.

5.1 Personal Exchanges

As a distinguished scholar, Beveridge had numerous intellectual colleagues, including economists. Among them, we examine four eminent professional economists.

The first was A.C. Pigou, Beveridge’s ‘old friend and fellow economist’ (Beveridge 1953: 293). As noted in Section 3, Pigou was greatly influenced by Beveridge’s first book on unemployment. Further, this influence possibly played into Pigou’s strong desire to create a new welfare economics. The Beveridge Papers at LSE hold at least eleven communications²⁴ between the two men from 1925 to 1941. They include a letter (dated 2 November 1925) about an expression of Beveridge’s sympathy concerning Pigou’s heart condition and a letter (dated 28 May 1930) where Beveridge thanks Pigou for his recommendation regarding the Cassel professorship in Commerce at LSE.²⁵ Pigou, in his review of Beveridge’s autobiography, *Power and Influence*, admired his outstanding and ‘sterling service’ (Pigou 1954: 76) as a public administrator and innovative thinker, underlining the mutual respect between the two.

Beveridge noted that John Maynard Keynes was another influential economist and an unforgettable friend. He wrote that ‘Maynard Keynes’s place is secure in history as one of the original constructive and imaginative of minds of his or any other generation’.²⁶ Curiously, in 1914, both Beveridge and Keynes had criticised Pigou’s dichotomy between the plasticity of wages in theory and the impact of a wage cut in practice,²⁷ a critique which may perhaps be interpreted as one of the first steps on the long road to the Keynesian Revolution. They remained in close contact during their wartime efforts in Whitehall and, after the war,²⁸ at Executive Meetings

²⁴See BP, BLPES: I Ib-24, I Ib-25, I Ib-28, I Ib-29, I Ib-32 and I Ib-40.

²⁵In 1930, Arnold Plant was appointed to this position following his return from South Africa.

²⁶‘Some Memories of Maynard Keynes’, n.d. (ca. 1952?), BP, BLPES: IXa-52.

²⁷Beveridge (1914b) and Keynes to Beveridge, 25 March 1914, BP, BLPES: I Ib-13.

²⁸For example, ‘I want your private and most serious advice as one interested in the development of Economic Science and teaching’ (Beveridge to Keynes, 14 February 1925, BP, BLPES: I Ib-24). This relates to Cannan’s retirement from LSE and how economics should be advanced at the School subsequently.

both at the London and Cambridge Economic Service (Cord 2017: 311) and at the RES. When Keynes's *General Theory* was published, Beveridge at first rejected it, as mentioned in Section 3. Beatrice Webb, a mutual friend of the two, found Keynes 'very depressed about the reception of his book, and the hopeless disunity of opinion among abstract economists'.²⁹ As such, both Beveridge and Keynes shared a certain hopelessness concerning orthodox economics. Partly due to this, Beveridge changed his opinion about the *General Theory* at some point during 1938.

Eventually, Beveridge became an enthusiastic supporter of Keynesian economics and policies. In terms of theory, one of the reasons he changed his position was that he came to realise that social security and full employment policies should (and could) be compatible after the post-war era. Another reason is that Beveridge and Keynes became much closer during the Second World War. In September 1939, they organised a critical group against the government, called 'the Old Dogs', consisting of five veterans³⁰ of wartime management. Meanwhile, in March 1940, at Keynes's initiative, Beveridge was elected President of the RES to follow Pigou.³¹ Finally, from March 1942, they frequently corresponded regarding the Beveridge Report.³² When Beveridge published the Report and got married in December 1942, Keynes presented him with a rare book by William Petty, published in 1691, adding in his note to, 'Sir William Beveridge this book by the founder of his (and my) craft' (Keynes quoted in J. Beveridge 1954: 127). The expression 'his (and my) craft' indicates a strong sense of collegiality.

Next, while Lionel Robbins wrote that Beveridge 'presented almost an archetype of the human tragedy' (Robbins 1971: 136), there are two areas in which their collaboration or influence should not be ignored. First, epoch-making Robbins (1932) was closely connected with Beveridge's discourse. Its primary targets for attack were both Cannan's definition of economics from the viewpoint of wealth and Beveridge's naive positivism. In addition, Robbins's definition from scarcity lacked any macroeconomic basis such as the causes of unemployment, the trend of the standard of living, the

²⁹Webb to Beveridge, 13 July 1936, BP, BLPES: IIB-35.

³⁰The other three were H.D. Henderson, A. Salter and W. Layton. They all engaged in war economy during WWI but had not been hired by the government at that time.

³¹BP, IIB-39, a letter from Keynes to Beveridge, 20 March 1939.

³²Beveridge's memoranda on social security left Keynes 'in a state of wild enthusiasm' (Keynes 1980: 204).

fall in birth rate or fluctuations in the rate of interest.³³ Second, the two LSE scholars collaborated on the matter of federalism (Komine 2017), a partnership between nations, and division of labour between a nation and a supra-national institution, to escape the likelihood of war. Indeed, this emphasis on federalism represented internationalism or renewed liberalism at LSE.

While less well known, Roy Harrod and Beveridge shared a mutual respect. After reading Beveridge's LSE farewell address of 24 June 1937 (Beveridge 1937), Harrod wrote that 'Beveridge is one of the best men now in this place [Oxford]'.³⁴ Beveridge, in turn, identified Harrod as the candidate best placed for nomination and then election to the British Academy in 1946.³⁵ Two factors can explain this mutual respect. First, they shared the same methodological approach to the social sciences,³⁶ meaning induction based on the presumption of the quasi-uniformity of social phenomena. Second, they shared the belief that experts' knowledge in economic analysis should be transformed into the political arena. In 1942, Churchill employed Harrod as a member of S Branch, the Prime Minister's Statistical Section, with the highest salary (£1300 a year).³⁷ Beveridge made a campaign speech for Harrod on 26 June 1942, who stood as an unsuccessful Liberal candidate in Huddersfield (Beveridge 1953: 345).

5.2 A New Type of Economic Idea

In the history of economic ideas, we can identify representative or dominant types: these types are revealed if we examine economists' attention to the principal motives by important economic entities at that time. From the ancient period to the medieval era, governance, fairness and appropriateness were popular themes for discussion. After the political and economic revolutions of more modern times, various concepts (such as self-interest,

³³Note on Professor Robbins' Essay on the Nature and Significance of Economic Science', 30 November 1932, BP, BLPES: I1b-32. The 'definition seems to me too narrow' (Beveridge 1937: 462). Acknowledging this view, Keynes further criticised Robbins's methodology in the light of the characteristics of a moral science, dealing with introspection, judgements of value, motives, expectations and psychological uncertainties (see Keynes 1973: 297, 300).

³⁴Harrod to Lindemann, 24 October 1937, quoted in Besomi (2003: 730).

³⁵Beveridge to Keynes, 15 February 1946, KP, King's College Archive Centre, Cambridge University: BA/1/208.

³⁶But with regard to your main contentions about the method and status of the social sciences I am in entire agreement' (Harrod to Beveridge, 24 October 1937, quoted in Besomi 2003: 729).

³⁷Appointment of Lord Cherwell as Paymaster General: Staff and Duties', 4 December 1942, TNA, Kew, London: CAB 21/781.

sympathy, emulation, luxury and industry) emerged and became dominant. Soon, the growth of wealth also brought about its maldistribution. Accordingly, solidarity and association appeared, at the same time as economic progress and evolution were important themes. Entrepreneurs and bankers became recognised as indispensable elements in the capitalist system, as well as traditional classes, such as landowners, capitalists and workers.

However, as governmental roles expanded and the State began to mediate between workers and employers, a new type of economic idea emerged: management in bureaucracy. Until the nineteenth century, poverty, obedience and anonymity were required of British civil servants, based on their moral values and a sense of public spirit. Since the early twentieth century, however, more active roles were required of them, based in part on scientific knowledge. Thus, management founded on economic intelligence became important. Beveridge, for instance, envisioned that Labour Exchanges could artificially match the supply of and the demand for employment. Following this, he was appointed to the Board of Trade in 1909 to oversee the work of the Labour Exchanges. Beveridge, as well as Hawtrey (Treasury), Keynes (India Office), Henderson and Layton (the Board of Trade) and Josiah Stamp (Inland Revenue), embodied the new type of economic idea. They all had plans to facilitate an expanded managed economy and were aware of the advantages and disadvantages of capitalism. As a result, they gravitated towards liberalism.

In this respect, the concept of an Economic General Staff (EGS) is important. An EGS is an expert body working in government which can advise the Cabinet on important and technical economic matters. It was Beveridge (1923/1924) who originated the concept and disseminated it. Borrowing from this, Keynes slightly altered its proposed characteristics and presented it to the prime minister in 1929.³⁸ The Macmillan Committee on Finance and Industry and the Economic Advisory Council were, though unsatisfactory, products of Beveridge's and Keynes's combined efforts. For over twenty years, Beveridge adhered to the notion of those who would work within an EGS as having a dual function, that of public officials and professional economists, like himself. He wrote in 1944 that, 'the central machinery of Government in Britain at last includes an organ capable of expert study of general economic problems, as the basis of orderly foreseeing treatment of them' (Beveridge 1944: 259). For Beveridge, the creation of an EGS was

³⁸The Industrial Situation: Notes by Mr. J.M. Keynes', n.d., TNA, Kew, London: PREM 1/70, P.M.C. 2.

an essential device for cutting through the Gordian knot 'of decrepitude, ineptitude and shortsightedness embodied in...politicians!' (Booth and Pack 1985: 162). The EGS was an organ through which economic expertise could and should be used within the political arena.

5.3 Welfare Ideas

After his debut as an unemployment expert in the 1900s, Beveridge engaged in examining labour problems, including social insurance. In the 1940s, he completed his welfare vision by publishing two trilogies. The first was made up of social security (Beveridge 1942), full employment (Beveridge 1944) and voluntary action (Beveridge 1948), the themes being individuals' political rights, economic rights and social duties, respectively. The second trilogy examined with freedom from want (Beveridge 1942), freedom from idleness or unemployment (Beveridge 1944), and freedom from war or fear of war (Beveridge 1945), with individual security being an overarching theme.

Beveridge's body of work can be better understood when we consider it as a package of visions concerning the evolution of politics, economy and society (Komine 2010). Beveridge's approach made it possible to combine three schools of thoughts in Britain. First, young Beveridge was influenced by British idealism or romanticism at Balliol College, Oxford, where T.H. Green's teachings prevailed. Edward Caird, Master of Balliol, challenged Beveridge to 'discover why, with so much wealth in Britain, there continues to be so much poverty and how poverty can be cured' (Beveridge 1953: 9). Beveridge held on to this idealism throughout his life and was reflected in his advocacy of a 'unified nation' (or a world federation) for peace after the Second World War.³⁹ Beveridge wrote that 'The world today is a graveyard of millions ... But the human spirit does not die. From all these graves some day human kindness will return to humankind' (ibid.: 362). Once Beveridge reached middle age, he was affected by LSE scientism and internationalism, the roots of which could be traced back to Beatrice Webb's methodological approach to the social sciences, and was enhanced by Robbins's academic circle from the late 1920s. Beveridge attempted to establish a unique scientific approach to social phenomena, yet that has been almost completely ignored. The vestiges of his attempts arguably remain around the core of economic science, in the form of appointments to professorships to anthropology,

³⁹As long as I live I shall remain a firm Federal Unionist' (Beveridge to Rea, 19 December 1960, BP, BLPES: XII-63).

social institutions, sociology and others. Finally, the elder Beveridge helped develop a more practical approach to economics in the form of the Cambridge School, particularly concerning Keynesian economics. In the end, Beveridge sympathised with the Cambridge approach, and, in doing so, admitted that discretionary and practical policies are necessary to remove the numerous defects of market failure. After 1938, he recognised that elements of the Cambridge approach could be incorporated into his own system of understanding political, economic and social phenomena. Thus, after thinking about social security, he could consider the issue of full employment.

In this way, Beveridge synthesised the three schools of thoughts. This is also one of the reasons he could, based on observing reality and for the purposes of the future, offer a comprehensive vision of welfare for all in the post-war era. Moreover, Beveridge's ideas on politics, economy and society had a significant impact on subsequent thinkers on welfare at LSE, two of whom turned out to be among the most prominent scholars on the subject.

T.H. Marshall (1893–1981) supported the welfare state in the light of a key concept of citizenship. In a series of lectures in 1949, he pointed to the historical and logical development of three parts of citizenship. First, the civil element is composed of 'the rights necessary for individual freedom' (Marshall 1950: 10), including the right to own property, which developed in the eighteenth century. Next, the political element means 'the right to participate in the exercise of political power' (*ibid.*: 11), which grew in the nineteenth century. Third, the social element covers 'the whole range from the right to a modicum of economic welfare and security to the right to share to the full in the social heritage' (*ibid.*), which emerged in the twentieth century. The welfare state after the Second World War, emerging from a conflict between capitalistic growth and democratic equality, helped to unify not only Britain but also other advanced nations with the universal national minimum (subsistence income) principle. However, after the Golden Age of Capitalism, Marshall came to consider 'the Hyphenated Society' (Marshall 1981: 102, 123), a composite social structure of democratic-welfare-capitalism, in which the three elements sometimes coexisted in a stable manner and sometimes in an unstable manner. Here, welfare ideas can (but often fail to) connect democratic egalitarianism and inequality in capitalism.

Richard Titmuss (1907–1973) identified three types of social policy. First, the residual welfare model presumes that there are two natural channels of welfare supply: the private market and the family. Social policy is (exceptionally) necessary when the two channels are broken. Second, the industrial achievement-performance model states that social needs should be met on the basis of merit, work performance and productivity. Third, the

institutional-redistributive model asserts that universal services are automatically provided through a major integrated institution. At the same time, Titmuss highlighted three types of welfare supply: social welfare, occupational welfare and fiscal welfare (Titmuss 1965: 16). He discussed an overlapping structure of welfare provision, but by distinguishing the social market from the economic market, he defended the institutional-redistributive model.

Beveridge, Marshall and Titmuss shared a similar type of thinking, which can be referred to as 'LSE welfare ideas'. They regarded a continuation of modern capitalism as a matter of course, sought an absolute minimum level of welfare for all citizens, and clarified universal policies applicable at the national level. Additionally, they defended the welfare state and envisioned a welfare society in which the three parts of market, community and State could coexist as independent but well-balanced elements.

6 Conclusion

William Beveridge made significant contributions to the economic discipline in a variety of areas. In Section 3, by identifying his contributions to modern economic analysis, we concluded that Beveridge, based on empirical data and the latest theories on trade cycles, completed a coherent diagnosis of industrial fluctuations and resulting unemployment in modern capitalism. In Section 4, we examined his contributions to professionalisation in economics and found that Beveridge founded an international intellectual group at LSE and exhibited enthusiasm for (albeit unsuccessful) attempts to create a renewed political economy. In Section 5, we studied his contributions to evolving economic ideas, reaching the conclusion that Beveridge served as the driving force to create innovative ideas by his personal exchanges with eminent scholars such as Pigou, Keynes, Robbins and Harrod. In addition, he was the representative of a new type of economic thought: management in bureaucracy. In a sense, he served as a bridge, connecting the three different schools of economics (Oxford idealism, Cambridge practicality, and LSE scientism and internationalism), which could in turn be sublimated into universal welfare ideas.

Beveridge was significant not only in the light of political and social thought, as Harris (1997) explains, but also from the perspective of economic thought. These three perspectives are not mutually exclusive but rather are complementary. The first and second pillars depend on the third (economic ideas), while the third presumes the first and second pillars. Beveridge's ultimate aim was that of human security for all citizens both

nationally and internationally. To realise this, three objectives were needed: peaceful, diligent and affluent society, the opposite of war, freedom from idleness and freedom from want, respectively. Such a broad viewpoint was possible when Beveridge analysed the conditions necessary to smooth market functions, while taking political reality and social rights into serious consideration; it was also possible for the man who deepened exchanges with renowned Cambridge, LSE and Oxford economists and attempted to transform his ideas for professionalisation in economics into reality inside and outside LSE.

Beveridge was recognised as an eminent economist by some of his contemporaries and direct acquaintances. In and of itself, one implication is that we should reconsider the real meaning what it means to be an ‘economist’.

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10

R.H. Tawney (1880–1962)

Noel Thompson

1 Introduction

I do not know what most men feel like when they are wounded. What I felt was that I had been hit by a tremendous iron hammer, swung by a giant of inconceivable strength, and then twisted with a sickening sort of wrench so that my head and back banged on the ground, and my feet struggled as though they did not belong to me. For a second or two my breath wouldn't come. I thought – if that's the right word – “This is death”, and hoped it wouldn't take long (Tawney 1916 [1953]: 18).

As I write this, it is 100 years to the day (1 July 1916) since an NCO advancing across no man's land was struck in the abdomen by a German bullet. The NCO was one among 42,000 wounded who, with the 19,200 killed, represented the greatest number of casualties ever suffered by the British Army in a single day. This was the first day of the Battle of the Somme and the NCO was Sergeant Richard Henry (R.H.) Tawney who survived, but only just, to write an account of the episode in *The Attack*, 1916, which, among other things, gives us one of the best descriptions in the English language of being hit by a bullet, one matched only by that in Orwell's *Homage to Catalonia*. But Tawney's survival also gave us an economic, social, cultural and intellectual historian, political economist, activist and commentator,

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social and political philosopher who came to be one of the most important, left wing, public intellectuals of the twentieth century, revered, unusually, by *all* wings of the broad church that was the labour movement.

2 Early Life¹

R.H. Tawney was born in Calcutta, India, on 30 November 1880. With the family's return to England, Tawney attended Rugby School, 1894–1899, and then Balliol College, Oxford, where he read Greats and, to the marked disappointment of his father, a notable Sanskrit scholar, managed only a Second Class Honours degree before graduating in 1903. This prompted his father to ask him how he was to expunge this academic disgrace and while Tawney's subsequent academic career provided an eloquent response, he was, initially, to leave Oxford for Toynbee Hall, the University's outpost in the East End of London, a move influenced by the ethos of community service characteristic of the Balliol of the period² under the leadership of men such as Benjamin Jowett and Edward Caird but also by his own active sense of Christian duty rooted in a Broad Church Anglicanism.

A three-year period in the slums of the East End led to a close friendship with William Beveridge, whose sister he subsequently married, and then to a post as Secretary of the Children's County Holiday Fund³ which, as its name suggests, aimed to give children a break from the enervating environment of the East End. However, becoming a member of the Workers' Educational Association (WEA) Executive Committee⁴ in 1905 set Tawney's career on a different track, one which involved a sustained engagement with the skilled working class⁵ through the teaching of economics and economic history as a WEA tutor (1908–1914) and, in the longer term, to a lifelong commitment to the cause of adult education⁶ and educational reform more generally. It was this, and a previous short period as an Assistant Lecturer in economics at Glasgow University, which engendered that interest in economics and

¹For a more detailed account of Tawney's early life and career, see in particular Goldman (2013, 2016).

²As well as Rugby School, also noted for its tradition of social activism.

³Established by Samuel Barnett, the Warden of Toynbee Hall.

⁴Established by Albert Mansbridge.

⁵Initially at Rochdale in Lancashire and Longton in the Potteries and then at a number of locations, including Chesterfield and Wrexham.

⁶He was President of the WEA from 1924 to 1944.

economic history which was to inform his academic career and lay the basis for a social philosophy articulated in books, articles, papers and pamphlets over the next fifty years.

In 1912, Tawney was appointed Director of the Ratan Tata Trust, which had been jointly established by a prosperous Indian businessman and the London School of Economics (LSE) to investigate the causes of poverty and the means for its alleviation. It was during Tawney's period as Director that the Trust produced two major studies into the consequences of the minimum wages established by the Trade Boards Act 1909 for the notoriously sweated industries of tailoring and chain making.⁷

3 LSE

Though associated with LSE through the Ratan Tata Trust, Tawney did not join its staff until 1920, when he taught courses on English Economic Developments, 1485–1760, and Social Developments since 1760. He was made a Reader in 1923, became Head of the Department of History in 1926⁸ and was awarded a Chair in Economic History in 1931, and it was as an economic and social historian that he subsequently established his academic reputation.⁹ In all, Tawney taught at LSE for more than thirty years and his intimate and sustained connection with it was such that, as its archivist has put it, when one opens a cupboard in the School 'a bit of Tawney is sure to fall out'.¹⁰ Among the economic historians, Tawney had a close working relationship in particular with Eileen Power and, though he differed from his views in many respects, T.S. Ashton. It would, however, be fair to say that, with the exception of Hugh Dalton and protégés such as Evan Durbin, he did not have such a relationship with the economists and that, politically and intellectually, he had greater sympathy and engagement with the School's political theorists, sociologists and social anthropologists, such as Graham Wallas, Harold Laski, Richard Titmuss and Bronisław Malinowski.

⁷Namely, *The Establishment of Minimum Rates in the Tailoring Industry Under the Trade Boards Act of 1909* and *The Establishment of Minimum Rates in the Chain-making Industry, Under the Trade Boards Act of 1909*.

⁸Until 1929 and again from 1940 to 1945.

⁹He was a founder of the Economic History Society in 1926 and of its journal the *Economic History Review*, which he co-edited between 1927 and 1934. His postgraduate seminar "Economic and social England, 1558–1640" attracted and trained some of the best historians of the future' (Goldman 2016).

¹⁰For this, see Goldman (2013: 4).

In part, this was due to the turn taken by economics at the interwar LSE under the leadership of Lionel Robbins. Appointed as Professor of Economics in 1929, Robbins, having embraced Austrian economics during a period in Vienna in the early 1920s, trumpeted the need in his Inaugural Lecture to address fundamental theoretical problems and apply the methods and earn the respect that attached to the natural sciences (see Durbin 1985: 101). With this seems to have come an intolerance of those who sought to plough a less positivistic methodological furrow, particularly when it led in a leftwards direction. Or, as Dalton put it after a visit to Germany in 1933, “Geistige Gleichschaltung” [Intellectual coordination] is the Nazi ideal in education. There is something of this, too, in the Economics Dept. at the School of Economics’ (Dalton cited by Durbin 1985: 103). Certainly Tawney, among others, ‘commented on the difficulty of discussing politics at the LSE, particularly after open warfare broke out over the acquisition of the Frankfurt Institute’s Library’, which provided ‘a centre of Marxist study’ (ibid.).

4 Methodology

As to methodology, Tawney, in a piece on ‘The Study of Economic History’ published in *Economica* in 1933, wrote that ‘methodological discussions have some resemblance to those Chinese dramas the spectator of which, after listening for five hours, to a succession of curtain-raisers, discovers that the performance is over at the moment when he hoped it was about to begin’ (Tawney 1933 [1978]: 56). Yet while, as such impatience suggests, he was rarely given to methodological disquisitions, Tawney had nonetheless a distinctive approach to history in general and economic history in particular, one that was inherently multi-disciplinary and methodologically eclectic. Thus, ruing what he saw as an increasing tendency to compartmentalise ‘wisdom’ into discrete subject areas, he opined that ‘she appears to prefer the debateable land where titles are ambiguous and boundaries intersect; nor is her business much advanced by what in humbler spheres are known as demarcation disputes ... [T]he best fish are caught when poaching’ (ibid.: 48–49). Again, in a lecture on ‘Social History and Literature’ to the National Book League in 1949, Tawney reflected critically on

the nature of scholastic institutions, not least universities, to proliferate to excess in the artificial entities known in the language of the trade as “subjects”. When we reach years of discretion – which I take to mean the age when youth shows signs of getting over its education – part of our business is to join those naturally connected interests, which the demands of examinations and the exigencies of time-tables have temporarily put asunder (Tawney 1949a [1964]: 196).

Of course, such methodological multi-disciplinarity is now much in vogue and the academic specialisation to which Tawney was reacting, the product of the emergence of the late-nineteenth and early-twentieth-century professionalisation of disciplines, has of late come under increasing critical fire. But at this juncture, Tawney was swimming against the tide and to some contemporaries at least his views smacked of that hubristic eighteenth-century, polymathic ambition which modern scholarship had sought to exorcise.

Moreover, one does not have to read far into historical works such as *Religion and the Rise of Capitalism* (Tawney 1926 [1972]), *The Agrarian Problem in the Sixteenth Century* (Tawney 1912 [1967]), and his magisterial introduction to Thomas Wilson's *A Discourse upon Usury* (Tawney 1925), to realise that Tawney practised what he preached. For these were works that all deployed an understanding of economic theory, sociology, social anthropology, theology, philosophy, political theory and law to encompass the social, economic, political, legal, cultural and intellectual world of the periods on which they were focused. This is not to pronounce upon the merits or demerits of the historical explanations which he propounded, or the strength of the causal links which he forged, or the reality of the historical patterns which he identified, but it is to admire the extraordinary texture, detail and multifaceted character of the history which he wrote.

Almost inevitably this led Tawney to take a critical view of the utilitarian methodological individualism which, rightly or wrongly, he felt underpinned much contemporary economics and vitiated its analysis. Thus, in *The Acquisitive Society*, he opined that

economic science ha[d] never escaped from the peculiar bias received from the dogmatic rationalism which presided at its birth. Man seeks pleasure and shuns pain. He desires riches and hates effort ... But mere crude eighteenth century rationalism still works havoc with the discussion of economic issues ... It is still used as a lazy substitute for observation, and to suggest a simplicity of motive that is quite foreign to the facts ... The truth is that we ought radically to revise the presuppositions as to human motives on which current presentations of economic theory are ordinarily founded (Tawney 1921 [1945]: 198–199).

Human motivation was complex and could be understood only by reference to the structural and subjective forces and factors which shaped it. That in turn required an acute understanding of historical context. As such, economic 'laws' or, more accurately, those economic regularities which were often given the status of laws were historically contingent. So, for Tawney, writing in the second edition of *Equality*, there were

scientific laws which state the invariable relations between phenomena, and there are juristic laws which state how men should conduct themselves, and there are laws which are neither juristic nor, in the full sense, scientific, though they belong, no doubt to the same category as the latter. Such laws neither state invariable relations nor prescribe conduct, but describe how, on the whole, *under given historical and legal conditions and when influenced by particular conventions and ideas, particular groups of men do, as a rule, tend to behave* (Tawney 1952 [1979]: 53; italics added).

To ignore this was to be guilty of a ‘treatment of economic motives...apt to strike a mere child of nature as *simpliste* to excess’ and to formulate theories which, in failing to engage with the world as it was and had been, were devoid of explanatory power (Tawney 1950 [1978]: 210). As Tawney wrote in an essay published in 1918:

[T]here has been no more mischievous habit of thought than the smiling illusion which erected into a philosophy the conception that industry is a mechanism moved by quasi-mechanical laws and adjusted by the play of non-moral forces, in which methods of organization and social relationships are to be determined solely by considerations of economic convenience and productive efficiency. By erecting an artificial barrier between the economic life of society and its religion, its art, the moral traditions and kindly feelings of human beings, that doctrine degrades the former and sterilizes the latter (Tawney 1918 [1964]: 105).

But worse than this, theories premised upon an egotistical individualism not only implied an attenuated view of humankind but were also used to instantiate the necessary nature of the status quo. Such apologists,

like a drunkard who pleads an alcoholic diathesis as an excuse for drinking... appeal to economic laws, the majority of which are merely a description of the manner in which, in a certain environment and in given circumstances, men tend to behave, as a proof that it is impossible for them to alter their behaviour (Tawney 1952 [1979]: 53).

In this regard, a scientific methodological individualism served a conservative function which was anathema to a socialist and social reformer such as Tawney.

So, given the complex and contingent nature of human motivation, the study of history became a *sine qua non* for an understanding of society’s economic trajectory but also its present predicament. This view informed both Tawney’s justification of historical research and in some considerable measure determined his particular focus on the early modern period.

5 History and the Historian

Tawney is usually classified as an economic historian. It is indeed the case that he sought to describe the economic forces which shaped particular historical epochs. But his historical methodology made him much more than this. Thus, in his review of Maurice Dobb's Marxist explication of capitalism in *Studies in the Development of Capitalism*, Tawney wrote of capitalism as 'more than a particular type of economic organization' owing

its existence and character, not merely to the operations of profit-making entrepreneurs, but to a complex of social habits, political institutions, and psychological attitudes in the society about them. Causation, as usual, has worked more than one way. The latter, which are influenced by economic factors, but also which at time move under their own steam, have reacted on the former, as well as the former on the latter (Tawney 1950 [1978]: 214).

This neatly encapsulates his approach to and understanding of that period of British history with which his name is primarily associated, namely the sixteenth and seventeenth centuries. Tawney understood history and historical change in organic, not mechanical, terms. Any explanation of the unfolding of history must be multi- not monocausal, and if that gave the whole an uncertain trajectory, that was something with which historians and socialists had to live. He had therefore little time for the kind of historical materialism that saw economic forces as the driver of a determinate process that took history ineluctably to a communist terminus ad quem: 'I do not share Marx's mid-Victorian conviction of the inevitability of progress; nor do I regard social development as an automatically ascending spiral with Socialism as its climax' (Tawney 1952 [1964]: 178). He would though have been more at ease with Marx's dictum that 'men make their own history, but they do not make it as they please...but under circumstances existing already, given and transmitted from the past' (Marx 1852 [2001]: 7).¹¹

Tawney's historical writing was therefore characterised by an account of the interleaved and indeterminate interaction of multiple forces: the economic, ideological, legal, institutional, political, social and cultural, something particularly manifest in his classic, *Religion and the Rise of Capitalism* (Tawney 1926 [1972]). This is the work that spawned the notion of a Weber-Tawney thesis, namely an explanation of the rise of capitalism that linked it to the theological developments associated with the Reformation.

¹¹Tawney's engagement with Marx and Marxism was consistently, if sympathetically, critical. But as Terrill has rightly said, he was 'unmoved by the brittle Marxism of the thirties' (Terrill 1973: 236).

However, as a number of commentators have made plain, Tawney's work was written before any sustained engagement with Max Weber's *The Protestant Ethic and the Spirit of Capitalism* (see, for example, Winter 1974: 98; Goldman 2013: 229). Moreover, while Tawney applauded the ambition and importance of Weber's attempt to link ideas to the emergence of capitalism—he 'not only threw a brilliant light on the particular field which [he] explored, but suggested a new avenue of approach to a range of problems' (Tawney 1930 [1978]: 190)—he [Weber] had, in Tawney's opinion, nonetheless failed, in his focus on the later phase of Calvinism, to grasp 'the profound changes through which [it had]...passed in the century following the death of Calvin' (Tawney 1937 [1972]: xi). More importantly, Weber's work did not recognise that economic organisation and social structure could shape religious ideas, just as much as the latter could play a part in the evolution of the former (see Tawney 1930 [1978]: 194).

So here again we have Tawney's embrace of the complex, interconnected and indeterminate outcome of historical forces: 'The only adequate history is *l'histoire integrale*, and the limitation of specialisms can be overcome only by a treatment which does justice at once to the economic foundations, the political superstructure and the dynamic of ideas' (Tawney 1933 [1978]: 64). In Tawney's view of things, religious ideas and changes in religious thinking had played a critical role in the emergence of capitalism. But that thinking had been influenced by the nascent capitalism which emerged in the sixteenth and seventeenth centuries. So, 'in the triple reconstruction, political, ecclesiastical and economic, through which England passed between the Armada and the Revolution, every ingredient in the cauldron worked a subtle change in every other. There was action and reaction' (Tawney 1926 [1972]: xii).

This nascent capitalism had swept away the old feudal order and in so doing it had transformed the organisation of economic activity; it had reconfigured social relationships and obligations; it had reset society's moral compass and altered its aspirations. But to sustain themselves, systems need ideological as well as structural support and it was here that Protestantism and in particular Calvinism, *as it evolved*, helped furnish the requisite system of ideas and beliefs. However, as Tawney made clear, the relationship between Protestantism and capitalism was far from unproblematic. Thus, some of the most scathing attacks on the acquisitive ethos of capitalism came from Protestant writers and

the left wing of the Protestant Party who saw in economic individualism but another expression of the laxity and licence which had degraded the purity of religion and who understood by reformation a return to the moral austerity of the primitive Church, no less than to its government and doctrine (ibid.: 146).

For Protestant divines preached a theology that envisaged society not in terms of the self-interested pursuit of gain by its members, but as held together by a system of reciprocal obligation (see *ibid.*: 37). Such views were clearly inimical to the spirit of capitalism. They emphasised the moral mutualism and responsibilities that attached to ownership and economic power and they heaped opprobrium upon those who sought riches in this world rather than salvation of their soul in the next. Calvinists and Lutherans were at one on this: '[A]ll insist[ed] that Christianity has no more deadly foe than the *appetitus divitarum infinitus*, the unbridled indulgence of the acquisitive appetite' (*ibid.*: viii).

However, while 'the traditional teaching of the Church as to social ethics [wa]s binding on men's consciences after the Reformation as it had been before it' (*ibid.*: 163), these ethics, forged in a very different economic environment from that of seventeenth-century England, were becoming increasingly inapplicable to an emerging commercial civilisation:

In an age of impersonal finance, world markets, and a capitalist organisation of industry, its traditional social doctrines had nothing specific to offer, and were merely repeated, when, in order to be effective, they should have been thought out again from the beginning and formulated in new and living terms (*ibid.*: 188).

Instead, with Calvinism, there ultimately emerged a theology which was sympathetic to those virtues considered characteristic of capitalism—thrift, diligence, sobriety and frugality. In effect, 'the good Christian' was crafted as an entity 'not wholly dissimilar from economic man' (*ibid.*: 251). Calvinism's quarrel was not, therefore, with the acquisitive accumulation of riches but with their self-indulgent or ostentatious use (see *ibid.*: 114). This was a theology which, for Tawney, started from an 'acceptance of the realities of commercial practice' in seventeenth-century England. It took account of the associated interests of the financier, the merchant and an increasingly commercially oriented landowning class. Moreover, as it evolved, there occurred a transition to a theology which preached 'the conscientious discharge of the duties of business' as 'the loftiest religious and moral duties' and celebrated the associated moral virtues of an acquisitive and self-oriented economic individualism (*ibid.*: 218, 239). In so doing, a space was created for a secular rather than a religiously oriented political economy, one which treated the economic as a discrete sphere of activity and relegated theology to a commentary on private conduct, 'set[ting] a naturalistic political arithmetic in the place of theology, substitut[ing] the categories of mechanism for those of teleology and turn[ing] religion itself from a master interest of mankind into one department of life with boundaries which it is extravagant to overstep'

(Tawney 1925: 106). This, for Tawney, was how religion was related to the rise of capitalism: a revolution in theological thinking that in canonising the individualistic virtues—foresight and thrift, moderation and self-discipline and rational calculation—and in seeking to accommodate the interests, aspirations and realities of a nascent capitalism, effected ‘the abdication by the Christian churches of one whole department of life, that of social and political conduct, as the sphere of the powers of this world and of them alone’ (Tawney 1921 [1945]: 231).

6 Critique of Capitalism

Like Marx, Tawney recognised that this revolution in thinking had helped to bring into being and supported an economic system that had produced fundamental material advances signally benefiting society. But the costs of economic progress had been considerable and some of these had increased with capitalism’s development. Here, Tawney highlighted a number of things. First, there were the social and economic consequences of the skewed distribution of income and wealth that stemmed from the private ownership of the means of production. Second, there was capitalism’s ethos of acquisitive individualism. Third, there was the resultant subjugation and exploitation of a working population for private gain, a subjugation that was not just material but cultural and spiritual. Fourth, there was the social antagonism that was a product of these three characteristics. Fifth, there was the moral and material damage done to those wielding economic power both by their instrumental engagement with humankind and the corrupting influence of affluence. Finally, there was the threat to democracy posed by the kind of capitalism that was emerging in the twentieth century.

As to its moral ethos, capitalism was a system with material riches as its primary objective and therefore used people ‘not as human *personalities*, but as tools, not as *ends* but as means’ (Winter and Joslin 1972: 13; italics in original). As Tawney put it in his *Commonplace Book*,¹² it was

the ineradicable assumption of the upper classes that the worker should be primarily a good productive tool. He is always judged from this point of view, from the assumption that all he wants, or ought to want, is not to live but to work ... [H]e [the worker] is not regarded as a human being, who wants [to] live, but as so much power that is bought in the market, like electricity or gas (ibid.: 5–6).

¹²The *Commonplace Book* was a private journal of moral, religious, social and political reflections kept by Tawney in the period 1912–1914.

So, for Tawney, even if the material grievances of the workers could be removed and their condition significantly improved ‘since even quite common men have souls, no increase in material wealth will compensate them for arrangements which insult their self-respect and impair their freedom’ (Tawney 1926 [1972]: 278):

The revolt against capitalism [therefore had] its source, not merely in material miseries, but in resentment against an economic system that dehumanizes existence by treating the mass of mankind not as responsible partners in the co-operative enterprise of subduing nature to the service of man, but as instruments to be manipulated for the pecuniary advantage of a minority of property-owners, who themselves, in proportion as their aims are achieved, are too often degraded by the attainment of them (Tawney 1949b [1964]: 145).

Tawney was clear that ‘what produces our social divisions is not mere poverty but the consciousness of a moral wrong, an outrage on what is sacred in man’ (Winter and Joslin, *ibid.*). For,

as far as the rank and file are concerned, the impulse behind the movement has been obstinately and unashamedly ethical. The revolt of ordinary men against Capitalism has had its source neither in its obvious deficiencies as an economic engine, nor in the conviction that it represents a stage of social evolution now outgrown, but in the straightforward hatred of a system that stunts personality and corrupts human relations by permitting the use of man by man as an instrument of pecuniary gain (Tawney 1949b [1964]: 145).

So it was this instrumentalist attitude to labour and the concomitant ethos of possessive individualism, not simply labour’s material impoverishment, which lay at the root of the social antagonism that was intrinsic to capitalism. Moreover, if the acquisition of material riches and the ‘acquisitive spirit’ that ‘permeates the totality of society’ were the drivers and standard of success, then workers too would be engaged in their relentless and combative pursuit: ‘When a society by precept and practice has fostered the doctrine that its foundation is the pursuit of personal pecuniary advantage, it will not be able to appeal to men to forego that advantage when it happens to find the application of the doctrine inconvenient’ (Tawney 1919 [1964]: 46). Further, the economic injustice of ‘payments which are made without any corresponding economic service...prevents different classes of workers from restraining each other’ while ‘uniting them against the common enemy’ (Tawney 1921 [1945]: 167). In these respects, ‘economic oppression and industrial strife [we]re not superficial and transitory incidents of the present industrial

order. They [we]re an expression of its essential nature as fundamental as its mechanical perfection and imposing prizes' (Tawney 1918 [1964]: 113).

Tawney was therefore clear as to the existence of 'a perpetual class struggle' under capitalism; clear too as to the 'class advantages and class disabilities' which were 'the characteristic and ruinous vices of our existing economic system' (Tawney 1952 [1979]: 27, 29). As to these, they manifested themselves partly in the hierarchy of authority which existed in the sphere of production, for 'the truth is that, at the present stage of its history, the economic system is necessarily a power system. It is a hierarchy of authority and those who hold its levers exercise, consciously or unconsciously, a decisive influence on human lives' (ibid.: 230), partly in 'the contrast...between different standards of physical well-being and different opportunities for mental development and civilization' (Tawney 1918 [1964]: 119), partly in the consequent monopoly of the professions and upper echelons of the civil service by those with access to education, and partly by the skewed capacity and 'standards of expenditure' that allowed 'a perpetual misdirection of limited resources to the production or upkeep of costly futilities, when what the nation requires for welfare...[is] more and better food, more and better houses, more and better schools' (Tawney 1952 [1979]: 27).

Economically damaging, socially corrosive and morally corrupting, the existence of capitalism and the class system was also a threat to democracy and the freedoms that the working class had managed to win for itself and the nation, a view that Tawney shared with Strachey's *Contemporary Capitalism* (1956), and for the same reasons.¹³ Thus, he was to write in *Equality* that 'democracy and extreme economic equality form[ed], when combined, an unstable compound' and that it might well be 'the case that democracy and capitalism, which at moments in their youth were allies, cannot live together once both have come of age' (Tawney 1952 [1979]: 30, 193). For in coming of age, capitalism had created powerful economic entities that were 'in effect, extra-territorial economic states, with which few political states dare risk a fall' (ibid.: 163). Democracy was therefore 'unstable as a political system' and would be so 'as long as it remained a political system and nothing more' (Tawney 1949b [1964]: 147). In this regard, what was needed for stability was to extend the ideal of democracy and its egalitarian implications into the economic sphere.

However, Tawney was under no illusions as to the magnitude of the challenge. For the British ruling class was a formidable opponent: '[A]t once as businesslike as Manchester and as gentlemanly as Eton; if its hands can be

¹³On Strachey's concerns, see Thompson (1993: 184–202).

rough as those of Esau, its voice is as mellifluous as that of Jacob' (Tawney 1952 [1979]: 64). They gave capitalism the veneer of a civilised polity. They spoke the language of liberty and opportunity. They trumpeted the opportunities for material self-aggrandisement and gave public expression to its attainment in their plutocratic lifestyle. However, they had 'one set of values...for display', but kept quite another 'for use', 'combin[ing], without conscious insincerity, the moral satisfaction of idealistic principles with the material advantages of realistic practice' (ibid.: 190). This was a class whose egregious articulation of the moral virtues of independence and striving, and veneer of social concern, belied an aggressive determination to realise their material ambitions and maintain their social pre-eminence at whatever cost to those who served them.

That said, if conscious of class, it is important to note that this, for Tawney, was not so much a social division between employers and employed but, in the context of contemporary capitalism, 'between all who do constructive work...and all whose main interest is the preservation of existing proprietary rights' (Tawney 1921 [1945]: 90). It was those who owned without working, who claimed reward without service, who were functionless and parasitic on the economic activity of others who, throughout his intellectual life, came under Tawney's sustained critical fire.

7 Tawney's Social Philosophy and Policy Prescriptions

If capitalism's ethos was greed and a concomitant instrumental conception of humanity, its economic philosophy was that of the market, a philosophy which, as Tawney saw it,

immensely simplifies the problems of social life in complex communities. For it relieves [society] of the necessity of discriminating between different types of economic activity and different sources of wealth, between enterprise and avarice, energy and unscrupulous greed, property which is legitimate and property which is theft...because it treats all economic activities as standing upon the same level (ibid.: 34).

It was the market, or human interaction mediated by the market, that determined what was to be produced, at what price, in what quantities and for whom in a manner that occluded ethical considerations. It embodied and it

gave expression to the virtues of economic freedom and choice, communicating and responded to individual desires but it did so without reference to the good or evil that might ensue.

It was this putatively non-judgemental, amoral, mechanical and autonomous character of the market mechanism that particularly provoked Tawney. As he saw it,

there has been no more mischievous habit of thought than the smiling illusion which erected into a philosophy the conception that industry is a mechanism moved by quasi-mechanical laws and adjusted by the play of non-moral forces, in which methods of organization and social relationships are to be determined solely by considerations of economic convenience and productive efficiency. By erecting an artificial barrier between the economic life of society and its religion, its art, the moral traditions and kindly feelings of human beings that doctrine degrades the former and sterilizes the latter (Tawney 1918 [1964]: 105).

Economic activity should not be seen therefore as a discrete area of human endeavour separate from considerations of justice, equity, freedom, human dignity and social purpose. Society was, at root, a moral organism, not a machine, and the use of scarce resources, the organisation of production and the distribution of income and wealth, necessarily entailed choices of an ethical nature. This should be acknowledged with a consequent determination to imbue economic decision-making and activity with a new social ethics. For Tawney, its principles could be simply stated. First, 'the essence of all morality is this: to believe that every human being is of infinite importance, and therefore that no consideration of expediency can justify the oppression of one by another' (Tawney quoted in Winter and Joslin 1972: 67). Second, 'the purpose of industry *is service*, to supply men with the material means of a good life'. So, for Tawney, a critical difference between the 'acquisitive society' and what he termed a 'Functional Society' was that the former 'honours wealth', the latter 'honours service and therefore the labour of those who provide that service' (Tawney 1921 [1945]: 35; italics added).¹⁴

The system of social ethics whose erosion and ultimate destruction that Tawney had charted in historical works such as *Religion and the Rise of*

¹⁴As Wright has stated: '[F]unction belonged to a vocabulary of service, duty and obligation' (Wright 1987: 59). As to the concept of function, 'it seems likely that Tawney was influenced in this respect by the Guild Socialist circles with which he had associated' (Greenleaf 1988: 454), though as Jackson has made clear, 'function' was a concept variously interpreted on the Left (Jackson 2007: 41).

Capitalism was one whose demise had been dictated by the emergence of a nascent capitalism. It was a system whose principles had become increasingly inapplicable both in an existential and a practical sense as capitalism had developed in the course of the sixteenth and seventeenth centuries. In *Religion and the Rise of Capitalism*, Tawney wrote of the then

natural, and not unreasonable, diffidence of men who were conscious that traditional doctrines of social ethics, with their impracticable distrust of economic motives, belonged to the conditions of a vanished age, but...lacked the creative energy to state them anew, in a form applicable to the needs of a more complex and mobile social order (Tawney 1926 [1972]: 274–275).

It was that attitude of mind which had resulted in the demise of a social philosophy that saw no dichotomy between the logic of economic activity and the imperatives of a theologically rooted moral sense; the contemporary challenge, and it was a formidable one, was the reintegration of these.

Yet present circumstances made both imperative, but also possible, the articulation of a new social ethics or, as Tawney put it, they had created an opportunity 'to restate the practical implications of the social ethics of the Christian faith, in a form sufficiently comprehensive to provide a standard by which to judge the collective actions and institutions of mankind, in the sphere both of international politics and social organization' (ibid.: 18–19). The times were propitious. The Great War had shown the potential of collective action to secure a major transformation in the purpose for which national resources were used. It had also induced a fundamental change in attitude towards the distributive and allocative iniquities that had characterised pre-war capitalism and to an economic and social system that had engendered a conflict of unparalleled magnitude between the major capitalist powers.¹⁵ So 'men who have endured the rigours of war in order to make the world safe for democracy, will find ways of overcoming the social forces and institutions which threaten that cause in times of peace' (Tawney 1918 [1964]: 122). They would also judge economic activity by different standards and, in particular, the extent to which it fulfilled a social purpose rather than merely satisfying the individual appetite for gain. For 'property and economic activity exist to promote the ends of society, whereas hitherto society has been regarded in the world of business as existing to promote them' (Tawney 1921 [1945]: 29).

¹⁵For a more extended discussion of the impact of the Great War on Tawney's thinking, see Winter (1974: 166–173).

Collective action to put economic activity on a different basis, one informed by a new social ethics, should take the form of the public ownership of those enterprises and activity which were of fundamental importance to the nation and also those which most obviously allowed the exaction of a rentier income. Again, contemporary circumstances were favourable to such an undertaking, with capitalism assuming an increasingly monopolistic and oligopolistic form, centralising control and ownership and therefore making easier its transfer into public hands. Thus, as early as 1913, in his *Commonplace Book*, Tawney had written of ‘economic privilege’ that he could ‘not see how that can be attacked except by a large transference of property rights, by the adoption of the principle that economic “rent” is not to be left in private hands’ (Tawney in Winter and Joslin 1972: 52). Later, Tawney had played an important role on the Sankey Commission (1919), which had recommended the nationalisation of the coal industry, an industry which, with its royalties to landowners, epitomised that more general combination of inefficiency and inequity that blighted the lives of the working class and impeded economic progress. He was clear as to the need for the public ownership of such industries and articulated this throughout the interwar and post-1945 periods in works such as *The Acquisitive Society* and *Equality* and in the literature and policy statements which he helped draft for the Labour Party, such as *Labour and the Nation* (1928).

As to the form which the public ownership of industries was to assume, this should be determined not in any dogmatic way but ‘intelligently’ according to their ‘character’ (Tawney 1952 [1979]: 186). Thus “nationalization”...which is sometimes advanced as the only method of extinguishing proprietary rights, is merely one species of a considerable genus’ (Tawney 1921 [1945]: 119). What was needed was

to classify industries by the degree to which they are invested, for one reason or another, with a public significance, and to treat them in accordance, not with any abstract formula but with the realities of their position ... Whether control should take the form of regulation, or of their acquisition by the State and management by a public body, is a question of expediency, to be answered differently in different cases (Tawney 1952 [1979]: 186).

However, whatever the form public ownership assumed, Tawney, throughout his life, emphasised the critical nature of economic democracy to the success of public enterprises.¹⁶ Without that, they would fail in three

¹⁶In terms of Tawney’s desire to extend the principle of democracy into the economic sphere, there is the possible influence of Harold Laski (see Greenleaf 1988: 457). But, as noted, Tawney was also undoubtedly influenced by the ideas of the guild socialists.

respects. They would fail to be imbued with that spirit of social service which was essential if such enterprises were to fulfil their social purpose; they would fail to embody the principle of economic freedom; and they would fail on grounds of efficiency, which Tawney saw as integrally linked to the engagement of the workforce in decision-making.¹⁷

What was needed for the success of public enterprise was a relationship of trust between managers and workforce; there must ‘be a spirit working within it not merely a body of rules imposed by an external authority’ (Tawney 1918 [1964]: 117). Given ‘the evil legacy of suspicion left by capitalism amongst the rank and file of workers’, that represented a major challenge, one that could not be met solely by the legislation which transferred productive capacity from private into public hands (Tawney 1949b [1964]: 163). Indeed,

it may well be the case that a generation must elapse before a cordial partnership between the public bodies responsible for the conduct of a nationalized industry and the employees in them, of a character to ensure that the latter make...their full contribution to the efficiency of the service, can be successfully established (ibid.).

But unless such a co-operative spirit prevailed, the extension of social ownership would fail as a means of realising socialist ideals. The reality of public ownership in the post-1945 period made this clear. The Labour government had achieved much. It had shown that

a Socialist Government, with the public behind it, can change the power relations within the system, can ensure that a larger part of the resources yielded by it are devoted to raising the standard of life of the mass of the population, and can compel those directing it to work on lines which, left to themselves, they would not choose (Tawney 1952 [1964]: 180).

It had acted on its manifesto commitments ‘with remarkable fidelity, and, in my view [Tawney’s], with impressive success’ (ibid.: 179). But ‘the danger of top-heavy bureaucracy and remote control’ within public enterprises remained and ‘effective supervision of these Leviathans by public and Parliament ha[d] hardly yet been established’ (ibid.: 182).

Despite the achievements of the post-war Labour government, there was still much to be done. Thus, writing in the Preface to the 1952 edition

¹⁷Though the Tawney of the *Commonplace Book* had believed that to argue for freedom on grounds of efficiency was ‘to sell the things of God for Gold’ (Tawney in Winter and Joslin 1972: 85).

of *Equality*, Tawney opined that ‘there is no support...for the notion that Britain has been moving in the direction of becoming a more equal society’ (Tawney 1952 [1979]: 20–21). Full employment and sustained economic growth had not ‘acted as an automatic leveller and abolished the political case for equality’ (ibid.: 21). The extension of political rights had not been matched by the extension of those of a social nature. As a consequence, there was a growing ‘tension between political democracy and a social system marked by sharp disparities of circumstance and education’ (ibid.: 34), with the latter being for Tawney a particular concern.

8 Not by Bread Alone¹⁸

However, while Tawney invested considerable intellectual energy in setting out the principles, practice and moral ethos that should characterise a socialist economy and society, he was invariably clear on one thing. Economic progress, and the wealth it brought, was a means to an end and not an end in itself. Throughout his life, Tawney condemned ‘the unreasoning and morbid pursuit of material gain of which the proper name is the sin of avarice, and civil war’ (Tawney 1921 [1945]: 234). Material abundance, of itself, did not bring contentment. Thus, in his *Commonplace Book*, he averred that

a poor society may be a very happy and contented society. A rich society may be a very unhappy and discontented society, because the springs of happiness and contentment are to be found not in the power of man to satisfy wants but in the power of man to regard his position in society and that of his fellows with moral approval or satisfaction (Tawney in Winter and Joslin 1972: 18–19).

Here, Tawney’s position, so characteristic of the Christian socialist tradition of which he was a part,¹⁹ anticipated contemporary research which has indeed shown that beyond a certain level of affluence there is no strong correlation between wealth and happiness.²⁰

¹⁸For earlier socialist writers in this tradition, see Thompson (2015).

¹⁹*The Agrarian Problem* (Tawney 1912 [1967]) was dedicated to William Temple and *Religion and the Rise of Capitalism* (Tawney 1926 [1972]) to Charles Gore, both of whom Tawney had a close personal connection with. The classic study of the Christian socialist tradition in Britain remains Jones (1968).

²⁰For one discussion of the evidence, see Layard (2011). There are also strong parallels here with those sixteenth- and seventeenth-century figures whose condemnation of an emerging society, with the acquisition of material riches as its primary motivation, Tawney had discussed at length in his historical writing.

For Tawney the desire for material things ‘cloud[ed] the soul’ (ibid.: 20). The problem was that ‘the working-class movement’, while it obviously embraced the ideals of social justice and solidarity, was nonetheless ‘apt to desire...not a social order of a different kind, in which money and power will no longer be the criterion of achievement, but a social order of the same kind, in which money and power will be somewhat differently distributed’ (Tawney 1952 [1979]: 40). But dissatisfaction, discontent and unhappiness were ultimately a spiritual and moral disease, not a material one. It resulted from a society where individuals were seen and used as ends not means. It stemmed from the absence of a moral compass that prioritised service over gain. But above all, it derived from a failure to see that fulfilment lay not in material possessions but in a realisation of the potentialities of the human soul. Like Matthew Arnold, Tawney believed that ‘perfection that consist[ed] in becoming something rather than having something’ (Arnold 1869 [1983]: 9). For

if the Kingdom of Heaven is not eating and drinking, but righteousness and peace, neither is civilization the multiplication of motor-cars and cinemas, or of any of the other innumerable devices by which men accumulate means of ever-increasing intricacy to the attainment of ends which are not worth of the attaining (Tawney 1952 [1979]: 82).

Tawney, like Keynes, and in words remarkably similar to the latter’s ‘Economic Possibilities for our Grandchildren’ (Keynes 1930 [1972]: 321–332), believed that ‘when three or four hundred years hence mankind looks back on the absurd preoccupation of our age with economic issues, [it would do so] with the same wonder as, and juster contempt than, we look back on the theological discussion of the middle ages’. Then, ‘the names which they [would] reverence will be those of men who stood out against the prevalent fallacy that the most important problems were economic problems, and who taught men to conquer poverty by despising riches’ (Tawney in Winter and Joslin 1972: 62).

This was one of the reasons why, for Tawney, education was fundamental.²¹ Of course, it was a social solvent. Its extension was a means of instilling those skills and capacities that made for social mobility and fulfilling working lives. It was the *sine qua non* of a meaningful realisation of the principle

²¹For a more extended discussion of the place of education in Tawney’s life and social philosophy, see Goldman (2013: 199–216).

of equality of opportunity and the fact that ‘differences of educational opportunity amongst children should depend on differences of wealth amongst parents [was] a barbarity’ that had to be eliminated (Tawney 1952 [1979]: 145). To effect this, in addition to his activity with the Workers’ Educational Association, Tawney devoted a considerable part of his working life to advancing educational reform. But it was the case too that Tawney saw in education the means of effecting that change in an individual’s priorities and perception of the world necessary to privilege a pursuit of spiritual self-realisation over a morally enervating materialism.

9 Conclusion

There are those, most recently Armstrong and Gray (2011) and Goldman (2013), who have seen essential discontinuities in the evolution of Tawney’s thinking and who have sought to avoid ‘the error of what Quentin Skinner has called the “mythology of coherence”’, an error committed by previous writers on Tawney such as Greenleaf, Terrill and Wright (see Armstrong and Gray, *ibid.*: 9). So, for Armstrong and Gray, ‘Tawney deliberately moved from [the] undeveloped Christian exclusivity’ of the *Commonplace Book*, ‘to a developed secular alignment’, characterised by ‘its explanatory, not its rhetorical, force’ (*ibid.*: 25). In short, they see Tawney’s thinking as evolving from the desire for an ethical renewal of society rooted in Christianity, to a more secular and political conception of the drivers and objectives of social transformation. Thus, we have ‘the gradual decline of the importance of religion in Tawney’s political thought...paralleled by the gradual rise in the importance of secular politics’ (*ibid.*: 187). Similarly, Goldman has seen the Tawney of the post-Great War period as ‘embrac[ing] socialism’ as the application of state-led utilitarianism, rather than Christian morality’ (Goldman 2013: 123). There is therefore a dualism in Tawney’s thought, a

contradiction between his earliest ideas about socialism which are premised on the individual and concern matters of personal behaviour and belief pre-eminently and his later ideas which are far more conventionally focused on the actions of a reforming state to socialize capital, redistribute income and wealth, and create, by a process of management and direction, the infrastructure for a socialist society (*ibid.*: 167).

However, these notions of dualism and discontinuity significantly underplay the holistic character of Tawney’s thinking. Of course, it would be unwise

to see Tawney's political economy as a seamless whole. Nonetheless, there is no contradiction between the desire of the *Commonplace Book* for a moral transformation of society and its subsequent articulation in practical form. For Tawney, the two were imbricated: the construction and effective functioning of a socialist economy would only be practicable and sustainable if society embraced those Christian moral values which anathematised the materialism and possessive individualism that characterised capitalism.²² Indeed, without the triumph of such values, there could not be socialism worthy of the name. In that regard, while Tawney may have come to embrace some of the prescriptive aspects of Fabian socialism, and certainly its ideal of service, this did not extend to its more mechanistic and amoral conception of social transformation. For, as Terrill has argued, 'if it is not certain that he [Tawney] derived his socialism wholly from Christianity, he located it within Christianity' (Terrill 1973: 246). Moreover, while Goldman might argue that 'the insistence on achieving economic and material equality' in a later work such as *Equality*, 'is...strikingly different from the overt anti-materialism of the *Commonplace Book*' (Goldman *ibid.*: 194), Tawney never ceased to condemn the enervating and corrupting materialism that characterised twentieth-century society. Indeed, he never ceased to be concerned that 'the working class would, after all, settle for comfort rather than freedom' (Wright 1987: 141), a concern which was voiced too in an impressively perceptive account of the historical evolution of the trade union movement in the USA (see Tawney 1942 [1979]). Perhaps Wright goes too far in arguing for the 'seamlessness and unity of Tawney's arguments' (Wright, *ibid.*: 75). But this more nearly captures the essential holism of Tawney's thinking.

As to Tawney's legacy, all elements of the Left, or almost all, have applauded the man and his aspirations. Gaitskell and Crossman, Crosland and the New Left, Shirley Williams and Tony Benn: all have bent the knee.²³ In this regard, his importance for the thinking of the twentieth-century Labour Party has been duly, and rightly, acknowledged.²⁴ But due obeisance

²²Tawney found persistently for high-minded austerity against a false evaluation of material welfare' (Dennis and Halsey 1988: 219).

²³He is perhaps the only man who can be saluted by Fabians, Marxists, Guild Socialists, trade unionists, co-operators and Christian Socialists alike' (Dennis and Halsey 1988: 251).

²⁴To take just one example from a recent study of 'equality' as a concept in the thinking of the British Left: 'Tawney's work on equality...exert[ed] a substantial influence on revisionist thought, both directly and in its dissemination into the Labour Party's conventional wisdom' (Jackson 2007: 168).

done, Tawney is apologetically categorised as someone whose thought, while it might still engender a warm nostalgic glow among some on the Left, is not immediately relevant to our present discontents. As Goldman has it, while he was 'drawn naturally to the analysis of Tawney's ideas as a first priority...as their relevance fades it is his life itself which contains the most important lessons and the elements of enduring relevance' (Goldman 2013: 320). Similarly, if more brutally, Macintyre damned Tawney for his 'curiously antique air', ' cliché-ridden high-mindedness' and 'banal earnestness', an assessment delivered in the course of a misanthropic review of Tawney's *The Radical Tradition* and which, in seeking to deface many of the so-called self-images of the age, managed to denigrate the achievements of the post-war Labour governments and associate Tawney with something that was 'merely an alternative Conservative Party' (MacIntyre 1971: 39, 41).

Such views as to the 'antique air' of Tawney's political economy and social thinking warrant a more extensive challenge than present space permits, but a number of points can be made. First, there is Tawney's view that beyond a certain point a rise in material affluence will not be correlated with an increase in happiness, a point that has recently generated both a considerable secondary literature and substantial empirical support.²⁵

Second, there is his sustained concern with the tension between capitalism and democracy, one that has a particular relevance to many aspects of contemporary capitalism, not least the political and economic power of transnational corporations and the concentration of media ownership.

Third, we have the concern expressed in *Equality* as to 'the contrast not only between different standards of physical well-being' but also between 'different opportunities for mental development and civilization' (Tawney 1952 [1979]: 74), a concern the contemporary relevance of which is consistently confirmed by empirical studies that evidence disparities in the health, longevity, educational attainment and life chances of different social groupings in *twenty-first-century Britain*.

Fourth, there is the continued dominance of political life, the professions and the upper echelons of the civil service by those with access to a public school and Oxbridge education. In modern parlance, it is the posh boys that still call the shots. Or, as Tawney had it, Britain had 'the oldest and toughest plutocracy in the world', one consisting of 'agreeable, astute, forcible, self-confident and, when hard-pressed, unscrupulous people, who know pretty well which side their bread is buttered, and intend that the supply of butter will not run short' (Tawney 1934 [1971]: 64).

²⁵For a short review of some of this literature, see Thompson (2015: 174–178).

Fifth, we have Tawney's condemnation of the demeaning nature of much contemporary labour²⁶ and what he saw as the treatment of workers as ends rather than means, as hands rather than human beings, a condemnation which, with the increasing reliance on zero-hours contracts in the UK and the recent revelations as to the employment practices of Sports Direct, also in the UK, has considerable contemporary resonance.

Sixth, the Christian and ethical socialist critique of existing economic and social arrangements has been sustained into the twenty-first century by writers such as David Marquand, whose *Mammon's Kingdom* (2014), is firmly, and avowedly, in the Tawneyite tradition.²⁷

Finally, and more recently still, we have Deidre McCloskey's *Bourgeois Equality: How Ideas, Not Capital or Institutions, Enriched the World* (2016), which in a manifestly Tawneyite vein has explained the Industrial Revolution as an ethically powered phenomenon. Tawney would have warmed to such an explanation, even if wholeheartedly rejecting McCloskey's celebration of the ethics that did the driving.

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²⁶The influences here were those of Ruskin and, more often acknowledged by Tawney, of William Morris.

²⁷The socialist tradition is best approached through the economic historian and cultural critic R.H. Tawney' (Marquand 2014: 204). For another example, see Sandel (2012: 202–203; italics added) who sees 'the era of market triumphalism' as having 'coincided with a time when *public discourse has been largely empty of moral and spiritual substance*', a remark which could have come straight from Tawney.

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11

Hugh Dalton (1887–1962)

John E. King

1 Introduction

In March 2015, the *Economic Journal* celebrated its 125th anniversary with a special issue containing ten of the best papers that it had ever published, together with critical appraisals by modern authorities. The first of the ten chosen articles was Hugh Dalton's 'The Measurement of the Inequality of Incomes', with an appreciation by Tony Atkinson and Andrea Brandolini (Dalton 1920a; Atkinson and Brandolini 2015). Dalton was in very good company: among the other authors so honoured were Frank Ramsey, Roy Harrod and Gary Becker. But Dalton is mostly remembered today (if at all) as a socialist politician who sat in the House of Commons for several decades, served in the Labour governments of 1929–1931 and 1945–1951, and ended up in the House of Lords as Lord Dalton of Forest and Frith in the County Palatine of Durham.

Dalton's academic career at the London School of Economics (LSE) lasted only 16 years, from 1919 to 1935, and due to his political commitments, he was for several of those years on leave of absence or employed part-time. Yet he made a significant contribution to the interwar economics literature,

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not only on the theory of distribution but also on public finance and the economics of socialism. His fundamentally pre-Keynesian ideas owed more to A.C. Pigou than to anyone else and reveal the substantial scope that neo-classical Cambridge economics offered for the development of a surprisingly radical Pigovian brand of democratic socialism.

2 Call Back Yesterday: Hugh Dalton, 1887–1962

Edward Hugh John Neale Dalton was born near Neath, Glamorgan, on 26 August 1887, the descendant of Welsh landowners on his mother's side and Anglican clergy on his father's side. His father, Canon John Neale Dalton, was tutor to two royal princes, one of them the subsequent King George V, and Hugh grew up in The Cloisters at Windsor Castle. He was educated at Eton and King's College, Cambridge, where he studied first mathematics and then economics, in which he earned an Upper Second-Class degree in 1910, narrowly missing a First. Dalton was taught by A.C. Pigou and by John Maynard Keynes, only four years his senior, with whom he enjoyed a somewhat uneasy social relationship. Robert Skidelsky notes that he was always known as 'Daddy Dalton' in Keynes's circle. There was some respect but little affection between the two men, and by 1945 'mutual antipathy kept them apart: Keynes used to call Dalton "the Dirty Doctor"' (Skidelsky 2004: 646, 957).

Dalton moved to London after graduating and read for the bar at Middle Temple, to which he was called in 1914. He began also to work on a doctorate at LSE on the personal distribution of income. Here, Dalton was supervised by Edwin Cannan, who was a strong influence on him, at least if his contribution to the Cannan Festschrift can be believed:

I have the notes of a course of his which I attended in 1911. It was very largely a running commentary on Marshall's *Principles*. It was full of humour and elements of positive construction. But he wasted no time on unessentials [sic]. Chapters which he thought unimportant were quickly dismissed with such summaries as "nothing much there!" or "nothing much in that!" Having only just come down from the Cambridge of that day, such iconoclasm took me by surprise. But to suffer mental shock is a vital element in education (Dalton 1927: 14, fn. 3).

In terms of content, Dalton was impressed by Cannan's work on the theory of distribution:

He found a confused discussion proceeding about distribution between factors of production and he not only straightened it out, but laid the foundations of that much more interesting and directly important branch of the subject, distribution between human beings. These were revolutionary changes in exposition and emphasis, though they seem obvious today (*ibid.*: 16).

One important result of this intellectual revolution, Dalton continued, was the light which it redirected, after nearly fifty years of academic neglect in this country, to the fundamental institution of inherited wealth, which was exhibited as the chief cause, under existing arrangements, of the inequality of incomes. More comprehensively still it brought the whole institution of property back into the academic limelight and focused attention upon social institutions generally, their economic effects and their essential variability (*ibid.*: 16–17).

Here, Dalton identifies Cannan, and by implication also himself, as an institutional economist. This has significant political consequences, he suggests, since Cannan maintains that ‘no natural harmony exists between individual self-interest and the common good, but only a partial and limited harmony, dependent on the deliberate creation of appropriate institutions’ (*ibid.*: 17). This doctrine, Dalton argues, ‘may be found to justify sweeping changes in existing institutions’ (*ibid.*: 18), and it also explains Cannan’s attitude towards socialism, which was broadly sympathetic but by no means uncritical (see *ibid.*: 18–23).

After war service in France and Italy, Dalton returned to LSE, where he completed his doctorate and, after working briefly as a teaching assistant, was appointed Sir Ernest Cassel Reader in Economics in 1920 (the title pages of some of his books describe him instead as ‘Reader in Commerce’). His former student Lionel Robbins (later a colleague) found him to be an excellent teacher, generous with his time to the young, if not always so attentive to the needs of his contemporaries (see Robbins 1971: 75–78). But Dalton was already active in the Labour Party and successfully juggled his academic and political careers until he resigned from LSE in 1935. His political career, which occupied the entire second half of his life, is documented at great length in the biography by Ben Pimlott (1985), summarised in Pimlott (2004); see also Durbin (1985). Dalton was a Member of Parliament (MP) between 1924 and 1931 and again between 1935 and 1959, representing Peckham (briefly) and Bishop Auckland. In 1960, he became a Life Peer.

Dalton was a member of Labour’s parliamentary executive and its National Executive Committee for three decades. Most of this was spent

in opposition, but he served as Under-Secretary for Foreign Affairs in the Labour government of 1929–1931 and as Minister of Economic Warfare (1940–1942) and President of the Board of Trade (1942–1945) in the Churchill coalition government during the Second World War. After Labour's victory in 1945, Dalton's appointment as Foreign Secretary was vetoed by King George VI in 'the most important exercise of the royal prerogative of the age' (Leventhal 1988: 422; see also Pimlott 1985: 414–417) for an examination of why the monarch intervened). Dalton's stormy period as Chancellor of the Exchequer (1945–1947) came to an end as a result of a budget leak, apparently much to his relief, and he served happily in lesser roles until the end of the Attlee government, first as Chancellor of the Duchy of Lancaster (1948–1950) and then as Minister of Town and Country Planning (1950–1951) and Minister of Local Government and Planning (1951). After 1951, Dalton remained active in opposition to the Conservative governments of Churchill, Anthony Eden and Harold Macmillan, before he retired as an MP in 1959 and accepted a life peerage in the following year. Hugh Dalton died in London on 13 February 1962.

As Lionel Robbins's tribute suggests, Dalton won the loyalty and respect of young men, including what has been described as 'the Dalton kindergarten', a group of talented young disciples that included Evan Durbin, Hugh Gaitskell and Douglas Jay (see Addison 1977: 49). Others found him rather less attractive. While acknowledging his abilities and noting Arthur Henderson's belief that he was a potential future leader of the Labour Party, Beatrice Webb was (in 1927) unimpressed by Dalton's character and personality:

But he has no personal magnetism and though an intellectual and moral man, he has neither intellectual nor moral uniqueness nor distinction. And in his curiously deferential and ingratiating method of address with persons who are likely to be useful to him, there is just a hint of insincerity; in his colourless face there is a trace of cunning. Is his faith in Socialism genuine and likely to endure? (Webb 1956: 145).

In conservative circles, Dalton was seen rather differently, as 'a renegade from the upper classes...[who] retained from his early years an innate sense of superiority, a ruling mentality displaced into left-wing politics. Bursting with ambition, he loved the rough and tumble of politics, intriguing noisily, clumsily, and without a blush' (Addison 1977: 49).

The crucial word was 'noisily', as this wartime anecdote suggests: 'Possessed of a booming voice, Dalton was once heard shouting in an ante-room when Churchill came in. "Who's that shouting?" he demanded.

“It’s Dalton. He’s speaking to Edinburgh”. “Why on earth doesn’t he use the telephone?” said Churchill’ (Skidelsky 2004: 957).

Dalton’s work in economics was no less powerfully self-confident.

3 The Distribution of Income and Wealth

Dalton’s celebrated article was concerned with the relationship between inequality in income and inequality in economic welfare. It began by modifying Bernoulli’s hypothesis, according to which ‘proportionate additions to income, in excess of that required for “bare” subsistence, make equal additions to economic welfare’ (Dalton 1920a: 350) to allow for the effects of the diminishing marginal utility of income. This permitted Dalton to conclude with great confidence that economic welfare would be improved by ‘transfers from rich to poor...a proposition that has excited the interest of “modern” public finance theorists of the neo-utilitarian school’ (Fishburn and Willig quoted in Peacock 1987: 360). But Dalton concluded that the use of the modified formula required much more accurate statistics on the existing distribution of income than were then available, and so he devoted the remainder of the article to other ways of measuring inequality that were less demanding. Drawing on his extensive knowledge of the Italian literature, Dalton introduced the Gini coefficient to the English-speaking world and also assessed the rival merits of seven additional measures of inequality. His work drew immediate praise from Corrado Gini (1921) and, somewhat later, a more critical appraisal by Dwight Yntema (1933).

Apart from these and a few additional references, Dalton’s article was largely ignored for half a century. Then, after 1970, its merits began to be appreciated, with no less than 1100 citations down to 2014 (according to Google Scholar), 484 of them coming between 2000 and 2009 (Atkinson and Brandolini 2015: 211). The question of a ‘Dalton-improving tax reform’ began to appear in the titles of journal articles (see Mayshar and Yitzhaki 1995; Yitzhaki and Lewis 1996). Thus ‘Dalton’s paper was ahead of its time ... It took half a century of substantial achievements in neighbouring fields, such as social choice theory and the theory of decision under risk, before his seminal contribution could grow into a fertile research field’ (Atkinson and Brandolini 2015: 212, 218). The paper had been completed too late for inclusion in the first edition of his book, *Some Aspects of the Inequality of Incomes in Modern Communities* (Dalton 1920b), but it did appear as an appendix to the 1925 reprint (presumably to save the publisher expense, it was placed after the index, paginated 1–16, and not included

in the table of contents). The book itself sold well, and there were further reprints in 1929 and 1935.

The 353-page volume is divided into four parts. In the brief Part I, ‘Some Ethical Aspects of the Inequality of Incomes’, Dalton reveals himself to be an uncompromising utilitarian, with no great interest in theories of economic justice; instead, he endorses ‘Mill’s saying that justice is included within the sphere of social utility’ (Dalton 1920b: 27). He also has no qualms about making interpersonal comparisons of utility:

Put broadly, and in the language of common sense, the case against large inequalities of income is that the less urgent needs of the rich are satisfied, while the more urgent needs of the poor are left unsatisfied. The rich are more than amply fed, while the poor go hungry. This is merely an application of the economists’ law of diminishing marginal utility ... An unequal distribution of a given amount of purchasing power among a given number of people is, therefore, likely to be a wasteful distribution from the point of view of economic welfare, and the more unequal the distribution, the greater the waste. Up to a certain point, the more equal the distribution, the further a given amount is likely to go in satisfying economic needs, and hence in increasing economic welfare (ibid.: 10).

Dalton does not advocate complete equality of incomes—‘crude equalitarianism’ (ibid.: 21)—as this would have serious disincentive effects and greatly reduce economic welfare. But he is quite certain that ‘a large reduction in the existing inequality could be made, which would result in bringing us considerably nearer to the ideal’ (ibid.: 11).

The much longer Part II deals with the treatment of income distribution in the history of economic thought in seven periods, from the pre-1776 era to the eight years 1911–1918. It reveals Dalton’s highly critical attitude towards Marx: ‘It cannot be maintained that he made any large and valid contributions to economic theory. If no better arguments for Socialism could be found than those contained in his pages, it would indeed be a lost cause’ (ibid.: 83).

Dalton is much more favourably inclined towards Mill, whose work is discussed at length ((ibid.: 60–74), and to whom there are 33 references in the name index), Marshall (ibid.: 106–111 and 36 index references) and Pigou (ibid.: 146–151 and 265–267 and 39 index references). Dalton is especially impressed by Pigou’s *Wealth and Welfare* (Pigou 1912), which he describes as ‘the most important book of the last few years before the war ... [A] very powerful book’ (Dalton 1920b: 146, 149).

Part III is devoted to the functional distribution of income. Dalton’s treatment of the relative shares of labour and capital is resolutely neoclassical,

but it has a curiously old-fashioned air. Written some years before the path-breaking work of Cobb and Douglas (1928), it makes no use of an aggregate production function or the concept of the elasticity of substitution. Instead, the analysis is conducted in terms of the relative elasticities of supply and demand of the two factors of production, with Dalton flexing his mathematical muscles in the process (see Dalton 1920b: 187, fn. 1, 188, fn. 3, 189, fns. 1 and 2, 194–197).

This leads to the core of the book, Part IV, ‘The Division of Incomes between Persons’, which is, as we have seen, Dalton’s principal interest. He distinguishes four sources of income: labour, property, gifts and ‘civil rights’, by which he means welfare benefits. In the chapter on ‘Incomes from Work’, there is an extended discussion of the restrictions on human investment that are due to low income and which he sees as the major cause of inequality in the acquisition of skills, not least in the case of women (*ibid.*: 252–270). Most of Part IV, however, is devoted to inequality in income from property, which had received ‘very little direct attention from economists’ (*ibid.*: 271). There are three sources of such inequality: different saving rates, differences in capital gains and differences in inheritances and gifts, the latter being ‘considerably the most important’ (*ibid.*: 272).

Thus, the question of inherited wealth takes up the next six chapters (*ibid.*: 281–345). Again, Dalton notes, this is an issue that ‘has been very much neglected, especially by professional economists’ (*ibid.*: 283). Its significance is not restricted to property incomes: ‘[T]he effects of inherited property in maintaining the inequality of incomes from work are also very great, since the children of those who inherit property inherit better economic opportunities, in the form of better chances than they might otherwise have had, of health, education and comfort’ (*ibid.*: 281). Dalton now puts his legal training to good use, with an extended discussion of the law of inheritance, both fiscal and non-fiscal (*ibid.*: Chapters VI–VIII, 287–310).

His policy proposals are wide-ranging and radical. First, there should be a substantial increase in welfare payments, including an increase in the old-age pension and a new benefit for widows with young children. Dalton also comes close to endorsing the ‘still ampler scheme’ proposed by Dennis Milner (Milner and Milner 1918) ‘who suggests that a “pool” should be formed by a general levy of twenty per cent on all incomes, and that out of this pool 9s. a week should be paid to every member of the community, children included’. ‘This idea’, which is better known today as ‘Basic Income’ (Van Parijs 1995), ‘has the merit of simplicity, if no other’ (Dalton 1920b: 250).

Second, Dalton calls for ‘better and more equally distributed facilities for education and training’ in order ‘to increase the ability of the great majority of workers, at the beginning of their working lives, to make a freer choice than at present between different occupations’, thereby reducing wage differentials (ibid.: 264). This would significantly increase ‘what has been called “vertical mobility”, in the sense of the mobility of workers from the worse paid to the better paid occupations’ (ibid.: 267). Legal and customary restrictions on the employment of women need also to be removed (see ibid.: 269).

Third, to reduce inequality in incomes from property, Dalton’s focus is overwhelmingly on the reform of the laws of inheritance. He expresses sympathy for Mill’s proposal to ‘limit the amount which any one individual may receive by inheritance or gift’ (ibid.: 298; cf. ibid.: 336) but suggests that this is best achieved through taxation: ‘Nearly all modern communities have inheritance taxes’ (ibid.: 312), he notes, and he would certainly have been dismayed by their disappearance in the late twentieth century in nations such as Australia, Canada and New Zealand (see Duff 2005).

Dalton’s preferred form of inheritance taxation was taken from the Italian theorist Eugenio Rignano (1901): ‘This principle is that of an inheritance tax, which shall be “progressive in time”, or, in other words, such that the rate of tax shall increase with the number of times that the property subject to it has already changed hands through inheritance’ (Dalton 1920b: 316; see also Dalton 1921a; Erreygers and Di Bartolomeo 2007). The Rignano tax ‘would do more than any other to keep sharp the stimuli to work and saving among the wealthy’ (Dalton 1920b: 340). It should be applied only to that part of the estate, which the deceased had himself acquired by inheritance or gift, and should be progressively graduated according to the size of the taxable estate. The net estate remaining after the deduction of this tax should be subject to a second tax based on the amounts of individual inheritances. This tax also should be progressively graduated, and the graduation should be so arranged as to take a hundred per cent of the inheritance above a certain amount, thus fixing a maximum individual inheritance, as proposed by Mill (ibid.). These inheritance taxes would not, however, remove the need for ‘a steeply progressive income tax’ (ibid.: 346).

At the very end of the book, Dalton raises the question of whether income inequality is increasing or decreasing ‘in modern communities’, which he describes as ‘one of the most important questions in economics’, but one that could not be answered from the very limited statistical sources that were available in 1920: ‘Conclusions pretending to much generality on

this subject are to be mistrusted’ (ibid.: 351). Dalton would have been fascinated by Thomas Piketty’s *Capital in the Twenty-First Century* and greatly encouraged by the huge critical literature that it has stimulated (see Piketty 2014; King 2017).

3.1 *Principles of Public Finance*

Towards the end of his first book, Dalton had foreshadowed the second, reminding his readers that ‘[t]he whole field of Public Finance is relevant’ to the inequality of incomes (Dalton 1920b: 346). His *Principles of Public Finance* (Dalton 1922a) soon became the leading text in the field, at least in the UK. It went into several editions, the 1929 fifth edition (the one that is cited here) containing substantial revisions and enlargements. A German translation (by Hans Neisser) appeared in 1926, and a summary of the implications for taxation policy was later provided for the general reader (in English) in Dalton (1935a). The last edition of *Principles of Public Finance* appeared as late as 1954, with five new chapters and extensive revisions. It was favourably reviewed by Ursula Hicks:

First published in 1922, the book was a pioneer achievement in breaking from the narrow Victorian outlook on the subject which emphasized the tax side almost to the exclusion of everything else. Dalton’s was the first book in the country to put public finance in the wider background which is now universally acknowledged to be its right, and to give due weight to the expenditure side of the budget. Moreover the book is simply and clearly written and eminently readable (Hicks 1955: 360).

It was ‘still selling 4000 copies a year in 1953’ (Durbin 1985: 36).

As Hicks noted, *Principles of Public Finance* was much more than a book on the economics of taxation, although the twelve chapters of Part II, ‘Public Income (mainly Taxation)’, do account for more than half of the 291 pages of text. They are preceded by a brief Part I (‘Introductory’) and followed by five chapters on ‘Public Expenditure’ (Part III) and a further five chapters on ‘Public Debts’ (Part IV). At least in the 1929 edition that I have used, the focus is overwhelmingly microeconomic, though some very interesting macroeconomic questions are raised in Parts III and IV. Moreover, it is overwhelmingly neoclassical in its theoretical orientation, drawing heavily on the work of Cannan and (especially) Pigou.

Dalton sets out the principles of 'a good tax system' with admirable clarity:

It is best to rely on a few substantial taxes for the bulk of the tax revenue. In so far as it is desired to tax the rich, incomes and inheritance taxes are the best means; in so far as it is desired to tax the poor, taxes on a few commodities of wide consumption, preferably commodities not necessary to health and efficiency. Some commodities, of which alcohol is the most important example, may indeed be taxed on their merits, or, as some would say, on their demerits, apart from any question of the distribution of the burden of taxation between different sections of the community (Dalton 1922a: 46).

He provides an extended account of the implications of the principles of 'equal sacrifice' and 'proportional sacrifice' for the way in which the burden of taxation is distributed. Both principles imply progressive taxation and both are subject to the general rule that 'the more rapidly the marginal utility of income is assumed to diminish with increasing income, the more steeply progressive must the tax system become' (ibid.: 92). The lengthy Chapters X and XI deal with the effects of taxation on production (ibid.: 103–128) and distribution (ibid.: 129–152). Here, Dalton discusses the respective merits of income taxation and taxes on inheritance (ibid.: 113–125), expenditure (ibid.: 135) and wealth (ibid.: 135–138). He concludes by briefly considering two other sources of government revenue: income from public property and public enterprises, and 'income from the printing press', which he does not favour. 'On balance', he maintains, 'inflation is likely to increase the inequality of incomes and operates, in effect, not as a proportional, but as a regressive tax', which is a sufficient (though possibly not a necessary) reason for rejecting it (ibid.: 183).

This is the first mention of macroeconomics, though a similar point had been made in Dalton's earlier book, where he advocated deflating the price level on similar distributional grounds (Dalton 1920b: 347, 349). Most of Part III deals with the microeconomic aspects of increased public expenditure, which he endorses on the grounds that, sensibly managed, it will tend both to increase production and to improve the distribution of income. Dalton had already, in the introductory section, noted a fundamental difference in the principles governing private and public expenditure. Individuals always discount the future, probably to an excessive extent:

But, since the community outlasts the individual, and since...the statesman should regard himself as a trustee for the future, the latter is not entitled to

discount the future at so high a rate as most individuals in their private capacity normally do. Indeed, it is doubtful whether he is entitled to discount it at all (ibid.: 20–21).

For Dalton, this was a powerful argument in favour of public investment.

Towards the end of this third section of the book, Dalton returns to macroeconomic issues. He advocates countercyclical government spending to increase employment in times of depression, which ‘may both increase the economic welfare of those directly affected and may result in the more effective use of the community’s productive power, a large part of which, in the shape of labour, capital and organising power, not merely lies idle, but deteriorates during periods of trade depression’ (ibid.: 228). In the fourth and final part of the volume, which deals with public debt, Dalton takes this proto-Keynesian argument further, anticipating one aspect of Abba Lerner’s principle of ‘functional finance’: ‘[A]ll transactions connected with an internal debt resolve themselves into a series of transfers of wealth within the community. It follows that there can never be any direct money burden, or direct money benefit, of an internal debt. For all the money payments cancel out’ (ibid.: 247; cf. Lerner 1944: 302–303).

However, Dalton’s subsequent discussion (Dalton 1920b: 247–252) does not culminate in a Lerner-style denunciation of ‘sound finance’ and the application of ‘Swabian housewife logic’ to fiscal policy (on which, see King 2015). He is concerned more with the clearly adverse distributional implications of increased public debt than with any possible beneficial macroeconomic consequences.

In 1923, Dalton published a 94-page pamphlet advocating the Labour Party’s policy for a ‘capital levy’ to pay off a large part of British government debt. The debt had grown explosively during the First World War, amounting in 1922/1923 to some £7.8 billion, of which £1.09 billion was owed to the USA with the remainder constituting internal debt. Annual interest payments on the debt, at roughly £300 million, were absorbing no less than one-third of all government revenue. ‘The object of the Capital Levy’, Dalton explains, ‘is to pay off quickly, by a special emergency effort, a large proportion of the War Debt, so as to allow of a permanent lowering of annual taxation and a permanent raising of the level of social expenditure’ (Dalton 1923a: 14). The Labour Party’s proposal involved a one-off wealth tax set at zero for those owning less than £5000, with a marginal rate rising from 5% (on wealth holdings between £5000 and £6000) to a maximum of 60% (on wealth in excess of £1 million). Total payments would rise progressively, from 1.2% of total wealth for an individual worth £6000 to 59% for

someone worth £10 million. The capital levy would yield at least £3 billion, Dalton estimated, enough to pay off almost one-half of the internal debt (see *ibid.*: 32).

Both Keynes and Pigou supported the principle of the capital levy, Dalton noted, if not the precise details set out by Labour (see *ibid.*: 12, fn. 1, 49, fn. 2). None of the alternatives to the capital levy was attractive: doing nothing was not an option; a sinking fund would take far too long to have a significant effect; a forcible reduction of the rate of interest on War Loans would unfairly discriminate against holders of one type of security; repudiation could not be contemplated; and currency inflation would enrich speculators at the expense of wage earners and fixed-income recipients (see *ibid.*: 14–21). In addition, none of the many objections that had been raised to the capital levy was at all convincing, Dalton claimed. It would not be easy to evade:

No person resident in this country and liable to pay the Levy would escape liability by exporting his capital. He would be assessed on his total net wealth, whether situated at home or abroad. Legal liability could only be evaded if, before the imposition of the Levy, the owner emigrated along with this capital (*ibid.*: 62) ... And the levy would not on balance discourage saving, since it would permit a substantial reduction in income tax rates, with much of the increased post-tax income being saved (*ibid.*: 63).

The Labour Party's proposal had already generated a great deal of ill-founded criticism from Conservatives, which Dalton dissected at some length (*ibid.*: 47–66). His own pamphlet provoked an immediate critical response in the form of a 71-page pamphlet by Harold Cox (1923), published by the National Unionist Association. In the 1929 edition of *Principles of Public Finance*, Dalton acknowledged that there was no longer any significant political momentum behind the proposal for a capital levy, but he continued to defend it in principle (Dalton 1922a: 264–269).

He returned to questions of public finance in 1934 as the lead author of a collaborative volume on *Unbalanced Budgets*, to which he contributed the introduction and conclusion. As he explained in the Preface, he had planned and supervised the work of three young holders of Acland Travelling Scholarships which took them (in 1932–1933), respectively, to Berlin (Brinley Thomas), Rome (T.J. Hughes) and Geneva and Paris (J.N. Reedman) to study the impact of the Great Depression on the finances of European governments. The first of Dalton's two brief introductory chapters bore the title 'Capitalism in Paralysis', yet the metaphor

was, as he acknowledged, a dangerous one: ‘Analogies of the capitalist system must, indeed, be handled with discretion. For the stoutest defenders of this system reject the suggestion that it lacks a central brain, and that this defect might be remedied by deliberate state action to create such an organ’ (Dalton 1934: 3–4).

As leading ‘extremists of academic Liberalism’, he names his own LSE colleague Friedrich Hayek, and also Ludwig von Mises (*ibid.*: 4). Unlike Dalton himself, they were not at all impressed by the ‘bold experiments’ that were being made in the USA and the Soviet Union ‘to break down the barriers which “crisis” and “depression” have been allowed to raise between mankind and better times’ (*ibid.*: 9).

In the second chapter, ‘Unbalanced Budgets’, Dalton begins by taking a rather orthodox stance: ‘One incident in the general mess is a world-wide epidemic of unbalanced budgets’ (*ibid.*: 11), due principally to the fact that a falling price level reduces government tax revenue much more rapidly than its expenditure commitments. As the metaphor of an epidemic suggests, he here views budget deficits as indications of an illness, ‘a vivid symptom of world-wide disorder, both in economics and finance’ (*ibid.*: 12). In the first of his two concluding chapters, ‘Some Comparative Performances’, Dalton abandons the medical metaphors and pays sympathetic attention to the decisions of those governments that ‘have deliberately preferred a moderately unbalanced budget to the strain which would be required, whether through increased taxation or increased economies, to balance it’ (*ibid.*: 437). Dalton is referring here to France and Italy, and his evident (and very disturbing) sympathy for Mussolini is again apparent later in the chapter when he discusses the use of increased government expenditure, more or less fully financed through increased taxation, on public works (*ibid.*: 445–448). It reappears in the final chapter, ‘Some General Reflections’, when he ‘speculate[s], without dogmatism, whether modern Italy is not moving along a path which will lead, not only to Economic Planning, but to Socialism’ (*ibid.*: 455).

Much of this final chapter is, however, devoted not to Fascism but to ways of ensuring social justice in times of economic crisis. This, Dalton argues, requires not ‘a uniform percentage reduction of all money incomes, of whatever kind, in proportion to the fall in the general price level since a given date’, but rather ‘a scheme of progressive cuts, taking a higher proportion from large incomes than from small’ (*ibid.*: 454–455). This might be termed classical macroeconomics with a human face. Only at the end of the chapter does Dalton return to ‘the expansionist doctrine’ according to which escape from budgetary disequilibrium, and from its causes, should be sought, not by economies, which are a policy of contraction, but by a planned expansion

of production, designed to bring back idle resources of all kinds into employment and thus increase revenue, while diminishing expenditure for the relief of unemployment (ibid.: 457–458). In a footnote he cites Keynes as a supporter of this proposal, along with Sir Arthur Salter and Wilhelm Röpke (ibid.: 458, fn. 1).

Dalton's own position is worth quoting at some length:

Whether or not this doctrine is well founded is one of the major issues in current controversy. For there are those who hold it to be a dangerous delusion which, if applied, would make bad worse and postpone, if not destroy, hopes of a real recovery ... Here I will only express the personal opinion, without embarking on the lengthy argument which would be necessary to justify it, that those who advocate the expansionist doctrine have the better case, but that their policy, in order to be fully effective, needs to be pushed a good deal further than most of them seem willing to push it. I believe that freedom from the plague of recurrent booms and slumps can be found only in a Planned Economy (ibid.: 458).

There is no mention here of budget deficits or the national debt. One interpretation of this rather enigmatic passage might be that, for Dalton in 1934, countercyclical fiscal policy is a necessary but by no means a sufficient condition for sustained recovery. In macroeconomics, it seems, he had by that time shaken off Pigou but had not enthusiastically embraced Keynes.

4 The Economics of Socialism

As Dalton had noted in his introduction to *Unbalanced Budgets*, the new planned economy of the Soviet Union was 'still in course of being worked out, by a process of trial and error, with wonderful energy, though in face of tremendous difficulties'. It was 'a very gallant effort, in which it is, I think, a sign of mental senility to take no interest, though of childish credulity to find no flaws' (Dalton 1934: 10). He had made an extended visit to Russia in 1932 with a party from the New Fabian Research Bureau, talking to central planners in Moscow and their local counterparts in Kazan, Magnitogorsk, Rostov, Stalingrad and Sverdlovsk. Dalton stressed the improvised nature of Soviet planning, which, 'it was admitted in one conversation, is definitely post-Marxian. No direct guidance concerning its problems is to be found in any of the Marxian writings' (Dalton 1933: 20). Nonetheless, and despite the many signs of great inefficiency, he approved

of the way in which ‘unemployment has been planned away’ in the course of ‘the swift transition from the slowest to the fastest tempo in the world’ in terms of economic growth (ibid.: 16, 33).

Dalton’s overall verdict was favourable: ‘I returned from the Soviet Union strengthened in my belief that, for a community as for an individual, bold and conscious planning of life is better than weak passivity and the tame acceptance of traditional disabilities, that trial and error is better than error without trial’ (ibid.: 33–34). This attitude was largely shared by Dalton’s young disciples, Durbin, Gaitskell and Jay: ‘Fervently anti-Marxist in domestic politics, the Dalton kindergarten was none the less profoundly influenced by the example of Soviet planning. Parliamentary democrats as they were, they were not Keynesians, but apostles of strong legal and physical controls over the economy’ (Addison 1977: 49).

Dalton’s book-length statement of his socialist principles, entitled *Practical Socialism for Britain*, also reflected the influence of the Soviet system (Dalton 1935b). It included detailed proposals for the operation of each nationalised industry as a ‘Public Corporation’, which ‘must be unified within the national area, under a single control, though there may be in suitable cases a large measure of local devolution in administration’. Each corporation should be managed by public servants, with ‘no element of private profit, in the sense of the participation by private investors in any surplus realised by the undertaking’. Critically, Dalton argued, ‘each public corporation must work according to a plan, whose aim is efficient public service, but the plans of different corporations must be continuously co-ordinated in a larger national plan’ (ibid.: 95). It was also necessary to ‘socialise the leading financial institutions, enforce a proper measure of social control upon financial policy, and infuse a social purpose, as distinct from a profit-seeking purpose, into financial operations’ (ibid.: 185). The Labour Party’s proposed National Investment Board would ‘be one of our most effective instruments of Socialist planning and national development, a powerful agency for dealing with unemployment’ (ibid.: 213).

Dalton advocated ‘five lines of policy to reduce unemployment. We must slow down the entry of the younger generation into the field of employment; speed up the exit of the older generation from this field; reduce the hours of labour; plan and push national development; plan and push international trade’ (ibid.: 253). If this placed the emphasis on reducing the labour supply and on planning, rather than on Keynesian measures to increase effective demand, Dalton did also mount an extended attack on ‘the orthodox Treasury dogma’, which he regarded as ‘quite untenable’. He cited Keynes and Colin Clark on the need for increased public expenditure and public

borrowing to reduce unemployment: ‘There is no valid ground, *in this connection*, for distinguishing between new public and new private works. Both lead to a demand for labour, materials and money. Both, when these are available and not in use, reduce unemployment. Common sense, therefore, triumphs over the Treasury view’ (ibid.: 259; italics in original). Dalton also maintained that it was important to keep both long-term and short-term interest rates ‘to the minimum’ possible. Cheap money was essential, and the proposed ‘socialised banking system both should and can promote this end’ (ibid.: 263).

It is significant that Dalton ended the economic section of the book with three chapters on ‘Equality’, culminating in a chapter-long discussion of ‘Inherited Wealth’ (followed by two chapters on foreign policy and the preservation of peace). In the final analysis, socialism for Dalton meant ‘a very great reduction in our present economic inequalities’, which implied that, ‘while the average level of well-being must be greatly raised, the rich shall become poorer and the poor richer’ (ibid.: 319). This should be achieved very largely through fiscal policy: greatly increased public expenditure on education, health and social services, together with progressive taxation of incomes and inheritances. But he was not averse to other proposals: ‘[W]e should, I think, turn our minds also to the possibility of new taxes on luxury consumption’ (ibid.: 326), together with the imposition of a capital levy ‘to reduce both the deadweight debt and that attached to [newly] socialised enterprises’ (ibid.: 327). Thus, Dalton’s early theoretical work on inequality of incomes and on public finance continued to inform his subsequent thinking on the practical economics of socialism.

Three years later his former teacher A.C. Pigou published a small book entitled *Socialism versus Capitalism*. The long concluding paragraph is worth quoting in full:

If, then, it were in the writer’s power to direct his country’s destiny, he would accept, for the time being, the general structure of capitalism; but he would modify it gradually. He would use the weapon of graduated death duties and graduated income tax, not merely as instruments of revenue, but with the deliberate purpose of diminishing the glaring inequalities of fortune and opportunity which deface our present civilisation. He would take a leaf from the book of Soviet Russia and remember that the most important investment of all is investment in the health, intelligence and character of the people. To advocate “economy” in this field would, under his government, be a criminal offence. All industries affected with a public interest, or capable of wielding monopoly power, he would subject at least to public supervision and control. Some of them, certainly the manufacture of armaments, probably the

coal industry, possibly the railways, he would nationalise, not, of course, on the pattern of the Post Office, but through public boards or commissions. The Bank of England he would make in name—what it already is in effect—a public institution; with instructions to use its power to mitigate, so far as may be, violent fluctuations in industry and employment. If all went well, further steps towards nationalisation of important industries would be taken by degrees. In controlling and developing these nationalised industries, the central government would inevitably need to “plan” an appropriate allocation for a large part of the country’s annual investment in new capital. When these things had been accomplished, the writer would consider his period of office at an end, and would surrender the reins of government. In his political testament he would recommend his successor to follow the path of gradualness—to mould and transform, not violently to uproot; but he would add, in large capitals, a final sentence, that gradualness implies action, and is not a polite name for standing still (Pigou 1938: 137–139).

It is difficult to see anything in this statement that Dalton could have disagreed with. Redistribution of income through progressive taxation of income and inheritances; substantial public investment in health and education; anti-monopoly legislation; countercyclical monetary policy managed by a nationalised central bank; significant and increasing public ownership of key industries; national economic planning: this reads like the platform of the new Labour government in 1945, in the construction of which Dalton was to play a major role, and Pigou’s final sentence reads almost like a critique of the work of that government—from the Left. Dalton’s was indeed a Pigovian socialism.

5 Conclusion

Hugh Dalton achieved a great deal in an academic career that lasted only 16 years, above all in developing a rigorous economic case for a more equal society. His brief article on the measurement of inequality has probably been cited more often than any other paper published in an economics journal in 1920, and his book on the reduction of inequality was influential both between the wars and subsequently; it permeates Anthony Crosland’s *The Future of Socialism*, a central text in the ‘revisionist controversy’ within the Labour Party in the 1950s and beyond (Crosland 1956). Dalton’s work on public finance was again motivated very largely by his belief in the need to reduce inequality, which was also an important consideration in his advocacy of a capital levy. Moreover, his vision of a socialist future for Britain

placed more stress on reducing inequality in social welfare than on taking ownership of the means of production.

Like his teacher A.C. Pigou, Dalton was sure that interpersonal comparisons of utility could and should be made. Also, like Pigou, he believed that market failure was often much worse than state failure, so that extensive government intervention was needed. Dalton's distinctively Pigovian brand of socialism serves to remind us that neoclassical economic theory has not always been used to apologise for neoliberal capitalism.

Appendix: Dalton's Other Writings

Between the wars, it was not unusual for an academic economist to write more books than refereed journal articles (this would be a very dangerous career strategy today!). Thus, Dalton published only two full-length articles on economics in the course of his 16 years at LSE. One was the 1920 paper on inequality, discussed in detail in Section 2 above. Eight years later came an article on the theory of population, in which he drew on the many books on the subject that he had reviewed for the LSE's house journal *Economica* to survey the literature on the theory of optimum population and its relationship to unemployment (Dalton 1928a).

Dalton wrote a large number of book reviews, beginning with several contributions in 1914–1915 to the Chicago-based *International Journal of Ethics*, for whom he reviewed books on economics, politics, social policy and religion by authors who included W.J. Ashley, W. Cunningham and J.A. Hobson. Dalton wrote review articles for *Economica* on public finance (Dalton 1921b) and the economics of industry (Dalton 1922b) and lengthy reviews of works on population (Dalton 1923b) and public finance (Dalton 1928b), together with many shorter reviews on these and related topics. A quarter of a century later, his last two academic publications were reviews of the official history of the wartime economic blockade (Dalton 1953a) and Ursula Hicks's treatise on British public finances (Dalton 1954).

His fluent command of Italian, presumably acquired during his military service in the First World War, made Dalton the reviewer of choice for Italian-language books for the *Economic Journal* in the 1920s and early 1930s, a decade in which the global domination of the English language in academic economics was much less absolute than it would become after 1945. Some of the authors that he reviewed were well known (Achille Loria in 1921 and 1922; Roberto Michels in 1922; Maffeo Pantaleoni in 1925), others less so (Ulrisse Gobbi in 1921; Umberto Ricci and Alfonso

di Pietri-Tonelli in 1922; Ugo Spirito and Lello Gangemi in 1933; Leone Wollemborg in 1935). The Italian author who most influenced Dalton's own work was the socialist Eugenio Rignano, as we saw in Section 3 (see Dalton 1921a). Dalton also wrote books on international relations (Dalton 1928c, 1940) and contributed articles on political themes to the journal *Political Quarterly* (Dalton 1931, 1935c, 1936). His diaries for the years 1918–1960 were published 24 years after his death (Dalton 1986a, b). Finally, mention must be made of the three volumes of his memoirs, which drew heavily on the diaries (Dalton 1953b, 1957, 1962).

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12

Frank Walter Paish (1898–1988)

Robert A. Cord

1 Introduction

Frank Paish was on the staff of the Economics Department at LSE for over three decades, spanning from 1932 with his appointment to a Lectureship through to his retirement from the Sir Ernest Cassel Professorship in Economics (with special reference to Business Finance) in 1965. Although he made important contributions in a number of areas, the chief focus of his research was on macroeconomics, in particular output, unemployment, inflation and growth. Section 2 of this chapter presents a brief outline of Paish's life and career followed by an examination in Section 3 of Paish's first book, *Insurance Funds and Their Investment*. Section 4, the main part of the chapter, details Paish's views on macroeconomics, in particular his claim made in the 1960s that the government should aim to keep spare capacity (unemployment) at a certain level if it wanted to keep a lid on inflation. As part of this, we also consider Paish's stance on incomes policies. Section 5 assesses Paish's work on business finance. Section 6 concludes and asks why, despite the high public profile he often attained during his career, Paish has since fallen into relative obscurity.

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2 Life and Career¹

Frank Walter Paish was born in Croydon, London, on 15 January 1898. He was the eldest of five sons born to Sir George Paish, the well-known liberal economist, editor of *The Statist* magazine and economic adviser to David Lloyd George when he was Chancellor of the Exchequer; Paish's mother was Emily Mary. He attended the public school at Winchester and in 1916 was commissioned into the Royal Field Artillery, serving as an artillery officer on the Western Front before being wounded; Paish was a recipient of the Military Cross. According to Lionel Robbins, Paish was one of the few people who seemed to enjoy the war, the trenches comparing favourably with the rigours of Winchester (see Peston 1988: 14).² Returning to Britain, he entered Trinity College, Cambridge, graduating in 1921 after having obtained a Second Class (Division One) in both History (Part One) and in Economics (Part Two).

Paish spent the years between 1921 and 1932 working for Standard Bank of Africa. He was initially employed in London but was soon posted to Aliwal North in central South Africa and finally to Cape Town. Paish married Beatrice Mary in 1927, the marriage producing two sons and a daughter. Upon leaving Standard Bank, Paish secured a position as Lecturer at LSE in 1932, with promotion to Reader at the University of London following in 1938, before becoming Sir Ernest Cassel Professor in Economics (with special reference to Business Finance) in 1949, holding this position until retirement in 1965, when he was made Emeritus Professor and then an Honorary Fellow at LSE in 1970.

Paish undertook a number of other roles apart from teaching at the School. He was active in the London and Cambridge Economic Service (LCES), a five-decade collaboration between economists based at LSE and Cambridge starting in the early 1920s and focused on improving British economic data and issuing Special Memoranda on a variety of topical issues.³ Paish was Secretary of the LCES from 1932 to 1941 and from 1945 to 1949, as well as managing editor from 1947 to 1949, and contributed a number of research pieces to the Service's publications. In addition, he was assistant editor of *Economica*, assuming this role when the New Series

¹This section draws in part on Rose (2004).

²Paish provides a detailed and at times gripping account of his war service in Paish (1998).

³The service was host to a number of luminaries across the decades that helped to run it and/or contributed to its publications, including Robbins, Hayek, Phillips and Paish from LSE and Keynes, Robertson, Kahn and Stone from Cambridge (see Cord 2017: 310–312).

of the journal commenced in February 1934 and stepping down in 1949 when he was succeeded by Alan Peacock. As well as academic articles, Paish penned a number of book reviews, mostly for *Economica* and the *Economic Journal*.

During the Second World War, Paish was employed in the Ministry of Aircraft Production, where, under the leadership of John Jewkes, he was one of three Deputy Director of Programmes, the other two being Ely Devons and Alec Cairncross. Paish was also in the Home Guard during the war, where he reached the rank of Captain. In 1953, he was President of Section F of the British Association and was Chairman of the Association of University Teachers of Economics (AUTE) during the years 1951–1965; Paish's efforts in strengthening the AUTE were recognised by its designation of an invitational Paish Lecture, delivered annually. Paish was for a time, along with Lionel Robbins, an adviser to Peter Thorneycroft, Britain's Chancellor of the Exchequer from January 1957 to January 1958. For a while, he gave advice to the Liberal Party on economic questions, was a consultant to Lloyds Bank from 1965 to 1970, and wrote numerous articles for bank reviews and newspapers, in particular the *Financial Times*. Paish died on 23 May 1988 at his home in Hereford.

3 Insurance Funds and Their Investment

Paish co-wrote *Insurance Funds and Their Investment* with fellow University of London economist G.L. Schwartz, the volume appearing in 1934. In the Preface, the authors note:

Numerous books deal with the investment activities of banks, describing the origin and nature of the resources at their disposal and relating these to the employment of those resources and we are surprised to learn that there appears to be no book, at least in English, dealing with insurance in the same way. The material is available in scattered papers, mostly presented to professional associations and conferences, and we have thought it useful to co-ordinate it for the general student of economic problems (Paish and Schwartz 1934: ix).

Granted, the volume is less than 120 pages in length and therefore can only be regarded as an overview of its subject as it then stood and not a detailed analysis. It is broken down into three broad parts. The first part examines the origin and nature of the money which is under the control of insurance companies. Within this, the issues discussed include the purpose of life

insurance, the impracticability of permanent contracts in the absence of an insurance fund, annuities and group insurance. The second part discusses the different methods through which the funds of an insurance company are employed, while the final part examines the operation of these methods over time, specifically by devoting a chapter to investment experience, in particular in the USA and the UK, during the period between 1890 and 1929, and another chapter looking at the experience after 1929.

It seems that the volume was not generally well received. Granted, one reviewer stated that it was ‘fundamentally sound’ (Elderton 1934: 363). This aside, the same reviewer and others pointed to the book’s various faults. A common criticism was that it was unclear whom it would help, this despite the claim made by the authors that they were writing mainly for the ‘general student of economic problems’. An expert on insurance would understand it but would probably not learn much while a student of the subject might be misled by ambiguities, which are only sometimes clarified later in the volume. Examples of ambiguity include the use of ‘insurer’ instead of ‘insured’ in a diagram, the use, in one instance, of both terms interchangeably in the same paragraph, and a failure sometimes to identify whether what was being discussed either related to US insurance arrangements or those in Britain (see *ibid.*: 362; W.P. 1934: 271–272).

Another drawback was the balance of the volume, notably the fact that nearly half of its pages were devoted to an exploration of how insurance funds come about with the rest being a potted history of recent insurance history. One consequence of this was that Paish and Schwartz did not devote any space to some of the questions which might be of deeper interest to students. Such questions might include the role played by insurance companies as conduits for the flow of savings into investment and the advantages and disadvantages of insurance companies being run as private concerns as opposed to control by the State (see Davenport 1935: 323–324).

4 Macroeconomics

4.1 Output, Inflation and Growth

Paish laid out his ideas on output, inflation and growth in a series of publications, including ‘The Growth of the British Economy’ (Paish 1960), *Studies in an Inflationary Economy* (Paish 1962a) and ‘How the Economy Works’ (Paish 1968a). The most important of these was *Studies*

in *an Inflationary Economy*, especially Chapter 17, ‘Output, Inflation, and Growth’. Here, Paish makes calculations of the productive capacity for the UK for the period 1948–1961. On the basis of this, he argues that in order to secure stable prices, there must be around 5% spare capacity, equating to 2.0–2.5% unemployment; were spare capacity to fall below 5%, there would be a danger of inflation.

Theoretical motivation for this idea came, in part, from Paish’s colleague at LSE, Bill Phillips and the famous Phillips curve (Phillips 1958). The curve graphed the rate of increase in money wages against the percentage rate of unemployment. Paish altered this by focusing on the postulated relationship between money wage inflation and the economy’s growth rate (see Peston 1988: 14). As Wulwick (1987: 852) states, Paish’s approach was premised on two states:

A. With ample involuntary unemployment, money incomes grew as fast as real output and thus the price level was stable, whatever capacity growth, B. With little involuntary unemployment, money incomes grew faster than real output [excess demand], constrained by capacity, and thus the price level rose, and syllogistically concluded that C. At any time there was a minimum margin of unused capacity at which money incomes grew as fast as capacity, which was the necessary condition for long run stability that defined equilibrium.

The kernel of Paish’s ideas can in fact be traced back to Keynes, in particular *How to Pay for the War* (Keynes 1940 [1972]), where Keynes argued that it was excess demand which generated inflation. A key difference with the Keynesian approach, however, was that Paish set his analysis within a dynamic framework, arguing that increases in demand could be met, over time, by higher productivity.

Lipsey (2016: 418) has characterised Paish’s approach as the ‘stick’ theory of growth. In this scenario, lower aggregate demand (the stick) forces companies to increase their productivity and profits, a wider consequence being a rise in the economy’s rate of growth. To quote Paish:

In conditions of excess demand, with every firm overbooked with orders, there can be no effective competition. Only if at least a few firms in every industry are short of orders and urgently seeking new business does competition become a reality. In its absence, there is no compulsion on the least efficient firms either to improve their efficiency or to go out of business. If, as the result of more effective competition, the rate of progress of the least efficient firms could be brought nearer to that of the most efficient, the resultant

increase in the average rate of growth of efficiency might well be substantial (Paish 1962a: 332).

Paish's approach was in contrast to the 'carrot' theory, identified with the Keynesians, whereby excess demand was necessary in order for profits to be generated to pay for investment and to increase productivity, even if this resulted in mildly higher inflation.

Part of the real-world motivation behind Paish's approach was the relatively low rates of real GDP growth that were recorded in the late 1950s and early 1960s in the UK. After growing by 5.5% in 1953, 4.3% in 1954 and 3.8% in 1955, the expansion in economic activity decelerated to 1.6% in 1956, 1.9% in 1957 and 1.3% in 1958. Although there was a pick up to a strong 4.1% in 1959 and an even stronger 6.3% in 1960, growth again slowed to 2.7% in 1961 and 1.1% in 1962.

Paish may also have had in mind evidence from the USA. One possible example of this was a 1960 study by Clarence Long, published in the *American Economic Review*. Long pointed out that in the USA in the decade from the late 1940s to the late 1950s, real GNP per worker increased at a more rapid rate when unemployment was higher, this accompanied, for the most part, by low inflation. Long postulated three possible reasons as to the effects of a higher rate of unemployment:

Newly born firms and expanding industries may need workers in larger numbers than are being released by those that are currently dying, and the workers released may have obsolete skills and need new training. The unemployed can fill the breach while new workers are adapting and moving to the areas of expansion. Were it not for the unemployed reserves, the expansion might grind more slowly or generate inflationary wage and price increases. This I call the "lubrication effect" ... Unemployment could also exert what I call an "insecurity effect". Fear of losing jobs could make labour—union or non-union—less aggressive in pressing for wage increases and more disposed to put extra effort and care into its work ... The insecurity effect on workers may also lead to...what I call a "pencil-sharpening effect" on employers. Pressure from directors and stockholders of firms losing money and stiffer competition from other firms in similar difficulties might make management more anxious to cut costs and therefore more resistant to wage demands and more willing to increase efficiency and cut prices in order to regain lost business and restore profits ... The insecurity effect, the lubrication effect, and the pencil-sharpening effect may all work in the same direction, and it is probably not possible to disentangle them quantitatively (Long 1960: 156–157).

Paish's ideas attracted attention in British government circles, the height of this influence perhaps coming in the UK Budget presented in April 1967 by then Labour Chancellor of the Exchequer James Callaghan. Paish's theories had already been doing the rounds in Whitehall prior to this and were an inspiration behind the approach to economic policy adopted by one of Callaghan's predecessors as Chancellor, Selwyn Lloyd, a Conservative, who had held the office from 1960 to 1962. However, it was Callaghan who became the first Chancellor to 'embrace the Paish doctrine explicitly and without reservation' (*The Spectator* 1967: 413). The context to the adoption of a Paishian approach was Britain's mounting balance of payments deficit, a problem which would eventually lead to the devaluation of the pound in November 1967. In the interim, it was Paish's view that inflation was the cause of shortfalls on the balance of payments and that, as a result, unemployment should be allowed to rise. Once this was achieved, the economy could be left to grow at its productive capacity. With unemployment in Britain averaging 1.6% in 1966 but rising in the latter months of the year and in early 1967, what was considered to be the relatively neutral April 1967 Budget suggested that the government was prepared to let joblessness continue rising, albeit only marginally, in order to lower inflation and take some of the pressure off the balance of payments.

Despite its apparent simplicity, Paish's approach came in for criticism on both theoretical and empirical grounds. To start with, in a review of *Studies in an Inflationary Economy*, Roy Harrod called into question, quite strongly it should be said, Paish's calculations of the UK's productive capacity:

Professor Paish supplements his general doctrine by an assessment of the degree of under-utilization of capacity since 1948. Were his assessment correct, the resulting figures would tend, *pro tanto*, to confirm his argument that a near approach to full capacity working has, for most of the time since 1955, tended to produce excessive income increases. But the trouble about this whole chapter is that the figures given for productive capacity lack any statistical basis. An unkind critic—some might say unkind to the point of unfairness—could affirm that Professor Paish had invented a column of figures for productive capacity of a kind that would support his conclusions, and that the only evidence that he has for the veracity of these figures is that they do support his conclusions. Those who disagree with his conclusions will find no reason for believing that his figures for productive capacity are correct. I am quite confident that they are not correct; they show more than 97% of capacity employed in the first three quarters of 1961 (Harrod 1963: 88).

Elsewhere, it was noted that even when unemployment is high, unions were sometimes successful in securing wage increases for their members which were in excess of productivity gains, this being a reflection of union power above anything else (see Denton et al. 1968: 290). The range of necessary unemployment proposed by Paish could not serve as a constant for all time, but rather needed to be revised according to changes in labour market conditions: '[I]t should be expected that labour market policy will gradually increase the degree of utilization of labour by matching supply and demand in different sectors of the market, thus lowering the previous minimum of unemployment above which incomes policy is relevant' (ibid.). Put differently, Paish's schema only argued that a small rise in unemployment could help to induce wage increases which would subside to a level where they were no longer inflationary; but there was no guarantee that inflation would remain stable. For example, unions might take the view that they would prefer to push for higher wages for their members, even if this meant a higher rate of unemployment across the economy and if this higher rate fell mostly on non-unionised workers. Or where there was excess demand for labour, employers who were finding it difficult to fill vacancies, might offer higher wages to those working for other firms in order to tempt them to switch. Paish was well aware of criticisms of this type and did in fact change his estimate during the 1950s of the level of unemployment required to keep prices at a stable level (see Peden 2017: 50).

Keen to test the robustness of the Paish doctrine, some economists carried out empirical analyses. Two are mentioned here. The first was 'The Costs of Professor Paish', by Michael Sumner (1968). In this paper, Sumner examined Paish's claim that 'the permanent maintenance of [a] small proportion of unused capacity...would be more likely to increase than decrease the rate of growth of capacity' (Paish quoted in ibid.: 299). Sumner does this by investigating the relationship between capacity utilisation and investment, specifically by looking at whether the average lag between a reduction in the rate of investment decisions and the resulting fall in realised investment is long in relation to post-war recessions; in consequence, the mild fluctuations in the growth of actual investment give a misleading impression of the long-run effects of a decrease in the pressure of demand (ibid.: 304).

Sumner used seasonally adjusted quarterly UK data from 1956 to 1966 to test several versions of a 'flexible accelerator' in order to determine the average distributed lag between output activity in the manufacturing sector and investment. A key finding of Sumner's paper is that the adoption of Paish's proposal to allow a slightly higher average level of unemployment would actually cause a significant drop in investment demand and thereafter

realised investment. Sumner also finds that although investment would eventually recover, capital spending would be permanently below the level it would have been in a non-Paishian world. He concludes by stating that: ‘While the precise effects on the growth rate remain uncertain, it is difficult to believe that lower investment would not constitute at least a substantial offset to the even less certain stimulus afforded by the permanent maintenance of a larger margin of unused capacity’ (ibid.: 311).

Another empirical analysis was carried out by Pramod (Raja) Junankar in 1970. Junankar notes that there are in fact two versions of Paish’s hypothesis, which appear to be contradictory: on the one hand, investment is not affected by the level of spare capacity, and on the other, investment is maximised at some optimal level of spare capacity. Using UK data for the period Q1 1957–Q4 1966, Junankar tests both versions of the Paish hypothesis as well as other models of investment, notably the distributed-lag accelerator model, the modified capital-stock adjustment model and the spare capacity model. Although Junankar finds that the modified capital-stock adjustment model and the spare capacity model outperformed the others, spare capacity adversely affected investment across all the models, the conclusion being that, in opposition to Paish, investment and growth are stimulated most when the economy is running at full capacity.

4.2 Incomes Policies

After the war, Paish was in favour of controlling excess demand through indirect taxation (see Conclusion). However, he had also become interested in the question of whether incomes policies could be used to reduce excess demand, his main publication on this issue being ‘The Limits of Incomes Policies’ (Paish 1964a). Following an introduction, Part II examines the different causes of inflation, the relationship between inflation and the money supply and trade unions as monopolists of labour. Part III proceeds by considering the different types of economic controls which may be available to government, namely the control of wages and the control of prices or profits, before outlining the various difficulties of enforcing an incomes policy. This in turn is broken down into three categories: (1) Strains on employers: perceived difficulties of an incomes policy include the avoidance of wage controls by increasing welfare payments, fringe benefits and bonuses (wage drift) and the impracticability of rationing labour in Britain; (2) Strains on trade unions: problems include unofficial strikes and breakaway unions; and (3) Strains on government: difficulties include the challenge of keeping pay

down in the public sector and the possibility of strikes in public services. In Part V, Paish discusses issues around implementing an incomes policy, in particular the time lag between changes in earnings and in wage rates, the lag between changes in demand and in unemployment, and trying to estimate the economy's productive potential. Part VI concludes by detailing the relationship between incomes policies and the balance of payments.

However, it is really Part IV, 'The Extent of Unemployment', which forms the crux of Paish's analysis. His objective here is to try to ascertain the upper and lower limits for the rate of unemployment within which an incomes policy could effectively operate as a supplement in the effort to keep inflation down. Paish does this by looking at recent movements in earnings, wage rates, demand and unemployment, coming to the conclusion that an incomes policy would be effective in the UK if unemployment was in the range of 2.0–2.25% (a slight narrowing on the 2.0–2.5% that Paish had advocated elsewhere in order to control inflation). If unemployment was more than 2.25%, an incomes policy would be unnecessary as demand would not be strong enough to stoke inflation. On the other hand, were the unemployment rate to be below 2.0%, the pressure of demand for labour would be too strong for an incomes policy to withstand. Either way, the very small range of unemployment within which an incomes policy could play a role—0.25%—was so small as to make its implementation probably not worthwhile, hence Paish's advocacy of other policies with the overall aim of stabilising prices.

5 Business Finance

In an obituary of his one-time LSE colleague, Maurice Peston, noted that Paish was 'almost the founder of the study of the economics of business finance in [the UK]' (Peston 1988: 14). Paish's thoughts on the subject are contained in his book, *Business Finance*, the first edition of which appeared in 1953. As a measure of its success, the volume, which was based on lectures delivered by Paish to University of London students, went through four editions, the final edition appearing in 1968.

In the Preface, Paish sets out his motivations for writing the book. He begins by arguing that 'The problem of providing the necessary finance for the improvement and extension of the equipment of British private industry is one of the most urgent confronting the country today' (Paish 1953a: v). Paish then points out that the heavy level of personal taxation on people earning higher incomes in the UK meant that savings had declined which,

in turn, meant that less money was available for banks to lend to business. As a result, private companies were increasingly forced to depend on using their own profits if they wanted to finance an expansion of production or fund a new venture. However, the capacity of firms in the private sector to adopt this means of financing was itself restricted by high taxes on business profits. Granted, higher personal and business taxation as well as loans and gifts from foreign governments, notably the USA and Canada, meant that budget surpluses in the UK were subsequently redistributed through increased investment by public authorities. But in the opening years of the 1950s, the availability of such investment was itself constrained by lower overseas aid and a rise in defence spending by Britain. As a result, business was being increasingly left to its own devices with respect to the sourcing of finance. It was against this background that Paish wrote his volume, in order to ‘explain some of the more elementary principles and practices of business finance in Britain’ (ibid.: vi).

Business Finance contains two parts, ‘Principles’ and ‘The Provision of Finance’ covering just over 140 pages. The first part is broken down into four chapters, ‘The Nature of Finance’, ‘The Risks of Finance’, ‘Finance by Borrowing’ and ‘Limited Liability’. The second part is made up of five chapters, namely ‘Internal Finance’, ‘Private External Finance’, ‘The New Issue Market’, ‘Alterations in Shareholders’ Rights’ and ‘Dissolution of a Company’.

The most important chapter is that on ‘Internal Finance’, this to be seen in the context of the issues outlined by Paish in the volume’s Preface. In this chapter, Paish examines how finance is secured by a small firm and then traces how this changes as the firm becomes bigger. A number of issues linked to the internal finance of a company’s operations are considered, including valuation, depreciation and taxation and, related to this, how profits are impacted by changing prices. Paish provides a clear demonstration of how, when using then accepted methods of working out profit, a company’s capital can decline when the price of materials and replacement costs is increasing perhaps without the business owner even being aware of it (see Hobson 1954: 382). On the same theme, Paish shows how, when prices are falling, companies may find that they are not paying enough in taxes and dividends, and vice versa when prices are rising, this in turn contributing to more extreme trade cycles:

The effects of a fall in prices will frequently be reflected in the balance sheet values of stocks more quickly than those of a price rise ... At the same time, it will normally prove impossible to maintain the customary margin between

selling prices and *original* cost ... Even though the proportionate margin between selling prices and *replacement* costs, and therefore real profits, is maintained or more than maintained, it will appear to the businessman that he is making much reduced profits or even losses. While, therefore, he will continue to set aside out of the proceeds of his sales enough to replace the original cost of the goods sold...he will normally not feel justified in using the excess for financing an expansion in the scale of his business. Nor will he be liable to pay the surplus in taxation, or able to distribute it as dividends. There will therefore tend to emerge a cash surplus ... Thus, just as traditional methods of calculating profits tend to promote capital consumption in times of rising prices, so they tend to promote over-saving when prices are falling. And as they make profits appear fictitiously large when prices are rising, and encourage the boom to further excesses, so they make them appear fictitiously small in times of falling prices and intensify the depression (Paish 1953a: 77–78; italics in original).

This and other observations go some way to supporting Peston's claim noted at the start of this section that Paish was indeed one of the very early pioneers of business finance research in the UK. Another of Paish's colleagues at LSE, the academic accountant Harold Edey, supported this view by stating that *Business Finance*, when coupled with the banking textbooks that had been written by William Manning Dacey and Richard Sayers and a volume on the London capital market by Normal Macrae, would serve as a useful bridge between general economics and the study of finance (see Edey 1955: 520).

6 Conclusion

Although this chapter has looked at Frank Paish's life and some of his most important work, it is worth noting that he made important contributions in other areas of economics, including: (1) 'Banking Policy and the Balance of International Payments' (Paish 1936a) which develops the concept of the marginal propensity to import but, more importantly, is one of the earliest presentations of the theory of the balance of payments based on the theory of employment, in other words that one of the ways of addressing an adverse balance of payments *may* be to reduce employment and income, this being in contrast to the classical approach of letting prices adjust; (2) Papers, collected in the 1950 volume, *The Post-War Financial Problem and Other Essays* (Paish 1950a), specifically looking at the difficulties faced by the British economy in the years immediately after the Second World War, in

particular suppressed inflation and an excess of expected investment over voluntary saving. Paish rejected the curtailment of private investment via a tightening of monetary policy, instead advocating greater enforced saving through indirect taxation; (3) A 1947 piece on ‘Planning and the Price System’ (Paish 1947a) in which Paish argues for a significant liberalisation of the price controls which were still in place after the war; (4) A 1950 paper on the economics of rent restriction (Paish 1950b), almost the only examination of this subject by a British economist written around that time. In line with his views on price controls, Paish argues against rent restriction, noting its various disadvantages, including a reduction in labour mobility and its discouragement of the adequate maintenance of the housing stock; and (5) A 1938 article on the economics of gold (Paish 1938).

As well as his written contributions, Paish’s expertise was recognised by his fellow economists. In 1963, Roy Harrod stated that, ‘Professor Paish has long since established himself as being a foremost authority on monetary and financial questions in this country. One could go further, and, with good show of reason, enter him as a candidate for being *the* foremost’ (Harrod 1963: 85; italics in original). Harrod was not alone in his view of Paish. Lionel Robbins wrote: ‘Paish...is really the greatest authority in the country on financial policy and financial statistics. All of us here [LSE] eat out of his hand ... He is a clear expositor and conveys a sense of utter command of the whole range of statistics’ (Robbins to Low, 3 January 1958, BLPES Archives: Robbins/3/1/16). Among his younger colleagues at LSE, Paish was also regarded as an important figure:

[Paish] was my kind of economist: practical, policy-oriented, and concerned with theoretical and technical sophistication only to the extent that the problems he tackled so demanded. He has left behind nothing like the theoretical reputation of Lord Kaldor, but in the running debate that these erstwhile colleagues conducted over the years, it seems to be that Kaldor had the more sophisticated theories, but Paish had the better feel for how the economy actually worked. Paish had few grand designs, but was for example predicting and explaining stagflation, using quite unsophisticated expository tools, when most of us were just beginning to recognise its existence (Wiseman 2000: 46).

Given all of this, the question which naturally arises is why Paish is not better known? At least three reasons suggest themselves. First, although his work on output, inflation and growth did bring him to the forefront of public and government attention for some of the 1960s, his notion of allowing unemployment to increase to 2.0–2.5% in order to minimise inflation

and strengthen growth looked somewhat naive given the UK's and the US's experience in the 1970s and subsequently of relatively high unemployment and persistent inflation. Indeed, problems were beginning to emerge before this. For example, in the UK, although a small upwards drift in unemployment in the latter part of 1967 was accompanied by lower inflation, the continued effectiveness of the Paishian experiment was somewhat compromised by the subsequent feeding through of the effects of the pound's devaluation. Indeed, by 1969, wages were increasing faster than they had been previously despite unemployment standing at around 2.5%. Second, Paish was no mathematician. Despite his skills as an interpreter of economic data, he did not make use of the multivariate analysis and other modelling techniques that had become available to econometricians. Finally, Paish was modest in the extreme, leaving it to others to receive the plaudits.

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13

Arnold Plant (1898–1978)

Robert A. Cord

1 Introduction

LSE has produced and been home to a number of influential economists, some better known than others. In what can perhaps be described as the School's heyday in the 1930s, it is reasonable to suggest that it was Lionel Robbins and Friedrich Hayek who were the leading lights, with a very strong supporting cast in the form of, among others, John Hicks, Nicholas Kaldor and Abba Lerner. However, it is also reasonable to suggest that they were far from alone in underpinning and forwarding the reputation of LSE as one of the world's leading centres for the teaching of and research into economics. Also on the staff were a host of luminaries who made important contributions across a spectrum of subfields within the discipline, some of whom do not always receive the recognition that they deserve. Into this category falls Arnold Plant, a student at LSE and subsequently a long-serving professor at the School.

Plant's interests and activities were many and varied. He is best known for his work on copyright on books and patents for inventions, writing two important articles on these issues in the mid-1930s (see Plant 1934a, b), this linked with his enduring interest in, and aversion to, monopolies (Section 3 below). In addition, Plant had a deep interest in African studies,

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in particular the South African economy, this sparked by the years he spent lecturing at Cape Town University in the 1920s (Section 4), and in the teaching of and research into business administration (Section 5). The chapter ends with a conclusion (Section 6). Before embarking on an examination of Plant's main works, we begin with a brief outline of his life and career.

2 Life and Career¹

Arnold Plant was born on 29 April 1898, in Hoxton, East London, son of William Plant, a municipal librarian, and his wife, Thomasine Hollingshead. He attended the Strand School from 1909 to 1915, leaving at the beginning of the First World War to take up a job in an engineering factory.² Although he signed up for military service in 1916, poor eyesight meant that he was forced to return to engineering, before a change in regulations meant that he was able to join the 4th Royal West Kent Regiment in 1918. Another return to engineering took place after Plant was demobilised in 1920, when he became manager of the Steam Fittings Company at the age of 21.

It was around this time that Plant was advised by William (later Lord) Piercy, also a Hoxton boy, that he should consider improving his business training.³ Fortunately for the economics profession, Plant heeded this advice and enrolled at LSE as a full-time student on the BSc(Econ) degree in 1920, choosing to specialise in economic history. As well as this, he embarked on private study as an external student for the BCom degree, securing it in 1922 with a First Class. This impressive performance, which was interspersed by Plant winning the Gerstenberg Scholarship in Economics and Political Science, continued when, a year later, he was again placed in the First Class of students graduating from the BSc(Econ).^{4,5} Among his various teachers at the School, the most influential on Plant was

¹This section draws in part on Coase (1986, 1987), Tribe (2004), and Yamey (1980).

²There appears to be no extant record detailing Plant's academic performance at School.

³After a period working for a timber broker, Piercy became an undergraduate at LSE in 1910. He was appointed to the School's staff on graduation in 1913 and was drafted into government service on the outbreak of war. A distinguished business career followed (see Coase 1986: 82).

⁴Plant was one of six students out of a total of 65 to secure a First in the BSc(Econ) in 1923, another being Robbins. However, the recipient of the Gladstone Memorial Prize for the best performance in the degree for that year was Lilian Friedlander (see Plant Papers, BLPES Archives: Plant/1/2, folder 1). Meanwhile, the fact that Plant was able to study for two degrees at the same time led the University of London to change its regulations so that this would no longer be possible.

⁵Plant was also President of the Students' Union at LSE in 1922/1923.

Edwin Cannan, Professor of Political Economy, whose practical but robust approach to economic problems was to be reflected in Plant's later work.

Plant's reputation as a promising young economist seemingly went before him,⁶ with Yamey (1980: 92) noting that he was appointed to the newly created Jagger Chair of Commerce at the University of Cape Town before he had even received the results of his BSc(Econ); Plant had emerged out of a field of 24 candidates, this despite his youth. It was not just within the confines of LSE or Cape Town that Plant's prowess was apparent. No less a figure than Irving Fisher wrote to him in South Africa in late 1926 asking Plant if he would be interested in receiving a complimentary copy of *Mathematical Investigations in the Theory of Value and Prices*, originally Fisher's PhD from 1892, which had been published in a new edition in 1925 (see Plant Papers, BLPES Archives: Plant/1/1, folder 2(F-G)).

Plant remained at Cape Town until 1930, creating a 'successful small department' (Yamey 1980: 92) and developing the University's BCom degree. He lectured on a wide array of subjects, including banking and currency, insurance, factory organisation, business finance, transport economics, public administration and on specific 'local' subjects, such as South African railways (see Coase 1986: 83). It was also during this period that he married Edith Render, their union producing two sons. However, by 1928 it was clear that Plant was keen to return to England. He had acquired significant power as Head of the Faculty of Commerce at Cape Town, a position which also meant that he had become well known in government circles, this in turn giving rise to more opportunities for advisory work, opportunities which would probably not have been available in England. But in a letter to Beveridge in April 1928, Plant indicated that he and wife did not want to remain in South Africa because of the racial prejudice in the country (Plant to Beveridge, Beveridge Papers, BLPES Archives: Beveridge/2/B/27/1(A-H)).

Interestingly, Plant had been responding to a letter from Beveridge asking him whether he would be interested in being a candidate for the new position of Head of Department of Economics and Commerce at the recently created University College, Hull, which Beveridge had heard about. For whatever reason, Plant, who expressed an interest in the position, was not appointed. There also seems to have been the possibility of him taking up

⁶Plant was already displaying a degree of self-confidence as an economist before he had taken up the post in Cape Town, his views on how to reduce unemployment being published in a letter to *The Times* on 19 September 1923. Noting the usual (subsequently Keynesian) case for public works projects, Plant argued that private sector employment might be increased by at least two measures: first, exemptions on business property tax and second, changes to the system of local rates (see Plant 1923: 6).

a post at the University of Liverpool as successor to Sir Edward Gonner in the Chair of Economics, although again this did not transpire.⁷ But Hull's and Liverpool's loss was LSE's gain as Plant was to return to the School in 1930 as the Sir Ernest Cassel Professor of Commerce (with special reference to Business Administration), this after a further letter from Beveridge to him in October 1929 made it clear that Gregory, Robbins, Dalton and Beveridge himself had all agreed that the staffing of LSE's Economics Department required another person of senior standing and that that person should be Plant (see Plant Papers, BLPES Archives: Plant/1/1, folder 1(B)).⁸

Plant's lecturing duties centred on the Industry Group in the BCom, where, as Coase (1986: 85) notes, he had a 'profound influence on his students'. Plant also lectured to postgraduates in the recently created Department of Business Administration, where he became head of department in 1935.⁹

During his years at LSE, Plant became involved in a number of activities outside of the School, many of which were connected to government. He was a temporary civil servant during the war, his duties including directing the wartime social survey, being an adviser to the Ministerial Chairman of the Interdepartmental Materials Committee and serving in the Cabinet Office. Outside of wartime, Plant's was involved with numerous public bodies, notably as a member of the Cinematograph Films Council (1938–1969) and the Monopolies and Restrictive Practices Commission (1953–1956); he also served as Chairman of the Advertising Standards Authority (1962–1965). For this and other work, he was knighted in 1947. Plant died in North London on 19 April 1978, survived by his wife.

3 Property Rights and Monopoly: Copyright on Books and Patents for Inventions

Plant's chief contributions to theoretical economics were in the field of property rights, specifically his views on copyright on books and patents for inventions, this in turn underpinned by his dislike for monopoly. As Coase

⁷As an indication of the difficulties that could sometimes occur in trying to fill senior economics positions in provincial universities, at the time that the authorities at Liverpool had inquired about Plant's availability, the Chair of Economics at the University had been vacant since Gonner's death in 1922.

⁸On their return to London, the Plants would settle in Hampstead Garden Suburb, North London, just a few streets away from the Robbins family. However, relations between Plant and Robbins soured after the Second World War (see Howson 2011: 715).

⁹From 1932 to 1937, Plant also helped to run, with Robbins and Hayek, the famous Robbins Seminar (see Howson 2011: 250). Separately, he was involved with the London and Cambridge Economic Service for some years (see Cord 2017: 311) and served on the Editorial Board of *Economica*.

notes, Plant was ‘well ahead of the rest of the profession in realizing the need for an economic analysis of property rights’ (Coase 1977: 87),¹⁰ the culmination of which was of course the winning of the Nobel Prize by Coase himself for his work in this area.

Before considering Plant’s work, it is worth spending a moment noting the influence on him of David Hume’s views on property. Plant was inspired to examine the question of property rights through his reading of Hume’s treatment of the subject in his *An Enquiry Concerning the Principles of Morals* (Hume 1751). Plant summarised his own reading of Hume thus:

Property, he [Hume] argued, has no purpose where there is abundance; it arises, and derives its significance, out of the scarcity of the objects which become appropriated, in a world in which people desire to benefit from their own work and sacrifice. Systems of justice, he went on, protect property rights solely on account of their utility. Where the security of property is adequately assured, property owners generally see to it that scarce “means” are directed to those uses which, within their knowledge and judgement, are most productive of what they want (Plant 1934b: 30).

3.1 Copyright on Books

Inspired by Hume, ‘The Economic Aspects of Copyright in Books’ (Plant 1934a) presents a situation where books are produced in the complete absence of any copyright provisions. Plant then asks first, whether any books would in fact be written in this scenario, and second, would they be published, one of the key considerations in attempting to answer these questions being whether sufficient remuneration would exist for both author and publisher.

Plant notes that there are various instances where an author will be not concerned with receiving any payment for his or her output. Indeed, they will be happy to themselves pay for their work to appear in print. This might be the case, for example, where output is ‘unaffected by demand conditions’: ‘so long as they [the author] can go on paying they will go on writing and distributing their books’. In addition, there is a group of authors who ‘desire simply free publication; they may welcome, but they certainly do not live in expectation of, direct monetary award’ (ibid.: 169). Plant argues that many academic authors fall into this category, an echo of the ‘publish or perish’

¹⁰Confirmation of this view can be found in the continued citation of Plant’s work, decades later, in research on copyright and patents. See, for example, Layson (1982), Littlechild (1986), Hui and Png (2002), and Galloway and Kinnear (2004).

mantra. Either way, for both of these sets of authors, copyright carries with it little attraction as it only serves to restrict circulation.

What about a third group of authors, those who write for a professional living and whose remuneration depends directly on the number of copies sold? Plant acknowledges that the restriction afforded by copyright, and thus a monopoly, means that the possibility of reprinting is curtailed, the result being a boost in sales of the original copies. Nevertheless, monopoly levels of output are not normally regarded as 'preferable to the alternative products which free competition would allow to emerge' (ibid.: 170). Patronage may be one such product. Moreover, as Plant points out, British authors who had their works published in the USA had often received what were regarded as decent royalties despite the fact that such publications were not covered by the laws of copyright in that country.

In order to provide a balanced framework of analysis, Plant outlines the supposed advantages of a copyright system. They include: (1) As books are not considered to be necessities but rather as luxuries, demand for them is usually somewhat elastic, the result being that the presence of copyright monopoly cannot be injurious to the public; (2) Copyright enables the production of more and cheaper books; and (3) At the same time, an absence of copyright can bring about *too much* or *too little* production, the former occurring in the case when ignorance or envy results in many publishers printing the same thing, so flooding the market, and the latter where publishers are able to observe the consequences of possible over-printing and so do not print anything, thereby suppressing learning.

Running alongside this theoretical analysis, Plant (1934a) also provides a history of copyright law in the UK, beginning in Tudor times and going right up to the then latest developments, notably the Copyright Act of 1911. This was an important piece of legislation on at least two grounds. It extended the copyright period from the life of the author plus seven years, or 42 years from publication, whichever was the longer, to the life of the author plus 50 years for works published after 1 July 1912.¹¹ The Act also introduced a royalty system into book publishing. Under this arrangement, a work could be reproduced without consent during the last 25 years of a copyright provided that a 10% royalty was paid to the owner of the copyright. Plant welcomed this change as it enabled the 'second generation of readers after the death of an author to enjoy a wider circulation of his books at lower prices' (ibid.: 191).

¹¹This length of copyright was retained in the UK until 1995 when it was extended to the life of the author plus 70 years.

Given its monopoly status, Plant was of the view that copyright should be abolished. However, in line with the practical approach to economics that he had inherited from Cannan, he also acknowledged that this was an objective for the future. In the meantime, small steps should be taken towards this goal. It was here that Plant put forward his main proposal for copyright reform, namely that the royalty system introduced as part of the Copyright Act 1911 should be made to operate five years after the first publication of a book rather than for the last 25 years of a copyright. Again, the inspiration for this suggestion can be traced back to Hume. Writing to his publisher, William Strahan, in 1771, Hume notes: ‘I have heard you frequently say that no bookseller would find profit in making an edition which would take more than three years in selling’ (Hume quoted in *ibid.*: 194). Plant brings Hume’s observation up to date by pointing out that it was the practice of publishers to follow a model whereby they would ‘very promptly’ follow up the publication of a successful, more expensive (read: hardback) edition of a book with a cheaper (read: paperback) edition. Plant may be guilty of a little exaggeration here, as it was undoubtedly the case then, as it is now, that the more successful the sales of a hardback edition, the longer a publisher is likely to wait until printing a paperback edition so as to maximise profits from hardback sales. This aside, Plant maintains that under his proposal, security for publishers against competition would be preserved until their first editions were either disposed of or ‘remaindered’, remuneration for authors would continue on all sales throughout the full copyright period, and the public would no longer have to wait more than five years for cheap copies of the books they wish to buy (*ibid.*).

Following on from this, Plant argues that if enough demand existed the original publisher would be forced to reprint a book before the end of five years at a low enough price in order to keep out competitors. By pitching a reprinted volume at such a price, one effect would be to limit monopoly profits, an outcome favoured by Plant. However, this position did come in for some criticism. For instance, Melcher (1934: 748) noted that lower profits would result in there being less money available with which publishers could support more risky publications. When they are successful, these risky publications are a valuable addition to a publisher’s backlist, this being one of the main ways in which a steady income stream is built.

3.2 Patents for Inventions

Plant’s views on patents for inventions were articulated in his ‘The Economic Theory Concerning Patents for Inventions’, which appeared in the February

1934 issue of *Economica* (Plant 1934b). At its heart, the basic objection to patents is the same monopoly argument that Plant presented in his analysis of copyright on books.

A few points stand out from Plant's analysis of patents. He states that the purpose of a patent is to give an inventor a definite time period during which they are able to control the disposal of their invention and, in turn, make it easier for them to realise an income from it. By holding out the possibility of remuneration for the efforts of the inventor, the aim of the patent system is to foster invention. After an interesting discussion of what determines the amount of invention and other matters, Plant then looks at various issues pertaining to the patent system in the UK as it then existed. He first makes the point that a number of inventions take place outside of the range of patent law, one example being the fashion trade where the rate of invention reaches 'probably its highest point', but where innovation continues. Similarly, many medical inventions do not attract a patent, this seemingly due to altruism and the lead often provided by professional associations. Indeed, the 'whole field of scientific discovery lies outside the scope of the system' (ibid.: 45). Next, Plant questions the system of reward implied by patents, specifically the fact that only one person or one group of people are able to realise any financial gain from an invention, i.e. those who originated it. However, in order to bring an invention to market where it becomes useful to others can require the efforts of many groups of workers, a matter which is left unrecognised, at least in financial terms, by the existence of the patent system. Linked to this, the very fact that compensation may be restricted can be a deterrent to other inventors entering a field where they may otherwise have been able to improve on an existing invention. Finally, Plant points out that the period for which patents apply—16 years in the UK at the time, since extended to 20 years—is arbitrary. The fixed period patent approach avoids a host of problems, but it is by no means an optimal solution (ibid.: 46). Even the modifications that had been introduced into UK patent law in the form of compulsory licences and licences of right were not enough, in Plant's view, to repair the 'lack of theoretical principle behind the whole patent system' (ibid.: 51).

Although Plant's 1934 treatment of the patent system is undoubtedly one of the seminal works in the field, there are some important, and familiar, arguments in favour of patents, which receive only partial treatment in his analysis. Here, we mention two. First, where an invention requires the significant investment of research and development, companies are usually more likely to make such investment when they hold a patent. Plant largely confines his discussion of this point in favour of patents by focusing on 'exceptional

cases' where a 'new mechanism becomes socially desirable for a specific and very special purpose' (ibid.: 43), the (peculiar) example provided by Plant being that of a flying machine which could carry out non-stop flights around the Equator. In such cases, he argues that a special fund could be created to provide finance, although it is not clear where such funding might come from.

Another area which Plant only briefly touches on is trade secrets:

A hundred years ago it was...argued as a merit of the patent system that it provided an inducement to inventors to make public the nature of their inventions so that they would eventually be generally available for wider exploitation. When businesses were small, and processes might remain one-man or family affairs, secrecy and monopoly might indeed persist longer in open competition than under the patent system ... But the conditions of industrial production have changed in this respect. With large-scale manufacture, few valuable processes can now be conducted on so small a scale that prolonged secrecy is feasible ... [T]here may exist chemical processes in which the nature of the product defies analysis and reconstruction of the method of manufacture, and in which the nature and proportions of the ingredients can effectively be maintained as the secret of a few people; but such cases, if they indeed exist outside the pages of detective fiction and sensational literature, must surely be exceptional, and unlikely to be eradicated by the inducements of temporary patent protection (ibid.: 44).

This points to what has long been argued is a contradiction between patents and trade secrets, namely the question as to why an inventor would want to take out a patent on an invention if the very act of securing the patent means that they are forced to give away any secret processes or the like which may be integral to the invention? However, this overlooks the fact that any trade secrets must usually be divulged only at the time that a patent is originally applied for; any subsequent changes do not have to be revealed and so can remain a secret. Moreover, without a patent in place, an inventor runs the obvious risk of their trade secret being found out and exploited by competitors.

3.3 Subsequent Work on Property Rights and Monopoly

Plant followed up his work on copyright and patents with various pieces. However, none of them quite reached the heights of his earlier contributions. A 1948 article for *Lloyds Bank Review* was an in-depth examination of

the Monopolies and Restrictive Practices (Inquiry and Control) Act 1948, which created the Monopolies and Restrictive Practices Commission in the UK.¹² Plant welcomed the establishment of the Commission, but also noted that its work could do ‘little more than exorcize the devil of restrictionism’ (Plant 1948a: 21) and that the government must review other aspects of its economic policy in order to help bolster enterprise.

Finally, in his Stamp Lecture of 1953, entitled ‘The New Commerce in Ideas and Intellectual Property’ (Plant 1953), Plant attempted to bring his earlier analysis up to date by applying it to the then burgeoning sound recording and broadcasting industries. But, as Coase (1986: 88) notes, although the Lecture is ‘interesting’, it adds ‘little to the analysis of [the 1934 papers]’. As an offshoot to his views on broadcasting, Plant became an early proponent of ‘pay as you view’ television in two articles, one for *The Times* (Plant 1956a) and the other for *The Listener* (Plant 1958a). He noted in the first article that a variety of delivery systems had been developed in the USA and that their implementation was awaiting approval by the Federal Communications Commission (FCC). However, Plant’s return to the same subject in 1958 appears to have been induced by the tardiness with which matters were progressing in the USA and, underlying this, the apparent power of oligopolistic interests. Specifically, the two leading television networks had objected to a proposed trial of a pay-as-you-go system, in turn forcing the FCC to postpone the trial for a year.¹³

4 African Studies

Plant’s interest in the economics of Africa, in particular South Africa, was sparked by the time he spent at Cape Town in the 1920s. Some of his main thoughts were contained in an article entitled ‘The Economics of the Native Question’, published in the South African journal *Voorslag* in 1927 (Plant 1927). Plant displayed a good deal of bravery in coming out against the South African government’s policy of separating the races. His argument

¹²The Monopolies and Restrictive Practices Commission has been reconstituted several times since its inception, first as the Monopolies Commission in 1956, then as the Monopolies and Mergers Commission in 1973, and once more as the Competition Commission in 1999. The Competition Commission was dissolved in 2014 and was replaced by the Competition and Markets Authority.

¹³There is an interesting, albeit brief, discussion of the work of the FCC by Plant in a letter to Coase in the Plant Papers, BLPES Archives: Plant/1/39. Plant wrote to Coase after having read his former pupil’s seminal paper on the FCC (see Coase 1959).

was one based on economics and was again consistent with his opposition to interference with the efficient operation of the market. Plant argued that the government's approach was stifling competition in the domestic labour market and that this was damaging to wider economic performance: 'Competition is the force which induces us to co-operate more economically with each other' (ibid.: 22). Instead, it was the responsibility of the government to provide better educational opportunities to native workers as this would enable them to compete on a level playing field in the jobs market:

The prospects of great economies in production through the increased employment of uneducated, unskilled brute labour at low wages are exceedingly small ... To resist co-operation with the native is therefore to lower our Western standards of civilisation. It is equally detrimental to our own economic interests to attempt to confine that co-operation, on the side of the native, to the performance of menial work (ibid.: 24).

Plant was to return to these themes.¹⁴ In a book review published in 1965, he noted that, 'The continuing prosperity of White South Africa is already absolutely dependent upon the employment in its expanding urban factories of increasing numbers of permanent African workers. As a rigid policy, apartheid is doomed to failure' (Plant 1965: 828).

Plant's final contribution to these issues was in 1970 in the form of an introduction he penned for a volume published by the Institute of Economic Affairs on *Economic Issues in Immigration* (see Plant 1970a). In fact, Plant had already signalled his stance on immigration in the late 1930s in a contribution he made to *The Population Problem* (see Plant 1938). With world war approaching and some countries turning towards autarky, he argued that the world should be regarded as one economic unit and that one of the implications of this is that immigration, and emigration, should not be restricted. Taking the example of the UK, Plant notes that:

If we look back over the economic history of this country, we cannot fail to be impressed by the benefits which it has received in centuries gone by from the immigration of foreigners with ability, training, and ambition. This country

¹⁴Plant's interest in the South African economy was not just confined to official policy on race. For example, in a 1931 article for *Economica* he provided a detailed examination of the anti-dumping regulations which had been introduced in South Africa (see Plant 1931). At a broader level, and also in 1931, he co-authored 'Tariff-Making in Practice', with Frederic Benham (see Plant and Benham 1931), which considered issues such as the differences between tariff theory and tariff practice, emergency tariffs and permanent tariffs.

has served itself well by welcoming immigrants. In seeking their fortunes in England, free from religious and political persecution, they have helped to develop our industries and have enriched our population (ibid.: 141).

Plant adds: 'It would be deplorable if we ever came to the point in this country of attempting to *prevent* emigration' (ibid.: 140; italics in original). Later, in his 1970 piece, Plant reaffirmed his view that immigration should be regarded as economically beneficial as it allows economies to absorb the skills that migrants have to offer.

5 Business Administration

As noted above, before Plant entered academia he spent some time working in the engineering sector. Together with the influence of William Piercy, this experience probably played a part in triggering his subsequent interest in the study of business administration, reflected in Plant's decision to take the new BCom degree at LSE (which had been created just after the First World War in response to demands from business) and his broader view that economics should be able to provide at least some guidance to the ordinary businessman having to make commercial decisions.

However, there is a broader dimension here, namely that the teaching of business administration had begun to expand rapidly at the international level particularly in the USA as a result of the establishment of various business schools in the early decades of the twentieth century, the most prominent being Harvard Business School, which opened its doors in 1908. In the UK, the first business school had been created even earlier, in 1902, at the University of Birmingham. As such, LSE had some catching up to do. The new Department of Administration was set up in 1930. Plant's Inaugural Lecture, delivered in 1931, was entitled 'Trends in Business Administration' (Plant 1932a), where he aimed to 'mollify those at the LSE who might be hostile to the creation of a professorship devoted to the study of business' (Coase 1986: 85). As Coase (ibid.: 86) points out, the central point made by Plant in the Lecture is that the businessman is but one part of the economic system and certainly not the dominant one:

The controlling employer in the productive system is the community of consumers. The businessman is simply one of the many faithful servants who, according to their skill and application, anticipate more or less accurately what will be required, and take more or less adequate steps to see that it is available (Plant 1932a: 52).

Even if consumer sovereignty is assumed, however, Plant was also keen to stress that the relationship between consumers and producers is not completely one-sided, as demonstrated by the use of marketing and advertising by businessmen in order to induce sales. In an echo of Say's Law, '[A] dvertisement seeks to mould consumers' choice rather than submissively to accept it; to create a consumers' insistence for particular products' (ibid.: 54). Where this does not produce the desired result, producers can resort to other devices, including trying to squeeze the operations of their rivals by monopolising channels of distribution or by not selling to retailers who stock competitor products.

Plant concluded his Inaugural Lecture by stating that one of the driving forces being the Department of Business Administration's work would be to better understand, through investigation and research, the circumstances in which businessmen choose not to pool certain information, such as changes in supply and demand, even where sharing would *not* compromise competitive efficiency and could also help to dampen the boom and bust effects of business cycles.

At least three other important papers on business administration were to follow, 'Centralize or Decentralize?' (Plant 1937a), 'The Distribution of Proprietary Articles' (Plant 1937b), and 'The Analysis of Costs of Retail Distribution: Illustrated from Data Relating to a Sample of Departmental Stores' (Plant and Fowler 1939), the first two of which appeared in the volume *Some Modern Business Problems*. Plant (1937a) considers questions such as the optimal strategies that can be employed by businesses if they are to expand production without increasing the workload of existing employees and under what conditions firms should either centralise or delegate their various activities. The second paper, Plant (1937b), which has tie-ins with Plant's work on copyright and patents, examines the trade in branded goods. Among the issues discussed are whether a product should be branded or not, the scope for grading as a substitute for branding, the costs and problems of brand advertising and resale price maintenance. Finally, Plant and Fowler (1939) is an example of Plant's enthusiasm for collecting data in order to analyse the everyday operations of businesses. In this example, data on the cost structures of department stores in the UK is collected in order to make inter-firm comparisons of performance.

Running alongside Plant's research were his efforts to build the Department of Business Administration at LSE. The early years of the Department were not easy, with at least a year having to be spent on preparing teaching materials and assembling staff and the subsequent two years impacted by the hardships of the Great Depression, which made

it difficult to recruit students. In 1931/1932, its first full year of teaching, the Department had 12 students, with this falling to just seven in 1932/1933. However, with interest in the Department's work picking up from 1932/1933, student numbers rose to 28 for the academic year, with 26 attending in 1933/1934 (see Beveridge 1935: 4). Students took a full-time one-year course, pitched at postgraduate level, with modules including economics (taught by Ronald Coase), business finance (Frank Paish), accounting (Ronald Edwards), distribution (Ronald Fowler), industrial psychology and factory visits (Sheila Bevington) and business administration (Arnold Plant). Discussion classes were the main method of course delivery combined with visits by students to factories, shops and offices. In addition, prominent businessmen were invited each week to talk to students about the latest challenges facing their companies.

Although it quickly recovered from the troubles of its early years and students were in general successful in finding employment after their courses had finished, it remained the case that the Department 'never seems to have taken root' (Coase 1986: 85) at LSE,¹⁵ with the number of students enrolled in any one year never exceeding the 28 recorded in 1932/1933.¹⁶ As such, it was perhaps not a surprise that the Department was shut down after Plant's retirement from the School in 1965, this decision also motivated by the establishment of the London Business School in 1964.

6 Conclusion

As Ronald Coase wrote of his former teacher, Arnold Plant's analytical system was 'unsophisticated but powerful' (Coase 1987: 891). At the heart of Plant's approach was a firm belief in the power and importance of competition and private property with a parallel view that State intervention in economic matters would often lead to monopoly outcomes as well as the

¹⁵The fact that Plant was forthright in his view that vocational education should be based in universities did not meet with universal approval at the School, including by a 'majority of members of the Academic Board' (Dahrendorf 1995: 419).

¹⁶Perhaps realising that more effort, albeit ultimately unsuccessful, needed to be made to try to bolster student numbers, Plant became Director of the Commerce Degree Bureau in 1938, no doubt one of his specific aims being to encourage those graduating with a BCom degree to consider carrying on their studies by enrolling with the Department of Business Administration. The Commerce Degree Bureau was established in 1918 to advise and assist external students of the University of London studying for a BCom, although its functions did change subsequently until its closure in 1987.

protection and promotion of particular political interests. Although Plant's views may not have been in fashion in the 1930s, they had certainly become part of conventional wisdom by the end of the twentieth century.

Plant published comparatively little, especially in terms of formal academic articles—this was especially the case after the Second World War and may in part have been due to his diabetes (see Coase 1986: 89)—and only ever held two academic posts. He was nevertheless able to exercise considerable influence through his work and activities. Plant's papers on copyright and patents 'must surely be regarded as among the leading contributions of the day to the study of economic institutions' (Robbins 1971: 126–127). They remain classics to this day. On top of this was his important work on Africa, particularly South Africa, and on business administration. A final, important channel through which Plant was able to exert influence was through the generations of students whom he taught at LSE. Among the most notable were Arthur Lewis, Arthur Seldon, Basil Yamey, Ronald Coase, Ronald Edwards and Ronald Fowler, the last three (the 'three Ronalds') forming the core of what Baxter refers to as 'Plant's kindergarten' (Baxter 1991: 138–139).

It is for the above reasons that Arnold Plant deserves to be considered as an important LSE economist.

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14

Lionel Robbins (1898–1984)

Susan Howson

1 Introduction

Lionel Charles Robbins entered LSE in 1920, as a 21-year-old ex-soldier who had seen active service as an artillery officer on the Western front in 1917–1918, to read for the BSc(Econ). Graduating with a First in 1923, he was appointed to the staff two years later. Apart from three years at Oxford, 1924–1925 and 1927–1929, he was at the School for (almost) the rest of his long life. He was appointed Professor of Economics in 1929 at the age of 30 and, apart from 1940 to 1945 when like most LSE economists he was in wartime government service, head of the department of economics until his official retirement in 1961. He taught at the School as a part-time lecturer for another twenty years.

This chapter concentrates on Robbins's work *as an LSE economist*, though acknowledging some of his highly influential policy work in and after the Second World War, and therefore discusses his education at LSE and his teaching and writing on economics before and after the war. I shall not discuss his many activities *outside* LSE such as his service on the boards of the Courtauld Institute, National Gallery, Tate Gallery and the Royal Opera

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House, nor the Committee on Higher Education or his chairmanship of the Court of Governors of the School and his role in the 'troubles' of 1966/1967 and 1968/1969 except where they impinged on his academic work.

2 Education and Early Career

Robbins was born at Sipson, Middlesex, where his father was an unusually successful farmer; he attended Southall Grammar School before he went up to University College London in 1915 to read for a degree in English. His stay at UCL was short because he joined the army as an (underage) officer cadet in January 1916. Commissioned in August, he could not serve in France until he turned 19 in November 1917; he was wounded and invalided home in April 1918 and demobilised in March 1919. He had become a socialist during the war and now found employment working for the Labour Campaign for the Nationalization of the Drink Trade as assistant to Arthur Greenwood. When he heard of LSE from friends who were already there, he thought it 'offered just the course of study and environment which would be most congenial to me', a study of the social sciences unbiased by political views; fortunately, a chance meeting with his father led to his father's generously offering to finance three years there (Robbins 1971a: 69–71).

All students reading for the BSc(Econ) degree had to take economics (including economic history and currency, banking and trade), the British constitution, geography and either mathematics or logic and scientific method for the Intermediate Examination at the end of their first year. The lecturers for these subjects included Hugh Dalton for economics, Theodore Gregory for currency, banking and trade, Lilian Knowles for economic history, who were readers at LSE, and Abraham Wolf, who held a Readership jointly with UCL, for logic and scientific method. Robbins was particularly impressed with Dalton's teaching (Robbins 1971a: 76–77):

Who of those who sat through his lectures can forget that powerful presence, the enormous bald head, the infectious grin and the booming voice? ... As an economist, he had been trained by Pigou and Keynes at Cambridge; and before the war he had done research at L.S.E. under Cannan. This combination of the two traditions gave to his teaching and his thought a quality which was very distinctive;... the breadth of his views, his feeling for arrangement, his simple and yet elegant expository style made his teaching highly successful; and his technical writings, particularly the textbook on public finance, were

quite notable in their day. No one who wishes to understand the history of L.S.E. in those days should neglect the beneficial influence of Hugh Dalton.¹

After the Intermediate Examination, in which Robbins did well in all subjects except geography, he had to choose a special subject for an Honours degree, to be taken along with compulsory economics, history and political science plus French and German for reading purposes. Robbins chose the History of Political Ideas and thus became a student of the socialist Harold Laski, in spite of his enthusiasm for Dalton's lectures and his interest in finance as taught by Gregory. He was still a socialist, although disillusioned with the labour movement, and wanted the opportunity for 'wide reading of the great international literature in which the leading minds of the past had explored the possibilities of alternative patterns of political society' (ibid.: 80). As a result, Robbins in his second and third years read a good deal of economic theory, of history, economic and political, and of political philosophy. He attended, by choice, Gregory's advanced courses in currency and banking and, most important, the two-year course on the Principles of Economics given by the Professor of Political Economy, Edwin Cannan. Cannan's lectures, which were on Production and Value in one year and on Distribution the other year, were very different from the standard textbooks, such as Alfred Marshall's *Principles of Economics* (1920), as Cannan was concerned mainly with a critical examination of the theories of the English classical economists, but since his references to Marshall were almost as frequent as those to Adam Smith and J.S. Mill (see Cannan 1929 [1964]) his students were well acquainted with Marshallian economics. Robbins inherited Cannan's interest in the history of economic thought but fortunately not his poor lecturing style (Robbins 1971a: 83–85). With Cannan's permission, he joined Cannan's class in economic theory in which students specializing in economics presented papers. Robbins's contributions included papers on the theory of land taxation, the place of land in economic theory and the causes of unemployment; the last included an attempt to use the 'difficult analysis' of Dennis Robertson's *A Study of Industrial Fluctuation* (1915) to outline the course of a typical cycle (see Howson 2011: 98–99).

In 1923, the final examinations were held in October. When the results came out in late November, Robbins was one of six (out of 65) students to get a First. By that time, he had become engaged to be married to Iris Gardiner and he therefore needed to find a job; he had previously been

¹He also admired Dalton for showing that economics could be separated from ideology and politics while not hiding his own political views.

thinking of living on part-time teaching and journalism. In February 1924, thanks to Dalton, he was appointed research assistant to the Director of LSE, William Beveridge, who was planning a new edition of his 1909 classic *Unemployment*. Among other things, this permitted Robbins to pursue his interest in trade cycle theory by reading all the standard works on the subject; as he told his fiancée, the theory was ‘intensely interesting’ and would be ‘an enquiry of real importance’ if one could discover how to reduce cyclical fluctuations (see Howson 2011: 119).

In July 1923, Robbins had applied for a Fellowship in economics at New College, Oxford, one of several colleges seeking an economist to teach for the new degree of Philosophy, Politics and Economics (PPE). He did not get the job, but he did get an interview, and a year later he was unexpectedly offered a one-year temporary appointment while the successful candidate was away on a Rockefeller Travelling Fellowship in the USA. Robbins declined a similar Fellowship himself, accepted the temporary position and married Iris Gardiner in August.

Robbins’s main responsibility at New College was to give tutorials in economics to second and third year undergraduates reading for PPE; he also taught elementary economics to undergraduates in their first year. In addition, he gave eight lectures on Elements of Economics to forestry students; these followed the main lines of Dalton’s introductory lectures at LSE: beginning with matters of scope and method, moving on to the theory of production and then tackling value theory and its applications to the supply of factors of production, money and the distribution of income.

Dalton and Beveridge soon offered Robbins a more permanent position at the School, to which he thus returned as an assistant lecturer in 1925/1926; he was promoted to lecturer at the end of the year. Before he returned to LSE he wrote his first book, *Wages* (1926), a short, popular account of theories of wage determination. He also began to translate part of the Austrian Ludwig von Mises’s *Die Gemeinwirtschaft* (1922) in which Mises attacked the feasibility of socialist planning. Robbins had begun reading Mises’s work in 1923: he had had to learn German for his degree and he had fallen in love with Vienna when he visited it with a Viennese friend in the summer of 1922; by 1924, he had also shed his youthful socialism (and decided to vote Liberal in the general election of that year) (Howson 2011: 131–132). Although his translation was never published, his friend from undergraduate days Jacques Kahane used it when he translated the second edition (Mises 1936).

Robbins’s first teaching at LSE was the first-year course on currency and banking previously taught by Gregory. He made clear to his students his

preferences for free trade and fixed exchange rates, though criticising the UK government for returning to the gold standard in April 1925 at the pre-war parity. His most frequent references were to Cannan's *Wealth* (1914), Mises's *Theorie des Geldes und der Umlaufsmittel* (1924), and Robertson's *Money* (1922). The following year Robbins's teaching was considerably more extensive. In addition to Gregory's currency and banking lectures, he took over Dalton's first-year lectures (and Dalton's syllabus). Cannan had just retired and his successor not yet appointed, so, in place of Cannan's two-year course on Principles of Economics, Dalton gave a one-year course on Principles while Robbins gave a complementary course on Comparative Economic Theory, which was to deal 'historically and comparatively' with the same material. He also gave a short course on the economic problems of war which he later credited as leading him to reject Cannan's definition of economics as the study of the causes of material welfare (Robbins 1971a: 146). He gave his 'Comparative' lectures again in 1927/1928 and 1928/1929, after Allyn Young had been appointed Cannan's successor and although he (Robbins) was back in Oxford.

When Harold Salvesen, who Robbins replaced at New College in 1924/1925, decided in March 1927 to leave academic life for his family's whaling business, the College immediately turned to Robbins. As he wrote (*ibid.*: 109), this was 'much too tempting to be refused' and he readily accepted. Beveridge was very proud that Robbins was the first LSE graduate to gain a Fellowship at Oxford or Cambridge. Beveridge also asked New College to allow Robbins to continue lecturing at LSE in Young's first year.

As a Fellow of New College, Robbins's teaching duties now included lecturing twice a week in two terms of the academic year, as well as giving tutorials in economics up to 18 hours a week. His lectures on Elements of Economics were a shorter version of those he had given at LSE in 1926/1927; for his Introduction to the History of Economic Theory with special reference to Adam Smith and Ricardo in 1927/1928 and for Comparative Economic Theory: Distribution in 1928/1929 he used some of his notes for his LSE Comparative lectures. What is particularly interesting about these lectures is that they were the last time Robbins followed the old-fashioned treatment of economics in terms of production and distribution.²

²Robbins's notebooks for his lecture courses in the 1920s and 1930s survive in his papers. Nicholas Kaldor, then an undergraduate at the School, took and kept notes of the Comparative lectures given at LSE in the Lent and Summer terms of 1929. (See Nicholas Kaldor Papers, King's College Archive Centre, Cambridge University: 4/4.)

In Hilary Term 1929, Robbins also offered eight lectures on ‘Unsettled Problems in Theoretical Economics’. He kept his notes with those for a slightly later set of lectures on ‘The Nature & Significance of Economic Science’, of which he noted that they were the first draft of his famous *Essay on the Nature and Significance of Economic Science* (Robbins 1932a)—to which I shall return shortly.

Many other of Robbins’s well-known writings also came out of lectures or talks. While he was in Oxford, he worked on three of his most famous articles. His devastating criticism of Marshall’s representative firm (Robbins 1928), which demonstrated that the concept was redundant as well as unhelpful, was first given as a paper to the London Economic Club on 14 February 1928 and accepted for publication by Keynes as editor of the *Economic Journal* a month later (see Howson 2011: 155). His ‘Economic Effects of Variations of Hours of Labour’ (Robbins 1929), which uses Marshallian partial equilibrium analysis, was his first paper to the British Association for the Advancement of Science in September 1928.³ ‘On a Certain Ambiguity in the Conception of Stationary Equilibrium’ (Robbins 1930a) began as a talk to the Marshall Society in Cambridge in June 1929.

Allyn Young died suddenly in the influenza epidemic of early 1929. Beveridge asked Dennis Robertson to continue Young’s Principles lectures and Robbins to give his Comparative lectures and take classes in economic theory. The 30-year-old Robbins was too young to succeed Young in Cannan’s Chair but Dalton persuaded Beveridge that he might be offered a new junior Chair of Economics while the University continued—unsuccessfully as it turned out—to look for a new senior professor. Hence, ‘Confronted with this offer, what could I do but accept? ... I decided to leave paradise and to take up the position which...excepting the years of the Second World War, was to be my main preoccupation for the next three decades’ (Robbins 1971a: 122).⁴

3 Professor of Economics

In the first of those three decades, Robbins’s heavy teaching load included Elements of Economics until 1935, General Principles of Economics and his famous graduate Seminar every year, as well as numerous short courses mainly

³He also used Marshallian partial equilibrium analysis in another article on labour economics written in early 1930 (Robbins 1930b).

⁴The Chair of Political Economy was never filled and was finally abolished in 1961.

on economic policy or in the history of thought. His heavy load was due to the small size of the department, which in 1929/1930 consisted only of himself and John Hicks (Dalton was on leave to serve in the second Labour government). By the end of the decade, Robbins had, with Beveridge's help, transformed it into the major department it has remained, with eminent senior colleagues (including Robertson from 1939) and a group of very promising young economists, many of whom had been Robbins's students (such as Evan Durbin, Nicholas Kaldor, Abba Lerner and Ursula Webb).

Robbins's lectures were well remembered by his students. B.K. Nehru recalled 'the young and very handsome Lionel Robbins, tossing back his flowing mane of hair...lecturing to packed classes in the large lecture room on the ground floor' (Nehru quoted in Abse 1977: 24–25). Kaldor, as a final year undergraduate, heard the Principles lectures when they were first given, in the Lent and Summer terms of 1930:

Robbins' economics was the general equilibrium theory of Walras and the Austrians, rather than of Marshall, and his lectures followed the method of presentation of Wicksell and of Knight, *Risk, Uncertainty and Profit*. Robbins as a young economist absorbed this theory—the keystone of which is the marginal productivity theory of distribution in its generalised form, as expounded by Wicksell and Wicksteed—with the fervency of a convert and propounded it with the zeal of a missionary (Kaldor 1986: 4–5).

Robbins had been able to read Wicksell in German; he had read Wicksteed's *Common Sense* (1910) [1933] as an undergraduate. The main topics of the lectures were: exchange equilibrium; equilibrium of production first with factors given and then with factors flexible; special topics in equilibrium analysis such as consumer surplus, the law of diminishing returns and the theory of costs; and analysis of variations, which included the theory of money and interest (and which he probably did not give until 1930/1931) (see Howson 2011: 174).

Robbins gave his 'Nature & Significance of Economic Science' lectures in the Summer terms of 1930, 1931 and 1932. The issues he raised there and earlier in his 'Unsettled Problems' lectures in Oxford had been concerning him for several years (see Howson 2004: 421–422). In a note on 'My Difficulties. Writing Elements Lectures Autumn 1926', he had summarised them:

1. What meaning should be assigned to the term wealth? Should it have an objective or a subjective classification? A difference with Cannan.
2. Must Economics include Ethics? (See Hawtrey's objection to Pigou.) If not, can it be urged that Economic Welfare has an ethical connotation. If so what can be substituted.

3. What is the best description of the subject matter of economics.
4. If this can be discovered how does it affect the traditional classification of subject. Production Distribution etc. (Howson 2004: 422).

Robbins was already clear that his answer to the second question was negative (and reviewed Hawtrey's recent book accordingly (Robbins 1927)). His Oxford lectures began with the subject matter of economics, criticising at length the usual definitions of economics, especially Cannan's. He concluded that he should try 'a new approach':

What we want is not a definition of economics which classifies out a certain set of activities which it labels economic but one which indicates what aspects of human activity in general are significant to the economist.

Now if we think of human activity in general there are two features which seem to have significance from our point of view.

In the first place the ends are various.

Secondly the means of attaining them are very often limited & are capable of alternative uses (Howson 2004: 426).

Robbins had thus arrived at the definition of economics which he stated in the *Essay* (1932a: 15) as 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses'. In arguing for the superiority of this definition, he relied heavily on arguments made by Wicksteed (see Howson 2004: 426).

Robbins's LSE lectures began with the same discussion before moving on to 'economics and ethics'—where he stressed that his definition of economics was 'entirely neutral as between ends'—and to 'statistics, technology & history', where he emphasised the relativity of economic quantities. There were then two lectures on methodology: the first arguing that economics was a deductive science and the second attacking inductivist methods. His final lecture, 'Economics and Political Theory', was intended to show the significance of his view of economic science for policy: by way of examples, he both criticised the use of the law of diminishing marginal utility to justify policies of income redistribution, as it involved interpersonal comparisons of utility, and emphasised the limitations of the doctrine of *laissez faire* (see Howson 2004: 428–433). The six sections of the LSE lectures became the six chapters of the *Essay* (though Robbins changed some of the economic-theoretic examples and dropped the discussion of *laissez faire*). As I have argued elsewhere (Howson 2004), and Denis O'Brien (1988: 24–25, 1990) before me, in spite of the many footnote references to Austrian writers, the book is more Austrian in appearance than in content.

There is no doubt, however, that Robbins had become deeply interested in Austrian *macroeconomics* in 1930–1932, not surprisingly given his long-standing interest in trade cycle theory (and his ability to read German). At this time, Hayek, following Mises, was developing Wicksell's theory into a monetary overinvestment theory of the cycle. Robbins, reading Hayek's work (1929a, b), suggested to Beveridge in the spring of 1930 that Hayek might be asked to give the University's annual lectures in advanced economics the following year; Robbins first met Hayek when he arrived at the School to give the lectures in January 1931. (He also suggested that Hayek's work be translated by his students Kaldor and Honor Croome (Hayek 1931a, 1933).) In the meantime, Keynes invited Robbins, as the new Professor at LSE, to join a committee he had persuaded the Prime Minister of the new Labour government to set up 'to review the present economic conditions of Great Britain, to examine the causes which are responsible for it and to indicate the conditions of recovery' (Howson and Winch 1977: 46–48). (The UK, which had been suffering from high unemployment since the mid-1920s, was now feeling the impact of the world slump of 1929–1932.) Keynes recommended the members, who included Sir Josiah Stamp and Hubert Henderson, like himself members of the government's Economic Advisory Council, and Pigou, Robbins and Robertson (who did not serve) as 'leading academic economists': '[A]ll...are well accustomed to the most up-to-date academic methods...are essentially reasonable and good members of a committee, and happen to have given already a good deal of time and thought to the problem which would be set them' (Keynes quoted in Howson and Winch 1977: 46–47). As is well-known, Robbins clashed with Keynes in the committee and refused to sign its report.

The clash began over macroeconomic theory but it came to a head over a different issue. Keynes gave the members of his committee the proofs of his *Treatise of Money* to read; Robbins soon noticed the different implications of the Hayekian and Keynesian versions of the Wicksellian analysis. While the Keynesian view suggested expansionary monetary policy could produce recovery by increasing investment, the alternative Austrian hypothesis implied there had been too much investment before the slump: attempts to speed up recovery by lowering interest rates could at best only prolong the 'boom' and at worst deepen the inevitable depression. Other 'remedies' for unemployment such as public works or tariffs were also likely to make things worse.⁵

⁵EAC(E)13 Answers by Professor L. Robbins to Questionnaire prepared by the Chairman, 23 September 1930, Robbins Papers, BLPES Archives: 1/3; see also Howson (2011: 181–186).

As a lifelong free trader, Robbins particularly objected to Keynes's tariff-bounty proposal: a general tariff of say 10% on all imports and an equivalent subsidy to exports. Although the other members of the committee also objected, they agreed to let Keynes prepare a draft report; when Robbins saw it, he made it clear he would not sign a report with which he did not agree, as he did again when he saw a revised version. Keynes tried unsuccessfully to stop Robbins producing a minority report, and Robbins walked out of his last meeting. But although the other members of the committee signed the majority report, they (and Keynes once he had got over his anger) respected him for the stand he had made.⁶ When Keynes made his proposals public Robbins attacked them in the *New Statesman & Nation* in March 1931; he continued to speak and write against all forms of protection at home and abroad for the rest of the decade (see Beveridge et al. 1931: 148–84; Robbins 1931, 1937a, 1939a: 107–210).

In the next few years, Robbins also wrote on Austrian trade cycle theory, beginning with Robbins (1932b). Hayek was now a colleague (and a close friend) after Beveridge had suggested following Hayek's successful public lectures appointing him as a Visiting Professor for 1931/1932; he had been appointed Tooke Professor a year later. Before Beveridge made his suggestion, Robbins had been hoping that Jacob Viner—a good friend since they first met in Oxford in 1927—would take the Tooke Chair which Beveridge had offered him, on Robbins's suggestion, after Viner's successful lectures at LSE in December 1930; Viner decided not to accept in mid-February 1931 (see Howson 2011: 200). Robbins's reaction to Hayek's lectures was given in his Foreword to the published version (Robbins in Hayek 1931b: xi):

[F]or profound theoretical insight and power to open up totally new horizons, I know of only one work of its kind published in English since the war with which they can be compared—Mr Dennis Robertson's *Banking Policy and the Price Level* (1926). English-speaking readers will know that one could give no higher praise.

Robbins's Austrian book was his *The Great Depression* (1934) which originated in a set of four lectures at the Royal Institution of Great Britain in October and November 1933 (see Howson 2011: 247–249). He began both the lectures and the book by pointing out that the origins of the present depression dated from 1914 and the disruption to the world economic order caused by the First

⁶For more on the proceedings, see Howson and Winch (1977: 48–72), Howson (2009a: 261–264) and Howson (2011: 180–193); see also Robbins (1971a: 151–156).

World War. After dismissing most of the current fashionable explanations for the developments of 1929–1933, Robbins put forward his own preferred explanation of the *downturn* in terms of an Austrian-style monetary overinvestment theory. He then, briefly in his final lecture and at length in the book, explained all the many other factors that had caused the subsequent slump to be so severe and prolonged, especially the international chaos that had followed Britain's departure from the gold standard in September 1931.

Since in this chaos, 'tariffs, exchange restrictions, quotas, import prohibitions, barter trade agreements, central trade-clearing arrangements—all the fusty relics of mediaeval trade regulation...were dragged out of the lumber rooms and hailed as the products of the latest enlightenment' (Robbins 1934: 114), he devoted a long chapter to denouncing restrictionism and planning, using arguments he was to repeat many times in the following five years, before considering the 'conditions of recovery' (which included the restoration of stable exchanges and the reduction of trade barriers) and the 'prospects' which did not look bright.

The book was rightly very well reviewed: while many commentators, including his friends Dennis Robertson and Jacob Viner (see Howson 2011: 266–267), were not convinced by his 'Austrian' explanation of the *downturn*, his analysis of the causes of the severity of the slump, which is not dependent on the preceding analysis, is of lasting value.

Robbins (1971a: 159) saw his next book *Economic Planning and International Order* (Robbins 1937a) as his contribution to the socialist planning debate initiated by Mises (1922) and continued by Hayek (1935). But it reflects more his internationalism and his distaste for all forms of 'economic nationalism' which he feared could lead to war between nations. It was, like *The Great Depression*, written first for a lecture series, this time at the Graduate Institute of International Studies in Geneva in June 1935; unlike *The Great Depression*, it was not a book he later wished he had not written (Robbins 1971a: 159–160). Its three parts covered: the international significance of national planning (which included tariffs, quantitative restrictions on imports, control of overseas investment and exchange controls); 'partial' international planning (bilateral trade agreements, international cartels and international commodity agreements, and international regulation of wages and hours of labour); and 'complete' international planning (international socialism or communism). In each part, Robbins considered the arguments for various forms of planning from a genuinely international point of view rather than from that of any one country. Attempts at partial international planning, like national protectionist measures, could benefit only a particular group of producers in one industry or one country.

As for complete international planning, it was not only practically impossible; it was also incompatible with democracy. He concluded, following Cannan (and Alexander Hamilton), that the only solution to the problem of world order (and the avoidance of war) was an international political federation, in which national states would surrender certain rights, especially the right and power to make war, to an international authority (Robbins 1937a: 241–257).

When Robbins lectured in Geneva again in April 1939, with war imminent, he explained his views on the economic causes of war and again argued for international federation (Robbins 1939b). As a result, he became actively involved and influential in the movement for Federal Union in 1939–1940 (see Howson 2011: 346–352).

At LSE, meanwhile, in the mid-1930s the Robbins Seminar, run jointly with Hayek and Arnold Plant, had been thriving, with many major papers of that decade having their first hearing there, most notably Allen and Hicks (1934).⁷ Ben Higgins (1992: 7) described Robbins's role:

Robbins was a master of getting the most out of a seminar, and surrounded by stars as he was, his “most” was a source of continual excitement. He had a remarkable talent for synthesis. He would listen to the discussion raging around him, slumping deeper and deeper into his chair, his long legs stretched out further and further in front of him, his long hair falling further and further over his face. Then suddenly he would sit up straight, toss his mane back from his face, and in a few trenchant sentences summarize the essence of the discussion and state his views on the truths to be derived from it.

The economic-theoretic innovations of the decade (such as Hicks–Allen) made their way into Robbins's Principles lectures, for which he rewrote many of his earlier lecture notes, and into the second edition of the *Essay* (1935). His lectures continued to concentrate on microeconomics; he left macroeconomics to Hayek, who lectured on industrial fluctuations and capital theory, and to their younger colleagues, in particular Durbin and Kaldor, who introduced Keynes's *General Theory* to their students (and became Keynesians) (see Howson 2009a: 267–271). Robbins's major revisions of the *Essay* were to the methodological chapters, where he tried to take account of the criticisms of a priorism by friends such as Gottfried Haberler and Fritz Machlup, as well as using Hicks–Allen to illustrate the nature of the propositions of economic theory (see Howson 2011: 271–272; see also Howson

⁷For more on the seminar, see Howson (2011: 168, 250–256, 316).

2009b). He explicitly declined to alter the parts of the book on which he had been most criticised on account of his denial of the scientific legitimacy of interpersonal comparisons of utility and defended himself against the common and inaccurate charge that he had recommended economists abstain from policy debates (Robbins 1935: vii–ix).

His own involvement in policy debates in the later 1930s shows that his views on economic policy were subtly changing. The clearest instance is that in 1937, after Keynes published his ‘How to Avoid a Slump’, Robbins published an article on ‘How to Mitigate the Next Slump’ (Robbins 1937b) which recognised the possible utility of suitably designed countercyclical public works. When he republished the article, he explicitly noted his ‘slightly more hopeful view’ of government expenditure and his recognition that ‘measures of old-fashioned financial orthodoxy...in the short run may have the effect of intensifying depression’ (Robbins 1939a: 213, fn. 1, 218, fn. 1).

4 The Second World War

Robbins taught at LSE in Cambridge during the phoney war period of 1939/1940, giving his Principles course to both Cambridge and LSE undergraduates and running the Seminar with Hayek. In June 1940, to his great relief, he was asked to serve in the government’s Central Economic Intelligence Service, which became the Economic Section of the War Cabinet Offices in 1941. In September 1941, he became the Director of the Section. As a member of the Section he was involved with his colleagues in creating the British war economy. Before Robbins entered government service, he had supported Keynes’s *How to Pay for the War* proposals (Keynes 1940 [1978]); once he (and Keynes) were in government, he helped to persuade the Treasury to facilitate the preparation of the new national income and expenditure estimates (by James Meade and Richard Stone) required to implement the proposals (see Howson 2011: 368–370). With Keynes’s help, he then turned to the problem of limiting consumption; during the early months of 1941, Robbins played the leading role in persuading ministers to allow the adoption of points rationing over the entrenched objections of the officials at the Ministry of Food (ibid.: 370–371, 375–378, 382–385); this was, as the official historian put it, ‘a signal victory...for economists over the *soi-disant* practical...a victory that was deserved on the merits of the case argued’ (Hammond 1951: 200).

As Director Robbins oversaw and actively supported the preparation of the Section’s plans for post-war reconstruction (which had begun under

his predecessor John Jewkes). On the domestic front, while James Meade wrote the first draft of what eventually became the Churchill coalition government's 1944 White Paper on Employment Policy, it was Robbins who rewrote Meade's major memorandum to make it more persuasive and then fought the fight to get its ideas accepted outside the Section (see Howson 2011: 438–439, 483–492). Meade, Robbins's Deputy Director, recalled: 'Lionel showed the greatest wisdom and diplomatic skill in confining the work of the section within manageable bounds and in establishing good relations between the section and the rest of Whitehall' (Meade 1984: 19). Cairncross and Watts (1989: 53–54) noted that 'Robbins...maintained particularly close relations with the Treasury and the Foreign Office. Above all, Robbins hit it off with [Sir John] Anderson [their boss]...[which] more than anything...allowed the Section to play an important and useful part as economic advisers'. Meade also described the Section under Robbins as 'a seminar of young academic economists chaired by their professor' (Meade 1984: 19) in which, as Robbins commented at the time, many issues of economic theory and policy were thrashed out amicably. Robbins in 1942 was prepared to admit in correspondence with Hayek that he had moved away from his Austrian position on macroeconomics (see Howson 2011: 418–419).

As a convinced internationalist Robbins was from the outset a powerful supporter of Keynes's Clearing Union plan for an international clearing bank to clear all international transactions between central banks and of his Commodity plan for internationally controlled buffer stocks for the main internationally traded primary commodities. He was equally supportive of Meade's complementary plan for a Commercial Union for post-war international trade. Robbins represented the UK at the UN Conference on Food and Agriculture at Hot Springs, Virginia, where he presented Keynes's Commodity plan in May 1943 and took part in discussions in Washington with US Treasury officials on the Clearing Union and on the American Stabilization Fund plan in June. Later that year he accompanied Keynes to Washington for the major Anglo-American 'conversations' on post-war international monetary and trade policy; in 1944, he was a member of the UK delegation to the UN conference at Bretton Woods which created the International Monetary Fund and the World Bank. Robbins's last major assignment as a civil servant was to join the difficult and protracted negotiations for a post-war loan from the USA in September–December 1945; there he succeeded in obtaining an agreement with the US Administration on post-war commercial policy, whose principles were incorporated in the General Agreement on Tariffs and Trade in 1947 (see Howson and Moggridge 1990).

Robbins had had little to do with the School since 1940, as Hayek lamented to their mutual friend Fritz Machlup in 1941, though he was involved in 1944 in the first post-war appointments of senior staff, including Karl Popper (see Howson 2011: 496–498).⁸

5 Post-war LSE

Robbins returned to the School at the beginning of 1946. After five-and-a-half years in government service, he had not only to resume his position as Head of the Economics Department but also to re-establish his own intellectual reputation in the profession. He chose to emulate his friend and mentor Jacob Viner and work in the history of economic thought, but first he had to begin lecturing again. (In and after 1947, he was also much involved in current policy debates, which generated some of his most astute and persuasive articles (see Howson 2011: 656–658, 672–680, 685–687, 695–696, 700–703, 721–727, 734–747, 795–803; Robbins 1997: 205–293, 331–369).) In 1946/1947, he gave a course on the Theory of Economic Policy, which was notable for Robbins's explicit use of the Pigovian welfare analysis; as Roger Backhouse (2009) has argued, Pigou's welfare economics was *not* a target of Robbins's attacks in his *Essay*. In 1947/1948, he resumed teaching Principles of Economic Analysis, after Kaldor left the School at short notice for the UN Economic Commission for Europe, and did so for three years, and again in 1957–1961 and 1964–1966; he now included current macroeconomic theory as well as microeconomics. (The macroeconomics presented was the 'neoclassical synthesis' version of Keynesianism.) Jack Wiseman heard the lectures in 1947/1948 (Wiseman 1989: 12):

[T]hey were a most skilful performance; how skilful I fully appreciated only after I became a teacher myself. For perhaps forty minutes, the fundamentals of the topics were laid out in a generally accessible fashion. In the last ten minutes, we were taken on a brisk tour of the frontiers of knowledge, so that the budding specialists took away a perception of the things they needed to know more about.

Robbins began to offer his famous History of Economic Thought course only in 1953/1954, as Terence Hutchison had been teaching it (at first jointly with Hayek before Hayek abandoned LSE) since 1947. When he first

⁸Robbins had heard Popper speak on 'The Poverty of Historicism' in Hayek's seminar in 1936; in 1943 he had read, at Hayek's request, the manuscript of *The Open Society and its Enemies* (1945).

took the course over, he utilised his notes for the several short courses in the history of thought he had given at LSE before and since the war, especially one on Schools of Economic Theory given in 1929–1931 (see Howson 2011: 752).

Robbins also revived the Seminar, at first with Viner who was visiting LSE in the summer term of 1946; the following year the topic was, at the suggestion of Ronald Coase, the economics of public utilities. After a couple more years of discussing applied topics, the focus of the Seminar reverted to economic theory. William Baumol thought it was ‘an incredibly efficient machine for stimulating ideas, exchange of ideas’ (Baumol quoted in Howson 2011: 652–654). With the appointment of another group of exceptionally bright young lecturers, the Seminar recovered its sparkle in the mid-1950s despite a greatly increased number of graduate students (see Howson 2011: 813–820). As in the 1930s, out of the discussions—especially those on recent publications in welfare economics (by Baumol 1952; Jan Graaff 1957; and Ian Little 1957) and monetary theory—came a run of important articles. One of the best known is Archibald and Lipsey (1958) who were inspired by the discussion of Don Patinkin’s *Money, Interest, and Prices* (1956) and ‘indebted to all members of the seminar for the stimulus afforded by these discussions’ (Archibald and Lipsey 1958: 1, fn. 1).

Since 1947 Robbins had been steadily rebuilding the School’s Economics Department, beginning by persuading Henry Phelps Brown and James Meade to join as Professors. As in the 1930s he enthusiastically encouraged the research and helped the careers of a long and distinguished line of students and younger colleagues, too numerous to list here.

Robbins’s first post-war book was *The Economic Problem in Peace and War*, the Marshall Lectures for 1946/1947, which he delivered in April and May 1947. There he used his wartime experience to illustrate both the general problems of policy making and his own changed views. With respect to the latter, he admitted he now thought macroeconomic policy should be used to maintain aggregate demand and employment: ‘I think this is the point on which I am most conscious of a change of point of view, not, I think, due to the war, but rather to the cumulative effect of reflections on pre-war controversies tested in relation to a somewhat new quantitative perspective’. He particularly acknowledged the influence of both Keynes and Robertson (Robbins 1947: 67–68; 1971a: 188).

His serious work in the history of economics began with his Simon Lectures at the University of Manchester in February and March 1950. These built on a short course on Theories of Economic Policy he had given at LSE in 1939 in which he had covered the topics of the social philosophy of the classical economists, Utopian and Marxian socialism, economic nationalism,

corporativism and syndicalism; at Manchester, he focused on the first of these (Robbins 1952: vii; Howson 2011: 703). Immediately after giving the lectures, he and Iris sailed for New York for a sabbatical term at the Institute for Advanced Study in Princeton, where he turned the lectures into his most successful book (Robbins 1952). His declared intention was to demolish the popular misconceptions about the views of the ‘English classical economists’ (by whom he meant ‘the two great Scotch philosophers, David Hume and Adam Smith, and their followers...Ricardo, Malthus, Torrens, Senior, McCulloch and the two Mills...also Jeremy Bentham) on the functions of the State and on social and economic reform (ibid.: 2–6). In a similar vein to the end of the first edition of his *Nature and Significance*, Robbins was concerned to emphasise their acknowledgment of the limitations of *laissez faire* and the need for some government intervention within a system of economic freedom. While he was at Princeton he added two chapters on their views on socialism, the second chapter devoted to J.S. Mill. In his final chapter, he strongly argued for their shared reforming instincts and utilitarianism. Robbins was criticised by some reviewers for his bias, but his writing and scholarship were rightly compared with those of Cannan and Viner.

Robbins’s most scholarly work in the history of thought was on Robert Torrens, which he worked on for several years after 1952. Starting with the intention of editing Torrens’s *The Budget* (1844), where Torrens had apparently first departed from the classical free trade position, he found his Introduction soon became ‘almost a book in itself’ (Robbins quoted in Howson 2011: 742–743, 748). One of the real strengths of the book which finally resulted (Robbins 1958) is its placing of Torrens’s writings in their contemporary context: as O’Brien (1988: 48) remarked, the money and banking chapters are ‘one of the great discussions of the Currency and Banking Debate in the entire literature’. Robbins was particularly pleased with the approval of Piero Sraffa and Jacob Viner. Also, having found that Torrens had stated his case against free trade in a publication earlier than *The Budget* he edited that instead (see Howson 2011: 783–787).

By the beginning of the 1960s, Robbins (Lord Robbins since 1959 when he received one of the early life peerages) had long been wanting to turn his Principles lectures into a book. A few draft chapters survive in his papers. In December 1960, however, the Conservative Home Secretary R.A. Butler asked him to chair a committee on the future of university education and research. He demurred, but when a former wartime colleague asked him ‘if I thought that anything I had in mind to write was likely to be as important’ he could not deny it (Robbins 1971a: 272–273). The book was never written. Robbins devoted most of the next three years to the ‘Robbins

Committee' and wrote its report (Committee on Higher Education 1963). He was now also the Chairman of the board of the *Financial Times* newspaper (from 1961 to 1971), an appointment which precipitated his retirement from his Chair in 1971. At LSE, he lectured only on the history of economic thought, though managing to keep the Seminar going with the help of Bill Phillips (whose famous machine had first been demonstrated in the Seminar in 1949). The Seminar became increasingly less important as specialised seminars in particular areas of economics became more popular; it ended, through lack of attendance, in 1965 (see Howson 2011: 816–820, 932–933).

Robbins published a useful collection of his recent papers in 1963. He took its title, *Politics and Economics*, from a lecture at the College de France in May 1961 in which he presented his views on the relation of economics to politics, arguing that 'there can be no question of a theory of economic policy which does not depend in the most intimate way upon political judgments and valuations' and that 'We must certainly hold fast to the idea of a neutral science of economics' (Robbins 1963: 19): in other words, we must recognise that economic policy making is inevitably normative but pursue the discipline of economics as a positive science. He included his review of Hayek's *The Constitution of Liberty* (Robbins 1961) in which he made clear his differences with Hayek on political philosophy. He strongly criticised Hayek's lumping the nineteenth century English Utilitarians together with Continental Rationalists and his accusing them of promoting collectivism. He equally strongly criticised Hayek's belittling of the welfare state and his *Road to Serfdom* (1944) warnings of its possible dangers.

Robbins published two other collections at the beginning of the 1970s (Robbins 1970, 1971b): the first a selection of his articles and reviews in the history of economic thought and the second a revision of an earlier collection (Robbins 1954). He gave the Chichele Lectures at Oxford in 1966, which were published as *The Theory of Economic Development in the History of Economic Thought* (Robbins 1968). In October 1968, he succeeded Lord Bridges as Chairman of the Court of Governors of LSE. His first year in the position was the year of the second round of the 'Troubles', at the height of which he was much criticised for apparently usurping the role of the Director, Walter Adams. As in the first round in 1966/1967 Robbins continued to give his weekly history of thought lecture, stepping over or round 'sitting in' students when necessary. He distracted himself from the stress by beginning to write his autobiography (Robbins 1971a) in the spring of 1969. A longer lasting source of stress was his Chairmanship of the Library Appeal to raise funds for the rehousing of the British Library of Political and

Economic Science (BLPES) in the former warehouse of W.H. Smith & Son: the success of the Appeal was ‘his last great gift to the School’ (Dahrendorf 1995: 479).⁹ In his subsequent ‘retirement’, Robbins’s major activities included his history of economic thought lectures, which were transcribed by one of his grandsons in 1979/1980 and 1980/1981 (Robbins 1998), frequent attendance and speaking in the House of Lords, and much overseas travelling, often to give lectures, usually on the history of economic thought, where he continued to keep up with the literature. As he remarked (Robbins 1970: 9), his continuing interest ‘in the history of economic thought and the authors thereof...began when, as a student, I sat at the feet of Edwin Cannan, and will close, I fancy, only with expiring breath’.

In 1980, Robbins gave the Richard T. Ely Lecture at the American Economic Association meetings in Denver, Colorado, at the request of his former graduate student and colleague at LSE, William Baumol, who told him that ‘everyone would be delighted to hear you speak on the *The Nature and Significance of Economic Science* after 48 years’ (Baumol quoted in Howson 2011: 1063). He duly obliged.

As Robbins said on the occasion, it was ‘a good opportunity to gather together some reflections on the subject of that essay and perhaps to put things in such a way as to make peace with some of my critics’ (Robbins 1981: 415). He ‘resume[d] his position on the definition of the subject matter of Economics’ (ibid.) where he still held to his scarcity definition. He also still believed that economics was a science, though now on Popperian lines:

Economics conforms fundamentally to our conception of science in general: that is to say the formation of hypotheses explaining and (possibly) predicting the outcome of the relationships concerned and the testing of such hypotheses by logic and by observation. This process is used to be called verification. But, since this way of putting things may involve an overtone of permanence and nonrefutability, it is probably better described, as Karl Popper has taught us, as a search for falsification (ibid.: 417).

But he remained sceptical (too sceptical for some of his friends) of empirical testing of economic hypotheses. His position on interpersonal comparisons of utility (and hence his ‘somewhat adverse’ view of welfare economics) was unchanged, but he made it clear that he was not saying such comparisons

⁹On the ‘Troubles’ and the Library Appeal, see Dahrendorf (1995: 443–475, 477–481) and Howson (2011: 975–1030).

could not be made but that there can be no objective measurement of such comparisons. Hence ‘the practical use of such judgments which it is legitimate to make...is incomparably less than the claims made for Welfare Economics with capital letters’ (ibid.: 422). But he was not against using economics in discussions of practical policy: indeed, he was ‘emphatically in favour’ (ibid.: 423) of doing so as long as they were acknowledged to be ‘political economy’ rather than economics, as he went on to argue in the fourth and last part of his lecture.

His Ely Lecture was to be his last article in a major journal (Robbins 1981). He suffered a major stroke in July 1982, which put an end to his teaching and travelling. Robbins died in May 1984.

6 Conclusion

There is no doubt, of course, that Lionel Robbins was an ‘LSE economist’. He learned his economics there and taught what he had learned from his LSE teachers and others to generations of LSE undergraduates and graduate students. The economics teaching he received, especially from Cannan, Dalton and Gregory, had a lasting influence on his thought and views. Cannan’s influence is the most obvious: Cannan engendered his permanent interest in the history of economics; Cannan also set him the puzzle about the scope of economics which led to his most famous book. All of them encouraged him to read widely—in European as well as British economics—with the result that LSE teaching was more ‘cosmopolitan’ than that at Cambridge or Oxford.

The intent of Robbins’s *Essay* has been misinterpreted, at the time and ever since, but there is no doubt that it was influential. Although it took time for its definition of economics to be generally accepted—not until the 1960s, according to Backhouse and Medema (2009)—its methodological principles had an immediate impact on value theory and welfare economics in the 1930s, perhaps partly because of the ambiguities in Robbins’s philosophically unsophisticated views (see Howson 2009b; Hands 2009). More recently, although Robbins’s deductivist methodological position is not now accepted by the economic profession, his definition of our subject by and large still is.

Robbins’s direct influence on his students (and younger colleagues) was equally long lasting. For James Meade (1984: 19), who heard in Oxford in 1928/1929 the lectures that Robbins was also giving at the School,

The ebullient and exuberant purposefulness of his exposition was infectious. He was not interested in devising new elaborate theoretical constructions, but used his first-rate analytic mind to discover and teach us how the application of good economic theory to the real problems around us could make an important contribution to the formulation of wise and effective policy. He inspired me and, I suspect, many other students with the same philosophy.

The other students included Evan Durbin, Hugh Gaitskell and Meade's friends and contemporaries in the Labour Club (see Elizabeth Durbin 1985: 99). At LSE too those of his students who like Nicky Kaldor and Abba Lerner came to hold different views on economics (and politics) always remembered Robbins's role in the development of their thought (Kaldor 1986; Lerner 1953). Their successors after the Second World War have also written warmly of his influence on their education and careers (see, for instance, Baumol in Robbins (1998: xiii–xiv), Lipsey (2009: 845–846) and Laidler (1997: ix–xxxi)).

In 1938, John Hicks acknowledged the source of the ideas in his *Value and Capital* in 'the sort of social process which went on among the people who were working there [LSE], at that time, under the leadership of Professor Robbins' (Hicks 1939: vi). In 1968, Robbins dedicated the book of his Chichele Lectures to John and Ursula Hicks, who wrote to him—in a letter which Robbins kept in a Special Letters file in his papers—'We really are very pleased and honoured that you should have remembered us in this way. You know how much we value what you taught us, and the stimulus you gave us, in the old days; we have been working it out, in our various ways, ever since'.

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15

Friedrich Hayek (1899–1992)

Peter J. Boettke and Ennio E. Piano

1 Introduction

Friedrich August von Hayek was born in Vienna, Austria, on 8 May 1899. The son of August, an amateur, though respected, botanist, and Felicitas, young Friedrich grew up in an intellectually stimulating environment.¹ After having fought on the Italian front during the Great War, Hayek obtained a law degree and a degree in political economy at the University of Vienna, where he had studied economics under world-renowned economist (and member of the first generation of the Austrian School) Friedrich von Wieser.² After graduation, Hayek spent a year in New York City, where he interacted with some prominent American economists, including Wesley Clair Mitchell of Columbia University and the National Bureau of Economic Research.

¹For an overview of Hayek's life and intellectual development, see Caldwell (2004).

²Hayek had also expressed an early interest in analytical psychology after spending the winter of 1920 at the laboratory of brain scientist Constantin von Monakow at the University of Zurich (see Caldwell 2004: 240). His experience in Zurich inspired Hayek to write a short paper in the following summer, which he would later expand in *The Sensory Order* (Hayek 1952a).

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Back in Austria, thanks to Wieser's recommendation, Hayek obtained a job at the Austrian Chamber of Commerce, where he first interacted with Ludwig von Mises, the leader of the second generation of the Austrian School. The two started a long-lasting intellectual and personal relationship that would influence Hayek for the rest of his life (see Hayek 1978).

Alongside Mises, Hayek established the *Österreichisches Institut für Konjunkturforschung*, a research centre dedicated to the study of macroeconomic fluctuations. During these years, Hayek developed his restatement of the so-called Austrian theory of the business cycle, which led to an invitation to visit the Economics Department at the London School of Economics and Political Science (LSE) in 1929. Two years later, he joined the faculty at LSE, where he stayed until 1950. Hayek spent the rest of his academic career at the Committee on Social Thought at the University of Chicago (1950–1962), the University of Freiburg (1962–1968), the University of California, Los Angeles (1968–1969) and the University of Salzburg (1969–1977).

In 1974, Hayek was awarded, alongside Swedish economist Gunnar Myrdal, the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* 'for their pioneering work in the theory of money and economic fluctuations and for their penetrating analysis of the interdependence of economic, social and institutional phenomena'. In his later years, Hayek grew increasingly interested in the issue of the evolution of social institutions, inclusive of language, law and morals, and in the development of a general framework for the study of spontaneous order.³

Hayek died on 23 March 1992, in Freiburg. Spanning technical economics to political theory and moral philosophy, his work has had a lasting influence throughout all the social sciences.⁴

This chapter is organised as follows. Section 2 summarises the evolution of Hayek's role within the intellectual milieu of LSE during the 1930s. In Section 3, we restate Hayek's (erroneous) perception of the intellectual threats to his own understanding of the nature of economics. Sections 4 and 5 outline the arguments expressed by both sides of the so-called socialist calculation debate. We argue that Hayek's failure to foresee the rise of market socialism would impact his research interests for the rest of his life. Section 6 concludes.

³On Hayek's view of the spontaneous evolution of ordered structures in economics and beyond, see Boettke (1990) and Sugden (1989).

⁴On Hayek's relevance to legal theory, see Zywicki and Sanders (2008). For his legacy in political science, see Munger (2016). For economics, see Boettke and O'Donnell (2013) and Bowles et al. (2017).

2 The Hayek Story

Sir John Hicks started his aptly titled essay ‘The Hayek Story’ with the following recollection:

When the definitive history of economic analysis during the nineteen-thirties comes to be written, a leading character in the drama (it was quite a drama) will be professor Hayek. Hayek’s economic writings...are almost unknown to the modern student; it is hardly remembered that there was a time when the new theories of Hayek were the principal rival of the new theories of Keynes (Hicks 1967: 203).

Hayek arrived in London in the fall of 1931, recruited by Lionel Robbins in an attempt to make the London School of Economics and Political Science (LSE) the best institution for teaching and research in economics department in Europe. Robbins was himself a peculiar creature within the landscape of British economics. A student of Edwin Cannan, Robbins had been exposed to schools of thought other than the dominant Marshallian tradition, notably the work of the ‘British Austrian’ Philip Wicksteed (see Kirzner 1999), the Swedish economist Knut Wicksell and the Austrian Ludwig von Mises.⁵

At LSE, Hayek was surrounded by scholars who would later have a lasting impact on the discipline of economics and on British public policy. Other than Robbins, at that time LSE was the home of John Hicks (one of the fathers of modern mathematical economics alongside Samuelson and Arrow), Arnold Plant, William Beveridge and many others (see Caldwell 2004). Still, within a short time of his arrival, Hayek assumed a leading position within the School. Some of the most brilliant students to attend LSE in the 1930s—such as Abba Lerner, George Shackle, Ronald Coase and Ludwig Lachmann—were strongly influenced by his teaching (see Hicks 1967; Coase 1937). Indeed, in the early 1930s, Hayek’s work ‘dominated the discussion of economics at the LSE’ (Coase 1988: 7). Much of Hayek’s influence at the School was due to his prominence at the weekly seminar on economic theory he led alongside Robbins. All the best students and academics at LSE, as well as other leading academics, attended the seminar regularly (Hayek 1978: 370–371).

⁵Robbins can thus be seen as belonging to the ‘British Austrian’ tradition as well, as he had attended Mises’ private seminar in Austria in the 1920s and had been strongly influenced by Mises’ methodological teachings plus his writings about socialist economics (see O’Brien 1988).

At the same time, Hayek had emerged as one of the leading economists in the world. During his time in London, he was involved in many of the contemporary major academic debates in economics. Perhaps most notably, by the early 1930s, he had already had an early skirmish with Keynes over monetary theory. Hayek's review (Hayek 1931a) of Keynes's *A Treatise on Money* (Keynes 1930 [1971]) led the latter to criticise Hayek's own work on money and the structure of production (Keynes 1931; see also Hayek 1931b). As per Hick's passage quoted above, Hayek's approach to the study of macroeconomic fluctuations has emerged as the primary contender to the Keynes's increasing influence.⁶ Hayek (1931c) constitutes the more complete formulation of his approach to the study of the business cycle. Building upon earlier contributions by Austrian school economists such as Böhm-Bawerk and Mises, Hayek identifies the effect of changes in the interest rate on the structure of production of a capitalist system as the fundamental determinant of the trade cycle. When market and natural rates of interest do not coincide, the decision of entrepreneurs in selecting which production plans to pursue is blurred by a signal extraction problem. Absent this interference, a simple present value calculation would reveal which, among the many possible production processes available to the entrepreneur, is the most consistent with the subjective desires of consumers and the hard facts of relative scarcity. The manipulation of the market rate makes this effort more difficult. Thus, some entrepreneurs will end up undertaking socially wasteful and often individually unprofitable plans, resulting in the misallocation of resources in the economy. Eventually, the incompatibility of these plans with the underlying economic reality will lead to the discovery of the unfeasible ones, putting the boom to an end and starting the bust. According to Hayek, then, economic downturns are really the necessary readjustment of the capital structure of the economy. Thus, the increase in unemployment and the reduction in overall output observed

⁶See also Hansen's review of Hayek's *Prices and Production*: 'The present volume is, it seems to me, the only book of recent years which at all approaches Keynes's *A Treatise on Money* in the impetus it has given to renewed interest and discussion of business-cycle theory. This in itself is high praise. Altogether aside from the soundness of its conclusions, the value of the book and its important place in the recent literature of cycle theory is unquestioned' (Hansen 1933: 332). Moreover, despite Keynes's perceived 'victory' in his battle with Hayek, the latter's contribution to macroeconomics was not completely ignored during the post-war period. Robert Lucas (1977) explicitly identified Hayek as one of the inspirations for his general equilibrium model of economic fluctuations. Writing around the same time, James Buchanan referred to Hayek's business cycle theory as follows: 'As of now, there is really no alternative theory worthy of much respect, and we can predict that more attention will be paid to the seminal Hayekian ideas during the next decades' (Buchanan 2015: 258).

during the bust are part of the unavoidable process towards a sustainable organisation of economic activities.

In the mid-1930s, Hayek found himself debating with another leading (if not *the* leading) economist of the time, Frank Knight. Knight was the leader of the ‘Old’ Chicago School of Economics and an influential economic theorist on both sides of the Atlantic (see Medema 2011). His *Risk, Uncertainty and Profit* (Knight 1921) would have a long-lasting impact on the economics profession by introducing the notion of perfect competition and had even been adopted by Robbins as the introductory economics textbook at LSE (see Hayek 1978: 130).⁷ Although Hayek and Knight shared similar methodological views in their theoretical and applied economics, they strongly disagreed on the nature and function of capital in a market economy (see Boettke and Vaughn 2002). To Knight, economists could and should treat capital as a homogeneous mass of inexhaustible material, without consideration of such problems as maintenance and decumulation. Hayek took the opposite view: following Böhm-Bawerk and Mises, Hayek saw capital as a structure of heterogeneous yet ordered capital goods. This ‘capital structure’ approach plays an important role in the Austrian theory of the market process, the trade cycle and economic development (see Mises 1949; Powell 2010).

Looking back at the evolution of economics since the 1930s, it is apparent that the profession did not take the directions suggested by Hayek in these two debates. By the 1950s, macroeconomics was dominated by Keynes’s legacy (see White 2012) and the Knighthian notion of capital (see Lachmann 1977). Between his arrival at LSE and the end of the 1930s, Hayek’s ‘audience had dispersed’ (Hicks 1967: 205). Notwithstanding the early fascination with the originality of Hayek’s ideas, his colleagues at LSE might have realised that while ‘[Hayek’s economics] was in English...it was not English economics’ (ibid.: 204), a factor Hayek himself had not yet become fully aware of.

3 ‘The Trend of Economic Thinking’

The year 1933 was an important turning point in Hayek’s intellectual development. In his Inaugural Lecture as Tooke Professor in Economic Science and Statistics at the University of London, Hayek, for the first time in his career, shifted his focus on to the epistemic foundations of the economic problem

⁷On Knight, see Emmett (2009).

(see Boettke et al. 2010: 75).⁸ The lecture, entitled ‘The Trend of Economic Thinking’ (Hayek 1933), is of great interest to anyone interested in the history of economics. Most of it consists of a passionate defence of the science of economics from the assault by its intellectual and practical enemies. The intellectual attack came from the Historical School. From its German homeland, the School had started to enjoy some traction in the USA as well, where it was known as Institutional Economics. From a strictly academic point of view, the danger posed by the historicists was relatively small: in the first decades of the twentieth century, a division of labour had emerged within the economics profession. Economic theory was dominated by marginalists such as Irving Fisher, Frank Knight and Jacob Viner in the USA and Ludwig Mises, A.C. Pigou, Knut Wicksell and Philip Wicksteed in Europe. Empirical work, of both a quantitative and historical nature, was mostly founded in the camp of the historicists and institutionalists, notably Wesley C. Mitchell, Thorstein Veblen, John R. Commons and others^{9,10} (see *ibid.*: 126).

The danger to economic science posed by the historical approach was its appeal to politicians and policy makers. Since the time of Adam Smith, economists have stressed the coordinative nature of social interactions within the context of a market economy: ‘[Economics] showed that an immensely complicated mechanism existed, worked and solved problems, frequently by means which proved to be the only possible means by which the result could be accomplished, but which could not possibly be the result of deliberate regulation because nobody understood them’ (*ibid.*: 129).

Guided by this, the economist is bound to frustrate the public’s demand for any interference with the market process and is for this reason rejected by populists: ‘Even today it is regarded almost as a sign of moral depravity if the economist finds anything to marvel at in his science, i.e. if he finds an unsuspected order in things, which arouses his wonder’ (*ibid.*: 124). These populists have therefore found that, to achieve their goals, they must discredit the ‘abstract’ and ‘detached’ theorising of the economist. Far from being an efficient form of economic activity, the market is thus depicted as chaotic, the Hobbesian anarchy leading to a short, nasty and brutish existence. Given the market’s shortcomings, the State, led by the right people, not only can but *must* intervene. Only the application of reason through central planning can make order out of the mess of the market.

⁸See also Boettke et al. (2016).

⁹For a discussion of old institutionalism, see Rutherford (2008). On the relationship between Austrian economics and old institutionalism, see Boettke (1989).

¹⁰Hayek himself had worked closely with Mitchell during his stay in the USA in the mid-1920s (see Caldwell 2004: 154).

In the conclusion to his lecture, Hayek provides some reason for hope. Exposed to the beauty of economic reasoning, young minds seem to be immune from these socialist fantasies: '[A]n increasing number of economists so completely disagree with the current popular opinion which considers a progressive extension of State control as inevitable' (ibid.: 134). Even the most progressives among the youngest economists 'take a conservative attitude' (ibid.: 135) with respect to the role of the State in the economy.

The following few years would, however, prove Hayek wrong: '[N]eoclassical economists moved from dispelling the utopian prognostications of the historicists and institutionalists to propagating arguments for neoclassical market socialism' (Boettke et al. 2010: 73). Hayek's mistake was due to the false presumption, shared by others, that all marginalist economists, regardless of the tradition that they belonged to, shared a common understanding of the nature and significance of the economic problem and the market process:

[In most cases] the economist will come to very different conclusions from those reached by those to whom economic phenomena represent a number of independent events, explained by their individual historical causes, and in no way implied by the inherent logic of the system. This does not by any means imply that the economist will arrive at a purely negative attitude towards any kind of deliberate interference with the working of the system. But it may, and very likely will, mean an almost consistently negative attitude towards those proposals for interference which are not based upon an understanding of the working of the system; namely, the proposals which spring most readily and regularly to the lay mind (Hayek 1933: 133).

But what if not all economists share the same 'understanding of the [economic] system'?¹¹

4 The Unexpected Rise of Market Socialism¹²

In his overview of the calculation debate, Hayek refers to the work of some of the younger economists, who have given thought [to the economics of socialism], have gone much farther and are prepared to go the whole hog

¹¹Hayek was not the only economist to have this view. See, among others, Mises (1933 [1976]) and Viner (2013).

¹²This and the following sections build upon our discussion of the calculation debate in Piano and Boettke (forthcoming).

and restore competition completely, at least so far as in their view this is compatible with the State retaining the ownership of all material means of production (Hayek 1935: 218).

Among these 'younger economists' was Abba Lerner.¹³ Born in Ukraine, Lerner's family had moved to London in his infancy. After a failed attempt to start his own business, he decided to enrol at LSE to get a better grasp of the reasons for his failure. Robbins and Hayek soon discovered Lerner's brilliant mind. During his years as a student at the School, Lerner published some important contributions in the theory of international trade (Lerner 1932, 1934a, 1936a), industrial organisation (Lerner 1934b) and the economic theory of socialism (Lerner 1934c, 1936b, 1937, 1938) and helped to found a student-led academic journal, the *Review of Economic Studies*. Indeed, before finishing his graduate studies in London, Lerner had already established himself as one of the major intellectual figures at LSE. Hayek himself supported Lerner's candidacy for a job at the School, although the position went to Robbins's favoured candidate, Nicholas Kaldor (see Hayek 1978: 370). More importantly, Lerner played a leading role in the development of market socialism.

Market socialism developed as a response to Mises' argument on the impossibility of economic calculation under socialism (Mises 1920 [1935]).¹⁴ Marxian socialists argued that the government of the socialist commonwealth would be able to replicate and even improve upon the ability of capitalism to produce unprecedented levels of output. Mises replied that, contrary to socialists' wishes, central planning would never be able to achieve such a goal. Capitalist economies rely on the ability of entrepreneurs to use monetary prices in order to calculate the profitability of investment. This profit-and-loss mechanism generates a 'selection process' that reallocates resources from one production plan to another according to consumers' preferences and the relative scarcity of the resources of society. By getting rid of money and property rights over the means of production, socialism eliminates the institutional underpinnings of rational calculation and, therefore, prevents society from ever achieving the rational allocation of economic resources.

¹³For a detailed overview of Lerner's private and scholarly life, see Landes (1994).

¹⁴'[T]hough Marxist theory may have provided the foundations for the analysis of capitalism, it was neoclassical economics that provided the blueprint for a working model of socialism' (Boettke et al. 2010: 79).

Mises' challenge to collectivist economic planning is arguably among the most misunderstood statements in the history of economic theory. The argument, however, is rather straightforward. Without private property of the means of production, there can be no exchange and, therefore, no market for the means of production. In the absence of a market for the means of production, there can be no money prices for the means of production. Without money prices for the means of production, profit and loss calculation is impossible. Without profit and loss calculation, the system as a whole cannot direct the factors of production towards their most valued uses (Mises 1920 [1935], 1949). If socialism is defined as the absence of private property of the means of production, Mises' challenge logically follows: The rational allocation of economic resources under socialism is impossible. For this 'impossibility theorem' to hold, the following conditions must be satisfied:

1. Communal ownership of the means of production under the direction of a planning committee.
2. Consumer goods are freely exchanged at agreed upon terms.
3. The central planner does not have access to any market-generated prices of the means of production.

These conditions are important to understand why arguments attempting to disprove Mises on the basis of the historical existence of self-proclaimed socialist countries misidentify the target. None of these historical examples fully meet the condition of no exposure to market-generated prices of the means of production, as these regimes had access to world markets. Moreover, with the brief exception of War Socialism in Soviet Russia (1918–1921), no socialist country actually attempted the variety of overarching central planning imagined by Marx and his followers (see Boettke 1990). Furthermore, Mises never argued that socialism broadly defined was itself impossible. He did not argue that it is impossible for a group of self-identified socialists to take control of the levers of power in one or more countries and use the coercive means of the State to allocate resources in a more or less arbitrary manner. Indeed, Mises witnessed with his own eyes the rise to power of socialist parties in Russia and even his own Austria.

Mises had rightly identified that the possibility of central planning relies on the ability of the planner to make calculations *in natura* about the relative scarcity and value of resources. With few exceptions, socialist economists recognised the significance of the argument for the feasibility of socialism. In the two decades following the publication of Mises' paper, a group of

left-leaning economists developed new models of socialism that explicitly recognised the necessity of the price system even for a socialist economy.¹⁵

The standard account of the socialist calculation debated stated that Mises' challenge was met by neoclassical economists. In the most extreme account, the demonstration by neoclassical economists of the formal similarity of the economic problem under capitalism and socialism was a refutation of Mises. In less extreme accounts, Barone's and Pareto's demonstrations of the mathematical complexity of the system of equations representing a socialist economy demonstrated that Mises was wrong. But in all the tellings of the standard account, Lange and Lerner had thoroughly refuted Mises' 'impossibility thesis'. According to this interpretation, the socialist side demonstrated the theoretical possibility of an efficient allocation of resources in a centrally planned economy, forcing Mises' followers such as Hayek and Robbins to retreat towards a more pragmatic position:

Initially the argument focused around the feasibility of one system versus another. It was eventually accepted that both the Lange-Lerner and input-output systems could theoretically answer the economic question about the allocation of resources and manpower. The debated shifted to a dispute over which solution would be the most efficient one (Goldman 1971: 11).

The Austrians were seen as acknowledging that socialism is possible 'in theory' but should still be rejected on the basis of its impracticability and the political danger it poses to liberal-democratic politics. As Lavoie points out, this treatment is biased by two fundamental misunderstandings. First, Austrians and market socialists had two different interpretations of the notion of theory. To the latter, to be theoretical meant to analyse equilibrium states (and, in particular, the sufficiency conditions for their existence and the mathematical relationship between their variables). To the former, on the other hand, all theory must ultimately be directed at understanding the real world. General and partial equilibrium are among the many conceptual devices that can be used to explain the functioning of real-world economies. Most importantly, for the Austrians, the analysis of decision-making in disequilibrium and the equilibrating process it generates has the same methodological dignity of equilibrium constructs.

Second, the Austrian camp itself committed some important mistakes in conveying their message to the other side. The animosity towards Marxians

¹⁵For a history of the socialist calculation debate, see Lavoie (1985) and Boettke (2000).

and Institutionalists in the first decades of the twentieth century led them to overestimate the common ground with the Walrasian and Marshallian marginalists. According to Lavoie, ‘Mises and Hayek tended to take it for granted that ever since the completion of the marginalist revolution of the 1870s, all trained economists had been as subjectivist as they themselves were’ (Lavoie 1985: 100–101).

Still, in 1933, Hayek’s main target was not mathematical economics, but those economists that by refusing ‘to believe in general laws [of economics were] constitutionally unable to refute even the wildest utopias’ (Hayek 1933: 125). One can understand their surprise to see marginalist economics being used to defend (and provide guidance to) a system of central organisation of economic activity (see Boettke 2006). If they had had a better understanding of the fundamental uniqueness of the Austrian market process approach, they could have expressed their position less ambiguously. The realisation of this uniqueness came only as a result of the debate and led Hayek and Robbins to reformulate the original Misesian position by highlighting the themes of the knowledge-generating and coordinating properties of the market process in real-world economies, the subjective and tacit nature of knowledge, and the role of institutions (see Kirzner 1988). Due to the differences in language and emphasis between Mises’ original argument on the one hand and Hayek and Robbins’s on the other, this restatement gave the impression to those who wanted to hear the message as being the Austrians’ acknowledgement of the victory of the market socialists.

Mises’ challenge produced a radical transformation in the socialist camp. If before 1920 socialist economics was essentially Marxian, after the publication of Mises’ paper the socialist approach embraced the tools and methods of the ‘bourgeois’ neoclassical economics. This change was not restricted to theory and method, but eventually led to the complete abandonment of the very notion of generalised central planning. The socialist response to the impossibility of rational economic calculation under socialism took two forms. The first form was the so-called ‘equation solving’ approach, initially championed by Dickinson (1933) and later resuscitated by Lange (1967). The second form was that of ‘trial and error’ or ‘competitive solution’. Alternative outlines of this were proposed by Durbin (1936), Lange (1936, 1937) and Lerner (1937, 1938).

The first wave of market socialist responses addressed the problem of economic calculation under central planning from a formalist perspective. Building on the work of Italian mathematical economists Vilfredo Pareto and Enrico Barone, H.D. Dickinson (1933) argued that socialism can be modelled with the same tools used for the unhampered market as a system

of equations. In order to achieve the efficient allocation of resources, the central planner must merely solve the system of equations.¹⁶ If the economy can be represented as a system of equations, then it is logically possible for a central planner to solve it and allocate resources to their most valued uses. Furthermore, some market socialists, including Dickinson (ibid.) and Lange (1967), argued that central planners could use these equations to guide production in real-world economies. Dickinson, the earliest proponent of this approach (although he later moved towards the trial and error method as a superior guide to central planning (see Dickinson 1939)), writes that, although '[t]heoretically...difficult' (Dickinson 1933: 240), the task of the central planner could be solved to an approximation sufficiently close for the guidance of the managers of industry, by taking groups of more closely related commodities (composite supply or joint demand) in isolation from other groups. Once the system has got going, it will probably be unnecessary to create in this way within the framework of the socialist community a sort of working model of capitalist production. It would be possible to deal with the problems mathematically, on the basis of the full statistical information that would be at the disposal of the S.E.C. [Supreme Economic Council] (ibid.: 240, 242).

The equation-solving argument did not last long. As Hayek (1935) pointed out in his overview of the market socialist literature, Dickinson and others had misunderstood both the works of Pareto and Barone and the original argument by Mises. Mises never claimed that the analysis of a socialist economy is beyond the scope of economics. He was simply pointing to the fact—a fact that Pareto and Barone had made explicit in their contributions—that if socialism is to achieve the same result as the unhampered markets of economic theory, it must conform to the same optimality conditions (Pareto 1971; Barone 1935). The system of simultaneous equations developed by Walras and his followers was meant to represent the complexity of the market economy and identify the *direction* of a price change caused by a change in underlying conditions. It was never meant to calculate the exact money prices that would prevail in the economy.

The second step in the development of market socialism consisted in the formulation of more sophisticated (and, to some extent, more realistic) models of the socialist economy that would not rely on an improbable calculating power on the part of the central planning authority. Lange (1936, 1937) and Dickinson (1939) are the main works on the 'competitive

¹⁶Although he never endorsed the equation-solving approach during the socialist calculation debate, Oskar Lange revived it almost three decades later (see Lange 1967).

solution' to the calculation problem. Of the two, Lange's model has had a significant influence on the profession and is credited as both a forerunner of mechanism design (Hurwicz 1973) and as having definitely disproven Mises' impossibility argument (see Schumpeter 2013: 173).¹⁷

The reasoning behind Lange's model is a testament to the brilliant mind of its author. Lange identifies the necessary conditions for the achievement of an efficient distribution of resources by competitive markets: decentralisation of production and the parametric function of prices. Given these two assumptions, the market will necessarily converge towards a Pareto distribution regardless of its starting point. The Walrasian auctioneer will keep adjusting the price vector until all excess demands are eliminated. A socialist economy can achieve the same result simply by adopting these two features.¹⁸ Lange imagines an economy in which production is decentralised and entrusted to the manager of individual plants and the markets for labour services and consumer goods are freely exchanged. In every period, the central authority announces the price vector for capital goods and natural resources. Managers are commanded from the central planning authority to produce output according to the equimarginal principle, taking centrally set factor prices and market-generated output prices as given. At the end of every period, managers will report whether their industry has experienced excess demand (either negative or positive). The central planner will then adjust the price vector accordingly, increasing (decreasing) the prices of factors going into the production of goods in excess supply (demand). Thanks to the parametric function of prices, the system will eventually converge to the efficient allocation of resources:

Our study of the determination of equilibrium prices in a socialist economy has shown that the process of price determination is quite analogous to that in a competitive market. The Central Planning Board performs the functions of the market. It establishes the rules for combining factors of production and choosing the scale of output of a plant, for determining the output of an industry, for the allocation of resources, and for the parametric use of prices in accounting. Finally, it fixes prices so as to balance the quantity supplied and demanded of each commodity. It follows that a substitution of planning for the functions of the market is quite possible and workable (Lange 1936: 64).

¹⁷For a critical assessment of the Lange model from a standard neoclassical perspective, see Bergson (1967).

¹⁸Indeed, the socialist economy will outperform real-world market economies as it would not suffer from the detrimental effects of the widespread presence of market power and technological externalities and will not produce macroeconomic crises (see Lange 1937).

Lerner's contribution to the calculation debate does not fall within either of the two market socialist positions above. While agreeing with the result of Lange's and Dickinson's analyses, he took a different methodological and analytical position. Lerner's approach is interesting in many ways. In particular, he claimed that his analysis was derived by the application of the 'Austrian' approach, which he thought superior to the Walrasian and Marshallian alternatives in identifying the implications of alternative economic systems (Lerner 1937: 254). According to Lerner, economists on both sides of the debate on the feasibility of socialism misunderstood the fundamental determinant of efficiency in a competitive market and, therefore, in a socialist economy. This confusion led some, including Durbin (1936), to identify two principles that, if closely enforced by the managers of socialist firms, would bring about the efficient allocation of resources.¹⁹ The first principle is that production must be carried on until $P = \text{Min}(AC)$, while the second principle requires that $P = MC$.

According to Lerner, this conclusion is deeply mistaken. $P = \text{Min}(AC) = MC$ is 'merely [an accident] of the state of perfect competition' and not a necessary optimality condition, and its application to the management of socialist firms would potentially result in the waste of economic resources (Lerner 1937: 261–262). The only necessary requirement for optimality is that any unit of output is exactly priced at marginal cost, the latter being defined as 'the physical quantity of any factor needed to produce another unit of product multiplied by the price of the factor' (ibid.: 257, 270). Therefore, the socialist economy should not attempt to reproduce the competitive process of market economies. Since the latter's desirability comes entirely from the presence of marginal cost pricing, all the socialist authority has to do is to instruct the managers of factories and industries to set $P = MC$ (ibid.: 271).

5 Hayek's Role in the Calculation Debate

Hayek's move to London coincided with the beginning of the English language version of the calculation debate. It was not a coincidence that right at that time, he would start to think more seriously about the methodological

¹⁹Lerner (1937: 253) argued that the optimal allocation of resources must be the goal of any rational direction of the economy: 'If we so order economic activity of the alternative that is sacrificed, we shall have completely achieved the ideal that the economic calculus of a socialist state sets before itself'.

foundations of economic science: ‘It was only after I left Vienna, in London, that I began to think systematically on problems of methodology of the social sciences, and I began to recognize that positivism in that field was definitely misleading’ (Hayek 1978: 18). Hayek was, alongside Robbins, firmly on Mises’ side. But he was somewhat unhappy with his teacher’s formulation of the argument. This focused mainly on the individual’s ability to use market-generated prices to ‘rationally calculate’ the profitability of one course of action against another. Mises did not specifically investigate the nature and significance of these market-generated prices or the epistemological foundations of the market process.²⁰ Hayek would spend the rest of his career developing this issue. In his view, market-generated prices are the result of a process of the telecommunication of subjective, imperfect and contradictory knowledge between the many individuals operating in the economy. According to Hayek’s characterisation, ‘the whole economic problem is a problem of utilizing widely dispersed knowledge which nobody possesses as a whole’ (ibid.: 274). Thus, prices are not a mere parameter for the solution of a technological problem, but rather signals pregnant with knowledge about the relative scarcity of resources in the economy. It is these signals that guide entrepreneurs in making, pursuing and continually adjusting their plans and allow for the mutual coordination of economic activities in a market economy.

To say, with Lange (1936: 64), that, under central planning, ‘the process of price determination is analogous to that in a competitive market’ is a severe misunderstanding of how markets actually work. Real-world markets do not rely on a Walrasian auctioneer, randomly shouting price vectors until the solution to the general equilibrium problem is arrived at. Lange was misunderstanding a modelling technique to identify equilibrium conditions—which requires the absence of excess demand for all commodities—with a theory of how markets solve the economic problem.

The transformation of marginalist economics into a tool for social engineering particularly surprised Hayek, who at the time was teacher and colleague with market socialists, such as Durbin, Lange and Lerner (see Boettke 2006: 55, 57). The progression of Hayek’s thought in the second half of the 1930s was indicative of his frustration with the tacit and unwarranted assumptions underlying neoclassical economics as it had been used by the

²⁰Hayek believed that his argument was complementary to that of Mises: ‘I found out that the whole Mises argument about calculation really ultimately rested on the same idea [about the dispersed and subjective nature of knowledge], and that drove me to the ‘37 article, which then became the systematic basis of my further development’ (Hayek 1978: 383).

market socialists (see Hayek 1935, 1945). He also took this intellectual pursuit in two other directions. The first one was an analysis of the political and economic implications of the centralised control of economic activities (see Hayek 1944a) and the potential consequences for the survival of liberal-democratic politics in the West. The second one consisted in a history of the philosophical ideas that influenced the rise of the 'scientific' approach in the social sciences (see Hayek 1952b).

Boettke (2006) summarises the results of Hayek's investigation into economic theory and the political economy of socialism in the following way. First, freely operating markets generate and mobilise the tacit and subjective knowledge of millions of individuals.²¹ Second, socialism cannot achieve the rational allocation of economic resources because of its inability to mobilise tacit, subjective, and decentralised knowledge. Finally, central planning is incompatible with liberal-democratic principles of governance and, if persistently and consistently exercised, is doomed to result in the rise of an autocratic regime.

The best versions of Hayek's reformulation of Mises' argument can be found in two papers: 'Economics and Knowledge' (Hayek 1937) and 'The Use of Knowledge in Society' (Hayek 1945). Although not directly aimed at the market socialists, the content of these papers was clearly influenced by the socialist calculation debate. The former focuses mostly on the issue of the assumptions that economists make about knowledge when modelling market economies. Here, Hayek criticises the practice of assuming that knowledge about preferences, technology and costs is 'given', that is as 'data of the market'. To Hayek, this leads to a conceptual confusion between 'the objective real facts, as the observing economist is supposed to know them, and [those] things known to the person whose behavior we try to explain' (Hayek 1937: 39). The importance of this distinction is given by the fact that [t]he equilibrium relationship cannot be deduced merely from objective facts, since the analysis of what people will do can start only from what is known to them. Nor can equilibrium analysis start merely from a given set of subjective data, since the subjective data of different people would be either compatible or incompatible, that is, they would already determine whether equilibrium did or did not exist (*ibid.*: 44).

Thus, while general equilibrium analysis can serve an important role in the analysis of the effect of exogenous shocks on relative prices and the

²¹For modern formulations of the Austrian notion of economic knowledge, see, among others, Lachmann (1986), Boettke (2001), and O'Driscoll and Rizzo (2014).

allocation of resources, this cannot, by itself, assess the ability of a system to deliver efficient prices and allocations. To accomplish this task, economists must study how the institutional features of the system under study impact the way in which subjective data are generated and transmitted throughout the economy, and whether these are internally consistent and compatible with the ‘objective’ underlying economic and technological conditions.

Hayek himself provides such an analysis in his 1945 paper. The agents operating in an economy based on private property and market-generated prices are constantly attempting to achieve ends of their own choice. In so doing, they choose the most appropriate means based on each individual’s budget constraint and subjective knowledge. These agents do not possess an objective understanding of reality. To interpret their environment, they must rely on subjective, tacit and often factually inaccurate knowledge. This knowledge can be said to be subjective in two senses. First, it is the result of the interaction of the individual’s mind, which is different in its structure from that of any other individual, and physical and social reality. Second, this knowledge can be said to be subjective because it is limited to the specific contingencies of time and space in which the individual operates (see Hayek 1948: 80). By its very nature, this knowledge is necessarily dispersed and cannot be collected by one individual or group of individuals without compromising its accuracy.

For a market economy to function, no such concentration of knowledge is required. The price system generated by the competitive market process economises on the knowledge required by each individual participant to adjust efficiently to each other’s behaviour, expectations and to exogenous change. In making their decisions about buying and selling, saving and investing, and so forth, individuals look at market prices, which reflect, although not perfectly, the underlying economic conditions of the economy. An increase in the price of a good leads to the marginal buyers to refrain from consuming it without any need to know the causes of such an increase. Thus, the market process leads to the allocation of resources towards their more valued uses without anyone in the economy knowing what these uses are (see *ibid.*: 85).

Understanding the price system as a process for the creation and discovery of subjective and often contradictory information sheds light on the fallacious nature of Schumpeter’s ‘*ipso facto*’ argument. According to Schumpeter (2013: 175), ‘consumers in evaluating (“demanding”) consumers’ goods *ipso facto* also evaluate the means of production which enter into the production of these goods’. Since the knowledge held by individuals is subjective and limited, there is no *a priori* reason to believe that it is ever

going to be correct. The price system (which relies on the feedback mechanism of profits and losses) leads to an adjustment of this knowledge and, as a consequence, of the individual plans that rely upon it.²² Economic losses signal to the firm that the knowledge and expectations on which its plan was based may possibly be incorrect. For example, the firm might have overestimated the demand of its output from consumers, meaning that it was using an inefficient amount of one or more of the factors of production. The firm can (although not all firms at all times immediately will) adjust to this newly discovered knowledge by reducing the level of output or it will eventually go out of business. Either way, the system frees up resources from wasteful production processes to be employed by others in more valuable ways.

6 Conclusion

When he arrived at LSE in the early 1930s, Friedrich Hayek was a young, emerging economist working on monetary economics and business cycle theory. When he left the school two decades later, his interests had somewhat shifted away from these subfields to the study of the nature of the market process, the epistemological foundations of the price system in a competitive economy and the evolution of intellectual debates pertaining the status of the social sciences in the organisation of society. We attribute this change to Hayek's realisation that he and the economics profession of his time did not share a common understanding of the nature and scope of economics as a social science and to Hayek's participation in the three major economic controversies of the 1930s and 1940s. First, Hayek debated Keynes on monetary theory and its implications for the theory of the trade cycle. Keynes's aggregate approach to the study of economic fluctuations, an approach that does without methodological individualism and relative prices, would soon become the dominant paradigm in macroeconomics. Few resisted the appeal of the Keynesian Revolution, including fellow Austrian economists Ludwig von Mises (1949) and Joseph Schumpeter (1936) and Chicago economists Jacob Viner (1936) and Frank Knight (1937).

²²In a market economy, prices serve three functions: ex-ante guides to exchange and production; ex-post assessments of previous decisions through the profit and loss mechanism; and signals for the existence of discrepancies between ex-ante and ex-post prices, which set in motion the discovery process within the market.

Knight himself was the main opponent in the second of these debates: the capital theory controversy. Knight's rejection of the Austrian notion of capital heterogeneity became part of the mainstream (see Knight 1944). On the other hand, his pure productivity theory of investment was partly rejected in favour of the loanable fund theory, which combines both productivity and time preference (the foundation of the Austrian theory of interest) as determinants of the equilibrium interest rate (see Hirshleifer 1970).

The final, and, in our opinion, most important of these debates was that regarding the possibility of economic calculation under socialism. Faced by the surprising rejection of Mises' original argument (Mises 1920 [1935]), Hayek decided to investigate the foundations of the economic understanding of the market process, foundations that had been lost in a time of increasing formalisation and focus on statics over dynamics. While it ultimately failed to convince his peers at the time, Hayek's work on knowledge and the price system had a direct influence on the development of market process theory (see Kirzner 1973) and an indirect one on that of information economics and mechanism design (see Boettke and O'Donnell 2013).

Contrary to the opinion of much of the rest of the profession, Hayek believed that his positions were mere extensions of the traditional economic understanding of social phenomena and that he had actually proven his intellectual adversaries' positions to be inconsistent with this corpus of knowledge. Hayek spent the following several decades of his career investigating the causes of this transformation. This brought him to develop his Abuse of Reason Project (see Caldwell 2004), which consisted of several articles written during the 1940s (Hayek 1941, 1942, 1943, 1944b), later published as *The Counter Revolution of Science* (Hayek 1952b), and what would become his most popular piece of work, *The Road to Serfdom* (Hayek 1944a). Here, Hayek explores the intellectual history of what he referred to as 'scientism' or the belief that mankind can amend the supposed failings of its social institutions by substituting reason for tradition.

At the same time as he was criticising the errors of scientism, Hayek was exploring the epistemological foundations of the social sciences (and economics in particular). He came to the realisation that the scientific study of social phenomena deals with the problem of the coordination of independent individual plans, each of which is conceived with only limited and often incorrect knowledge of the circumstances within which they take place (Hayek 1937, 1945). This further led him to investigate more deeply the role that social and economic institutions play in enhancing this coordination. This research produced Hayek's magnum opus, *The Constitution of Liberty* (Hayek 1960 [2011]), and two follow-ups, *Law, Legislation and*

Liberty (Hayek (1973–1979 [2013])) and *The Fatal Conceit* (Hayek 1988). Here, he stresses the fundamental role of the legal and political institutions which have emerged in the Western world since the late Middle Ages in fostering social cooperation under the division of labour and, ultimately, economic development.

Hayek's exploration into the institutional foundations of economic performance came at a time when the rest of the profession was growing increasingly preoccupied with mathematical modelling and general equilibrium (see Boettke 1997). His work was an attempt to reunite economists with their intellectual forefathers, the likes of Hume, Smith and Mill. We believe that this work was not in vain. In the last thirty years, the new institutionalist revolution in industrial organisation, economic history and development economics has radically changed the face of the academic discourse in economics departments. Today, institutions figure as one of the most studied topics by professional economists. Even though Hayek's contribution is, in our humble opinion, still underappreciated, his influence has had profound and lasting effects and is partly responsible for this transformation. The Hayek drama might still have a happy ending after all.

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16

Abba P. Lerner (1903–1982)

Warren Young, Daniel Schiffman and Yaron Zelekha

1 Introduction

Much has been written on the watershed contributions of Abba Lerner to economic theory and policy analysis. Throughout his long and productive career, as academic, economist and policy adviser, Lerner was influenced by, and exhibited, his LSE roots. The aim of this chapter is not to provide a detailed biographical or bibliographical account of Lerner's work on economic theory and policy analysis; this can be found in Colander (1980, 1983), Scitovsky (1984), Landes (1994) and Colander and Landreth (1996). Rather, our focus will be on Lerner's works—books, papers and book reviews that were:

- (i) Published during his tenure at LSE;
- (ii) Directly related to, or included elements of, work he did while at LSE;
- (iii) Directly impacted by what we call his LSE-based 'eclecticism'.

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We delimit our treatment to the above categories as Lerner was such a prolific author that a full account of his work—both directly and indirectly following from the LSE headwaters affecting his weltanschauung and output—covering the span of the half-century, from his first published piece in 1932 to his death in 1982, is beyond the scope and limitations of this chapter.

Section 2 surveys Lerner's academic career at LSE and the immediate aftermath. It surveys his activities and some of the major papers he produced while there, his membership in the 'Robbins Group' and the 'socialist calculation' or 'market socialism' debate between Lerner on the one side, and Dobb, Durbin and Lange, on the other; the development of the 'Lerner-Lange Theorem' or model, over the years 1934–1939, is also briefly discussed. Finally, Lerner's participation in the London–Cambridge–Oxford seminars and his important, albeit scarcely cited, papers presented at the September 1936 and December 1938 Econometric Society meetings are dealt with.

Section 3 focuses on the development, reactions to and impact of what can be considered Lerner's most influential book, *The Economics of Control*, which he started to think about as a research fellow at LSE in 1932 and which became his doctoral dissertation. We also focus on the development of Lerner's notion of 'functional finance', and what we believe is his first public presentation of this idea at the July 1939 Cowles Commission Research Conference. The impact of Lerner's functional finance on Franco Modigliani and the development of his own approach to the 'Keynesian System', what was later called the 'neoclassical synthesis' as manifest in his famous 1944 *Econometrica* paper (Modigliani 1944a), are also dealt with in this section. In this context, the reviews of *The Economics of Control* in the major US, UK and Canadian journals are surveyed, including reviews by three future Nobel Laureates, namely Stigler, Meade and Friedman. Moreover, Lerner extended his eclecticism in what can be called LSE book reviews. These included reviews of such major works as Hicks's *Value and Capital* (Lerner 1940a) and later, Robinson's *Accumulation of Capital* (Lerner 1957), among others. Lerner's review of *Value and Capital* is dealt with in some detail here as Hicks was Lerner's first-year supervisor at LSE and it best represents, in our view, Lerner's LSE-based eclecticism.

Section 4 deals with Lerner's academic and professional activities from the post-war period onwards. The focus here is first on his essay 'Economic Liberalism in the Postwar World' (Lerner 1943a), published in the influential volume, *Postwar Economic Problems*, edited by Seymour Harris. It will be shown that as early as 1943, Lerner discussed the interaction of his notion of

functional finance in the international context of floating exchange rates and the monetary approach to the balance of payments. Lerner's 1951 volume, *Economics of Employment*, based on the possible employment policy applications of functional finance, is also discussed in this section. Lerner's now famous 1952 *Economica* paper, 'Factor Prices and International Trade'—originally presented by Lerner at an LSE seminar in December 1933, while a graduate student there, as recognised by Samuelson (1949)—will also be dealt with here.

We then look at Lerner as a policy adviser, especially during his tenure in the 1950s at the Economic Advisory Staff (EAS) and Ministry of Finance in Israel. Here, once again, his LSE eclecticism comes into focus as he applied his analytical approach to dealing with the economic problems of the new Jewish State, such as inflation, price-wage policy, balance of trade and payments, economic growth, economic independence, and the establishment, structure and functions of the central bank. We then briefly survey Lerner's contributions in the 1960s and 1970s.

In Section 5, we provide a conclusion that sums up, what, in our view, is Lerner's LSE-based life's work.

2 Lerner at LSE

Born in Russia in 1903, Lerner arrived in England with his family as Jewish refugees in 1906, settling in London's East End. Between age 16 and his entry into LSE a decade later, Lerner worked as a machinist, capmaker, type-setter, tailor and salesman of women's shoes; undertook Rabbinical studies; and taught at Hebrew school. He owned a small business that went bankrupt with the onset of the Great Depression, and entered LSE as an adult undergraduate in 1929, at age 26 (see Samuelson (1964) and Landes (1994)).

On his own account, Lerner 'came to the LSE as a socialist, with Marxist inclinations and with some rather grandiose notions of turning bourgeois economics to socialist use' (Lerner 1977a: 235). He was tutored by John Hicks in his first year, 1929, as an undergraduate. He became a Research Fellow in 1932 and stayed on as a Research Fellow, PhD student and Assistant Lecturer until the end of the 1937 academic year, when he left to take up academic positions in the USA, something that lasted until his death in 1982. Lerner's extended relationship with LSE overlapped, for the most part, with what Shackle called *The Years of High Theory* (Shackle 1967). As an LSE undergraduate, Lerner won both prizes and scholarships. In 1929, he was awarded the Tooke Scholarship and in 1932, the Gonner Memorial

and Gladstone Prizes. He was granted LSE Research and University of London Leon Fellowships over the period 1932–1935.

As an LSE academic, Lerner's published contributions showed his LSE-based eclectic approach and ranged from articles on topics in microeconomics, trade theory, the socialist calculation debates and finally, on Keynesian economics, after spending two terms visiting Cambridge (1934–1935). While at LSE, he also founded in October 1933, along with Ursula Webb (later Hicks) and Paul Sweezy, the *Review of Economic Studies* as a publication outlet for graduate students and younger staff members at the School, Cambridge, Oxford and other British universities.

At LSE, Lerner was an active member, albeit an ideological 'outlier', of the 'Robbins Circle'. This eclectic group, which included Robbins, Hayek, Kaldor, Hicks, Shackle, Plant, Ursula Webb, Coase, Allen, Sayers, Rosenstein-Rodan and Lerner, shared a belief in the market economy, although Lerner readily suggested 'market socialism' as a viable alternative to laissez-faire (on this group, see Winch (1969), Howson and Winch (1977) and Wiseman (1985)).

Lerner's work while at LSE, and publications immediately following his tenure there, encompassed:

- (i) The first treatment, in general equilibrium terms, of increasing returns in international trade, specifically the notion of factor price equalisation and the diagrammatic representation of the relationship between goods and factor prices in a trade model; the impact of import and export taxes and the treatment of asymmetry between them; the 'Lerner Paradox' diagram; and an analysis of the 'elasticity of demand' (see Lerner 1932, 1933a, 1934a, 1936a, 1952a);
- (ii) Development of a cogent theory of monopoly; a measure of market power under monopoly: duopoly (Lerner and Singer 1937); the 'Lerner Index' (Lerner 1934b); the first formal—albeit non-mathematical—statement of the First Fundamental Welfare Theorem, later mathematically presented by Lange (1942) and Arrow (1951);
- (iii) The relationship between 'mainstream' economic theory, 'socialist economics' and the notion of socialist statics and dynamics in the form of the 'socialist calculation' or 'market socialism' debates with Lange, Dobb and Durbin, and the development of the theoretically based Lange-Lerner 'theorem' or 'mechanism' (see Lerner 1934c, 1935a, 1936b, 1937a, 1938a, 1939a, 1940b);
- (iv) Following from his conversion to 'Keynesianism', a series of papers outlining his *own* interpretation of Keynes's approach (see Lerner

- 1936c, 1938b, c, d, 1939b, 1940a, 1944a), which, over time, has come to be called ‘Lernerian’ (see Colander 1984). In this context, he also compared what he took as Keynes’s approach to that of the Swedish School (Lerner 1939c, 1940c);
- (v) Flowing from his LSE eclectic training, Lerner was able to apply Hicks’s ‘synthetic approach’—as manifest in *Value and Capital* (Hicks 1939), which Hicks described as being ‘conceived at the London School of Economics’ (ibid.: Preface) and which Lerner reviewed positively (Lerner 1940a)—to produce his own ‘LSE book’, based upon his PhD dissertation, and published in 1944 as *The Economics of Control* (Lerner 1944b), where a variation of the Second Fundamental Welfare Theorem was presented;
 - (vi) Exposition of the notion of functional finance, first, in a presentation at the Cowles Research Seminar in July 1939 and later, in an influential paper in *Social Research* (Lerner 1943b), and in *The Economics of Control* (Lerner 1944b), which dealt with the issue of how to generate adequate aggregate demand, thus ensuring full employment; and this when facing a situation of possible deflation due to insufficient demand projected for the immediate post-war period.

Regarding Lerner’s LSE-based contributions to trade theory, perhaps the most significant was that on factor price equalisation, which only appeared in published form in *Economica* in 1952, albeit having been presented at an LSE seminar in December 1933, as will be discussed below.

Lerner’s 1934 paper ‘The Concept of Monopoly and the Measurement of Monopoly’ is his most widely cited. The paper’s central message relating the social loss due to monopoly to the price–average cost relationship shifted the economics profession’s focus from monopoly profits to the issue of allocative inefficiency. Indeed, as Samuelson observed in his 1964 paper, ‘A.P. Lerner at Sixty’, Lerner’s approach, while seemingly ‘simple’ (Samuelson 1964: 173), was indeed profound. Moreover, as Scitovsky put it in his 1984 survey, ‘Lerner’s Contributions to Economics’, Lerner’s 1934 insight, in which social loss resulting from monopoly identified the ‘social optimum’ with a state of Pareto-type optimality, was the ‘first clear, rigorous and definitive statement of Pareto optimality’ (Scitovsky 1984: 1551). Moreover, over 75 years after his treatment of monopoly, Blaug and Lloyd (2010: 16) referred to Lerner as one of the leaders in economic theory and analysis as he originated ‘The Lerner degree of monopoly...and its diagram’.

The ‘socialist calculation’ or ‘market socialism’ debate between Lerner and his LSE colleagues and others has been dealt with by Lavoie (1985),

besides many others, and will not be surveyed here. Suffice it to say that Lerner's argument, seen especially in his 1937 paper 'Statics and Dynamics in Socialist Economics', resulted in a revision of Lange's position on the issues, as manifest in the rewritten versions of his 1936 and 1937 papers as published in the 1938 volume on the economic theory of socialism edited by Lippincott (1938). This, in turn, led to the conflation of their respective views, resulting in what is now called the 'Lange-Lerner Theorem' or model, that provides a neoclassical-type foundation of a hypothetical socialist economy contingent upon a combination of public ownership of factors of production along with a Central Planning Board, based, in turn, upon a 'trial and error approach', so as to simultaneously determine targets relating to output, equilibrium and Pareto-efficiency. Indeed, Lerner showed his affection for the price mechanism in the conclusion to his 1937 paper when he wrote: 'Price must be made equal to marginal cost. This is the contribution that pure economic theory has to make to the building up of a Socialist economy' (Lerner 1937a: 270).

Over the period October 1933–1935, Lerner attended LSE–Cambridge–Oxford joint seminars. They were also attended, from time to time, by senior figures such as Robbins and Hayek from LSE, and Keynes from Cambridge. For the most part, however, they provided a meeting place for the cross-fertilisation of ideas between younger LSE academics such as Lerner, Hicks, Ursula Webb, Kaldor, Rosenstein-Rodan, Sayers, Maurice Allen, Roy Allen, Durbin and Gaitskell; Oxford economists, notably Harrod and Meade; and Cambridge followers and colleagues of Keynes, including Kahn, Austin and Joan Robinson, and Sraffa. Academic visitors to LSE, Cambridge and Oxford also occasionally attended these seminars. Lerner at first accepted the multiplier arguments of Kahn and Meade, although he only became a full-blown Keynesian after spending the Michaelmas and Lent Terms of 1934/1934 at Cambridge.

However, while others, such as Hicks, Harrod and Meade, were busy in late 1936 'Interpreting Mr Keynes', Lerner—having by then written a very favourable review of *The General Theory* that appeared in the October 1936 issue of the *International Labour Review* (Lerner 1936c)—also presented a paper at the now-legendary September 1936 Econometric Society meeting held at Oxford at which Hicks and Meade gave interpretations of Keynes's approach in the form of their respective 'IS-LM papers' (see Young 1987). Lerner, in his paper given on Sunday afternoon, 27 September 1936, however, did not deal with Keynes of *The General Theory*. Rather, he gave a survey of recent work on trade theory entitled 'International Trade and

Transfer'. As it has not been dealt with until now in the literature, we cite at length from the *Econometrica* meeting report summary of his paper (Lerner 1937b: 371–372):

Recent writers on International Trade have been troubled by the difficulty of fitting the theory of the subject into the general framework of economic doctrine, and this is reflected in the whole tenor of their work. The difficulty arises from the peculiar historical development of International Trade Theory. An initial precocity at the beginning of the nineteenth century, when the Law of Comparative Costs partly freed it from the labour theory of value, put it ahead of the other branches of economics, but, not unnaturally, was followed by a retardation of further development while the rest of economic theory achieved a more complete emancipation. To this situation some writers, like Ohlin and Iversen, react by rejecting the whole of the classical structure and building anew an Interdependence Theory of international trade on Walrasian lines. Others, like Haberler and Viner and (more intensively) Harrod, defend the classical real cost analysis by a process of benevolent interpretation. Ohlin appears to be too much enamoured of the Walrasian scheme to be willing to admit that real (if incomplete) results can be obtained only by partial analysis. Instead of passing from a Walrasian preface to a body of particular analyses, he tries to deal with everything. The result is an exercise in one-thing-at-a-time analysis attempting to be everything-together analysis by jumping very quickly from one thing to another. Ohlin makes some concessions to the need for partial analysis by simplifying the whole scheme somewhat. In doing this he falls between two stools, achieving neither complete generality nor realistic particular results, and arrives at some false conclusions in the theory of the equalisation of factor prices by international commodity movements. The main merit of his method lies in its suggestiveness of interconnections that might be overlooked ... In the theory of Transfer, the use of the concept of the transfer of buying power developed by Ohlin (following Bastable and in a sense Ricardo) and the deprecation of the discussion of elasticities of demand, is [a] dangerous procedure. It is possible by a fairly simple diagrammatic scheme to show that the terms of trade will move in favour of or against the paying country according as the elasticities of supply of exports and imports are greater or less than the elasticities of demand, all measured in terms of domestic goods. Because of the possibility of the movement of factors of production between domestic and export goods but not between domestic and import goods, there is a presumption that the elasticities of supply are greater, so that the terms of trade are more likely to move against the paying country. Discussion on this topic has been confused by the entry of irrelevant problems and prejudices ... Harrod keeps to a real-cost analysis in order to be able to discuss the gain from international trade. It can be shown that all his results are obtainable without the objectionable and difficult real-cost analysis, and can be demonstrated

much more simply by the use of the indifference-curve technique ... Another important case of a traditional bias is the belief that taxing the foreigner by import or export duties is a practical impossibility. Haberler makes great use of this. But the application of the theory of monopoly gain shows that there is an ideal tax on every import or export commodity of not less than the inverse of the elasticity of supply or demand from the point of view of the country ... This becomes negligible only if all elasticities are of a very high order, an assumption for which there appears to be no basis.

Two years later, at the Econometric Society meeting in Detroit in late December 1938, Lerner turned to Keynes's theories in the context of the paper he gave entitled 'Equilibrium and Dynamic Concepts in the Theory of Employment', a summary of which appeared in the report of the 1938 meeting in *Econometrica* (Lerner 1939d). As this paper has also received no attention in the literature, we cite from its summary at length (*ibid.*: 186–187):

Mr. Lerner said that the recently developed theory of employment associated with Mr. Keynes can be usefully described as more dynamic than the "classical" theories of equilibrium that left no place for unemployment. It is, however, incorrect to suppose that this is because it brings in expectations in using concepts like the marginal efficiency of capital rather than the marginal productivity of capital. Expectations were always fundamental in "classical" economic theory and what distinguishes Mr. Keynes' marginal efficiency of capital is rather the stress on the rate of investment instead of on the quantity of capital in clarifying the dimensions involved, so that it should really have been called the marginal efficiency of investment. The theory is more dynamic in that it develops a shorter-period analysis and so enables a path to be investigated that may or may not lead toward the long-period equilibrium position. The path consists of positions of short-period equilibrium, the determinants of which change in the longer period. The theory is not as dynamic as some would wish because it assumes the short-period equilibrium to be reached, and a failure to recognise the (short-period) equilibrium nature of the analysis has been responsible for unhelpful criticisms of concepts like the multiplier or the liquidity-preference schedule. It would indeed be better to have a more dynamic analysis (which would have to run in terms of a still-shorter-period equilibrium) but such analysis has not yet been developed in usable form. Dynamic paths are inevitably discontinuous, consisting of points where some shorter-period equilibrium or adjustment is reached. They should not be confused with planned paths which can be conceived of as strictly continuous because the discovery and correction of error does not enter. However, Harrod and Kalecki have been able to develop other dynamic aspects of the theory of employment.

In a paper published in the *American Economic Review* in 1939 entitled ‘The Relation of Wage Policies and Price Policies’, still identifying him as being at LSE, Lerner wrote (1939e: 158):

I shall consider wage policies and price policies from the point of view of the economy as a whole and not from that of either a particular firm or that of any particular section of the economy. The policies are conceived to be directed to the object of achieving and maintaining the prosperity of the economy as a whole. The main difficulty of this problem lies in the danger of taking propositions that have been established as true when applied to sections of the economy and illegitimately applying them to the economy as a whole. What is true of a firm or of a particular industry or of a set of industries need not be true of the economy as a whole. To draw attention continually to such relationships between the parts and the whole is probably the most distinctive function of the economist.

A focus on wages, prices and employment became an important theme in Lerner’s writings thereafter.

3 ***The Economics of Control and Functional Finance, 1932–1944: Advocacy, Critiques and Impact***

The eclectic nature of Lerner’s approach is seen in the Preface to *The Economics of Control*:

This study was started about twelve years ago while I was a student at the London School of Economics. During this time, it has changed in scope and direction. Originally it was to be a development of the theory of the price mechanism of a socialist society ... In the course of the development of my ideas on the subject, while this work was continually being interrupted by other tasks, it gradually became clear to me that the maintenance and further development of the democratic way of life, as it grew under capitalism and was extended by the labor movement within the capitalist society, not only formed a far more essential part of the socialist ideal than the negative “abolition of private property in the instruments of production” but was in much greater need of careful tending ... If socialism is to be identified with the belief that the abolition of private property would automatically establish the brotherhood of man – and many socialists did, while some apparently still do, believe this – then socialism must be counted out as false. State control or ownership of the instruments of production where the State itself is not thoroughly

democratic is not socialism and is much further removed from socialism than socialism's "opposite", capitalism (Lerner 1944b: vii).

Lerner went on to say that:

The title *Economics of Control* was proposed in 1932, with the idea that the principles of the price mechanism would also be applicable to nonsocialist but autocratic collectivist societies. The name is perhaps even more appropriate for the present form of the book, now that the stress is taken from collectivism and applied to the idea of conscious recognition of the problems of social organization and the exercise of conscious control over the economic system (ibid.: vii–viii).

He continued:

I do not think I ever was guilty of raising collectivism from a means of bringing about the socialist ideals to an *end* in itself, but, like many socialists, I tended to overemphasize its importance. The economics of *control* is still contrasted with the economics of *laissez faire*, but control does not necessarily mean collectivism. It suggests the deliberate application of whatever policy will best serve the social interest, without prejudging the issue between collective ownership and administration or some form of private enterprise (italics in original).

In my original plan I had intended to provide a theoretical solution for each economic problem of a completely collectivized economy and then see to what extent, if at all, and by what means the problem is in fact solved by a capitalist society. But the abandonment of the dogmatically, and therefore completely, collectivist economy as identical with the ideal of a society organized in the social interest still permits a similar procedure to be followed with slight modifications.

It is almost impossible for me to say now exactly in what respects this work shows true originality. Most of it doubtless was absorbed from my teachers at the London School of Economics (ibid.: viii; italics added).

Among the personalities he acknowledged in the Preface (ibid.) were his LSE teachers, namely Robbins, Hayek, Hicks and Robertson, for their 'original training in handling the tools of economic analysis'. He also thanked Plant and Hutt—'right-wing' personalities—'for their special insistence, long resisted' by Lerner, 'on the possibility of approaching social problems' from the 'free enterprise starting point'. He then thanked two 'Socialist' personalities, Laski and Dobb, for 'helping to direct' him to the topic. He went on to thank Kahn and Joan Robinson 'for the great pains they took in getting me to overcome my prejudices against

Mr. Keynes's great advancement of economic understanding'. He also thanked Kalecki and Lange for their 'criticisms' and 'reminders of the larger problems of social organisation into which economic issues have to be fitted'. Finally, he thanked the readers of his manuscript—Devine, an accounting expert and economist at the University of Minnesota, and Stigler, later a Nobel Laureate.

As noted above, in *The Economics of Control*, Lerner gave a version of the Second Fundamental Welfare Theorem. In doing this, however, Lerner made the direct link between pure theory and policy (see Colander 2004: 15). The welfare maximising 'rules' developed by Lerner (1941, 1944b) and Lange (1942) became, as Colander noted (ibid.: 15) 'the templates for the textbook presentation of both micro and macro policy discussions'.

Lerner's notion of functional finance has been dealt with by many observers, both critical and supportive (see Bell (1999), among many others). All accounts of the origins of functional finance as presented in *The Economics of Control* date it to Lerner's June 1941 article 'The Economic Steering Wheel' and more widely cited February 1943 paper in *Social Research*, entitled 'Functional Finance and the Federal Debt'. In the latter paper, he both outlined his approach and then presented what he called the 'first' and 'second' laws of functional finance. As Lerner put it:

The central idea is that government fiscal policy, its spending and taxing, its borrowing and repayment of loans, its issue of new money and its withdrawal of money, shall be undertaken with an eye only to the *results* of these actions on the economy and not to any established traditional doctrine about what is sound or unsound. This principle of judging only by *effects* has been applied in many other fields of human activity ... The principle of judging fiscal measures by the way they work or function in the economy we may call *Functional Finance* (Lerner 1943b: 39; italics in original).

Lerner went on to say that the '*first law*' of functional finance is that 'total spending can be kept at the required level, where it will be enough to buy the goods that can be produced by all who want to work, and yet not enough to bring inflation by demanding (at current prices) *more* than can be produced' (ibid.: 40; italics in original).

Regarding the '*second law*', Lerner wrote:

The second law of Functional Finance is that the government should borrow money only if it is desirable that the public should have less money and more government bonds, for these are the *effects* of government borrowing ... When taxing, spending, borrowing and lending (or repaying loans) are governed by

the principles of Functional Finance, any excess of money outlays over money revenues, if it cannot be met out of money hoards, must be met by printing new money, and any excess of revenues over outlays can be destroyed or used to replenish hoards (*ibid.*: 40–41; italics in original).

Thus, as Lerner put it:

In brief, Functional Finance rejects completely the traditional doctrines of “sound finance” and the principle of trying to balance the budget over a solar year or any other arbitrary period. In their place it prescribes: first, the adjustment of total spending (by everybody in the economy, including the government) in order to eliminate both unemployment and inflation, using government spending when total spending is too low and taxation when total spending is too high; second, the adjustment of public holdings of money and of government bonds, by government borrowing or debt repayment, in order to achieve the rate of interest which results in the most desirable level of investment; and third, the printing, hoarding, or destruction of money as needed for carrying out the first two parts of the program (*ibid.*: 41).

At this point we present what we believe is the first public exposition of Lerner’s notion of functional finance, that is, in his paper entitled ‘Budgetary Principles’, which he gave on Tuesday 11 July 1939 at the Cowles Research Conference held at Colorado College. Below, we cite at length from the abstract of the paper, as it appeared in the conference proceedings (Lerner 1939f: 38–39; italics in original):

It is necessary for a department of the government or of any other organization to limit its expenditures to its budgeted allowances because that is the mechanism whereby the activities of the department are coordinated into the general plan of the whole organization ... These considerations do not apply to the sovereign government of a well-established modern state. All talk of the necessity, propriety, or usefulness of a government balancing its budget, whether weekly, annually, cyclically, or over any other period is nothing but an irrational transference to general public finance of the principles that are appropriate only for departmental finance or private finance.

The government should spend money or subsidise expenditure by others wherever that is necessary to provide the effective demand for adequate employment or where a particular expenditure is in the public interest. It should tax away income wherever this is necessary to prevent too great an effective demand which would disorganise the economy through inflation or where a particular form of expenditure or income is considered to be socially undesirable. It should regulate its borrowing and lending of money entirely by the principle of keeping the rate of interest at a level that gives the ideal rate of

profitable investment; hoarding or destroying the money that it receives; and dishoarding or printing any money it may need in carrying out the policy.

Although in this formulation of fiscal principles there is no room for any principle of attempting to make total revenue from taxation equal to total expenditure over any period of time, the idea of ‘balance’ is not eliminated. Rather, it is refined and preserved in the sense of a nice adjustment between expenditure and taxation so as to equalise their marginal significance for social welfare. The balance is only *marginal*; so that an inequality between total revenue and total expenditure, if it should be reached (and there may be a tendency for it to be reached over the very long period), would be only an accidental result of policies framed in the light of other principles.

It is thus clearly evident, in our view, that Lerner publicly presented his idea of functional finance for the first time to an audience at the July 1939 Cowles Research Conference, which included, among others, his future colleague at the New School, Marschak, and also Ezekiel, Haavelmo, Roos, Tintner, Triffin, Wald and Yntema, a distinguished group of mathematical economists and econometricians indeed. Interestingly enough, Lerner appeared on the Conference Program and at the head of the abstract as published in its proceedings as being an ‘Assistant Lecturer in Statistics’ at LSE (Lerner 1939f: 38). Whether or not he convinced this group of the efficacy of his idea, Lerner still proceeded to develop and expand on it in subsequent articles (see Lerner 1941, 1943a, b), and it comprised a major ‘talking point’ regarding *The Economics of Control* and its impact. Keynes’s view of functional finance was broadly positive. Thus, in a letter to Lerner from September 1944, Keynes saw merit and originality in Lerner’s idea, saying that he would try to introduce it to the UK Treasury mandarins (see Keynes to Lerner quoted in Colander and Landreth 1996: 116).

What is important to recall here is that the impact of Lerner’s notion of functional finance went far beyond the efficacy of fiscal policy and was one of the factors that catalysed what came to be known as the ‘neoclassical synthesis’. For, in our view, perhaps the most significant impact of the notion of functional finance was upon the ideas of Lerner’s PhD student at the New School, Franco Modigliani. According to Modigliani, the impetus for his own seminal January 1944 paper ‘Liquidity Preference and the Theory of Interest and Money’—based on a chapter in his PhD thesis entitled ‘The General Theory of Employment, Interest and Money Under the Assumptions of Flexible Prices and of Fixed Prices’ (Modigliani 1944b), published before the thesis was submitted—was his arguments with Lerner. Indeed, Modigliani clearly recalled his ‘debate’ with Lerner regarding the nature of both the ‘Keynesian Revolution’ and functional finance. It should

be recalled here that Lerner replaced Marschak as Modigliani's doctoral supervisor after Marschak left the New School for Columbia University. As Modigliani put it in his *Macroeconomic Dynamics* interview with Barnett and Solow (2000: 225):

Functional finance led me to the 1944 article. In functional finance, only fiscal policy could have an impact on aggregate demand. Therefore, it was an economy that belonged to what I later called the Keynesian case. I tried to argue with Lerner and to have him understand that Keynes did not say that. This was the origin of the 1944 article, trying to put Keynes in perspective.

Due to its wide scope, *The Economics of Control* was reviewed in most of the major American, British and Canadian journals in both economics and political science. The reviews varied in their assessments. One of the first appeared in the December 1944 issue of the *American Economic Review*. This review essay, by the British economist and collaborator of Kaldor, M.F.W. Joseph, was positive for the most part. She concluded however that, 'In addition to the Economics of Control we shall require a companion volume on the Politics of Control that will show us how to secure and maintain the wise and benevolent government which will act upon—or even read—the principles expounded by Prof. Lerner' (Joseph 1944: 887). Meanwhile, in the January 1945 issue of the *Annals of the American Academy of Political and Social Science*, Hoch (1945: 223–224) was somewhat critical of Lerner's method of presentation, calling it 'far from inviting', unclear 'in some instances', and 'sometimes' merely 'creating a new language for old concepts'. He concluded that 'The general reader will find a good summary of Lerner's views minus the mental worksheets' in Lerner's essay 'Economic Liberalism in the Postwar World', an essay which we will discuss below.

In the March 1945 issue of *Political Science Quarterly*, Stigler reviewed Lerner's volume (Stigler 1945). Stigler's assessment was mixed. On the one hand, regarding theory, he wrote that Lerner's 'exposition of pure theory' was 'lucid and penetrating', and his discussion of it 'rigorous and consistent with the accepted doctrine', his overall treatment being 'first rate' (ibid.: 113). On the other hand, when dealing with policy, Stigler asserted that Lerner's views were inconsistent, moving between stability and possible instability of a competitive price system (ibid.: 114). Stigler went on to criticise 'Lerner's well-known scheme of functional finance'. According to him, while having 'attractive simplicity', it avoided 'real problems' (ibid.: 115). Stigler wrote that in the functional finance approach, 'the ability of the central monetary authority to avoid both unemployment and inflation is not demonstrated. What statistical indices will guide the authority's policies, and

precisely how will they be followed? How is unemployment in a variety of industries, but not in business generally, to be eliminated by fiscal means?’ (ibid.).

The next month, in the April 1945 issue of the *Economic Journal*, Meade, also a future Nobel Laureate, reviewed Lerner’s ‘LSE book’. In what was essentially a review essay of over twenty pages, Meade focused on the welfare economics aspects of Lerner’s tome. Meade’s review was overall positive. He wrote that Lerner’s approach constituted a ‘third school of thought’ as against the ‘battle royal between Planning and Laissez-faire’. As he put it:

Mr. Lerner believes passionately in the principles of the economic calculus, in the use of the price mechanism, in the avoidance of arbitrary centralised planning, and in freedom of choice for consumers and workers; but he does not believe in unqualified laissez faire. He preaches the “controlled economy” by which he means an economic system in which the price mechanism is made to work at the dictation of the free choice of the individual consumer, in such a way as to attract factors of production to the uses in which the valuation set by the consumers on their marginal product is higher. In many cases, according to Mr. Lerner, this can best be achieved by competition; in other cases it is necessary to institute socialist production to achieve this end by equating prices to marginal costs. The “controlled economy” is the economy in which controls are introduced of a kind and on a scale necessary to achieve just this object of making the price system work, and from which all other regulations are removed (Meade 1945: 47–48).

It is not surprising that in an earlier diary entry for 28 January 1945, Meade had described Lerner as ‘a real Liberal-Socialist-Welfare-Marginalist economist’ (Meade 1945 [1990]: 37–38).

Timlin’s review essay appeared in the May 1945 issue of the *Canadian Journal of Economics and Political Science*. She recognised the possible application of Lerner’s ‘Rule’ regarding marginal benefits and costs, and marginal values of factors to centralised or decentralised collectivist societies on the one hand, and ‘the controlled economy’ or ‘perfect competition’ on the other (Timlin 1945: 286–289). She also noted, significantly, that ‘*the controlled economy will adopt an eclectic position* (italics added) always looking to the *end* (italics in original)’ in this regard (ibid.: 287).

Milton Friedman, a third future Nobel Laureate, for his part, also reviewed Lerner’s ‘LSE book’ in the October 1947 issue of the *Journal of Political Economy* (Friedman 1947). Friedman’s review, as Stigler’s, was mixed. Friedman asserted, in line with Meade, that Lerner’s book was essentially ‘an analysis of the problem of maximising economic welfare’ (ibid.: 405). In his view, Lerner’s notion of functional finance was one of the key

ideas in the book. However, Friedman took issue with Lerner's proposal that functional finance would be able to 'handle the problem of maintaining adequate aggregate demand' (ibid.: 412). Friedman maintained that while Lerner's 'discussion of "functional finance"' was 'a brilliant exercise in logic...the relevant question was whether the discussion of "functional finance", besides being a logical exercise, is also a prescription for public policy'. Friedman's answer was 'clearly negative'. He went on to say that 'to make' functional finance 'into a prescription' to generate 'full employment', Lerner should have provided a method by which to ascertain the timing of 'insufficient total demand', whether it was 'a temporary deficiency', was 'in the process of being corrected', or if the state of the economy was at the 'beginning of an increasing deficiency that, if left alone, would [lead] to drastic deflation' (ibid.: 413). Friedman then asserted that Lerner 'must tell us how to know what medicine to use when a diagnosis has been made, how large a dose to give, and how long we may expect it take for the medicine to be effective' (ibid.). Friedman concluded in a mixed tone:

The proposals in the book have considerable suggestive value and may stimulate others to useful and important work in developing them. The book throughout reveals Lerner's very considerable gifts – his acuteness as a theoretician and dialectician, his skill and patience in exposition, his flexibility of mind, his profound interest in social welfare, and his willingness to accept and courage to state what seems to him right social policy, regardless of precedent or accepted opinion. In the reviewer's judgement, however, these gifts have been imperfectly realized because they have been employed in a vacuum and have not been combined with a realistic appraisal of the administrative problems of economic institutions or of their social and political implications (ibid.: 416).

In his 1960 essay, 'Economics and the Control of Man', Lerner retrospectively wrote:

It is hard for me to remember, but it is possible that in 1932, when I first thought of writing *The Economics of Control*, I too had a prejudice favoring regulation by authorities. Or there may be other explanations of my having chosen the word "control". Anyway, over many years, in the course of learning more economics, and writing the book, the meaning of the word changed subtly, and it was softened. "Control" came to mean to me not commands to people to act in a certain way, but institutions that caused men to behave in a desired way without feeling that they were being deprived of their freedom, and indeed, without their really being deprived of it. The great paradox is rather that the economist was concerned with understanding the operation

of institutions that caused men freely to behave in such ways as to achieve the end of increasing every individual's opportunities for doing what he preferred or getting what he wanted. There are situations in which people can be forced to be free, such as those in which laws command them to keep to the right of the road so that they are free to drive with much more safety. The economic institutions are those by which people are not forced but induced to do of their own free will what is needed to guarantee freedom in general.

In recent decades, such pious worship of laissez-faire has become less and less possible as the responsibility of government for more and more social objectives has come to be taken for granted. The alleviation of poverty, the provision of public health services, the furnishing of education for all on higher and higher levels, the responsibility for the availability of more elaborate and more essential communication services, and the massive equalisation of income and of wealth by progressive taxation, all go to make up what might be called the revolution of our time, were not the word usually reserved for more spectacular but less important changes in the life of man in society. And now so vast has become the government's necessary expenditures for defence, in addition to all these other activities, that it is almost impossible, but alas not quite impossible, for people in authority to be unaware of the way in which the financial activities of the government impinge on the economy. The government cannot evade the responsibility for so directing these programmes as to keep the totality of economic activities in the country from generating the evils of inflation on the one hand, and of depression, or perhaps I should say recession, on the other (Lerner 1960a: 378–379, 381–383).

Lerner also reviewed many books through the lens of his LSE-based eclecticism. Perhaps the best example of this is his review of Hicks's *Value and Capital* entitled 'Professor Hicks' Dynamics' published in 1940, some three years after he had left LSE for the United States. Indeed, if *Value and Capital* is an 'LSE book', then Lerner's review of it is essentially an 'LSE review'. According to Lerner (1940a: 298), calling *Value and Capital* 'the most important publication for economic theory since the appearance of Keynes's *General Theory* does not quite do it justice'. He continued:

For not only do some of the important "Keynesian" results, reached independently and earlier by Professor Hicks, appear in their final form in this volume, but the elegance and precision with which fundamental notions are presented and the astonishingly simple way in which the intricate argument unfolds itself makes it certain that the book will remain a classic for students to read and re-read long after Mr. Keynes' book has been rendered obsolete by a more careful presentation of its argument at the hands of other writers (ibid.).

Lerner's review then was very positive—as against the majority of reviews (see Young 1991)—but this is not surprising. For as Harrod put it in his own review of *Value and Capital* (Harrod 1939: 294; italics added): '[O]ne is reminded of "a sort of social process which went on among the people who worked there [LSE], at that time, under the leadership of Professor Robbins" (Preface). There are probably some things in this book the full value of which can only be appreciated by members of *that society*'. And Lerner, while a 'Keynesian', was also a member of '*that society*', that is a former LSE student and staff member, present, in spirit at least, at the 'creation' of *Value and Capital* itself.

4 Academic and Professional Activities: Post-war to 1982

One of Lerner's lesser known contributions, in which his notion of functional finance played a major part, was his essay 'Economic Liberalism in the Postwar World' (Lerner 1943a) which appeared in the volume *Postwar Economic Problems*. In this essay, Lerner popularised the approach he had taken in his paper on functional finance published in *Social Research*, dealt with above. As a result of what we consider to be its importance in the dissemination of Lerner's LSE-based eclectic approach regarding international macroeconomic policy, we cite from 'Economic Liberalism in the Postwar World' at length. Moreover, the essay itself contains prescient statements regarding flexible exchange rates and the monetary approach to the balance of payments, as will be seen below.

Lerner defined what he saw as 'Economic Liberalism' in concise and lucid terms. He wrote (Lerner 1943a: 127–128):

First, it must be emphasized that Economic Liberalism does not now mean *laissez faire*. *Laissez faire* is only a means for the achievement of the ends of Economic Liberalism. It works only in special circumstances and does not always deliver the goods ... Economic Liberalism aims, by setting up the appropriate institutions, to maximize the freedom of each individual member of society to satisfy his own desires wherever this does not interfere with the freedom of other individuals ... Unfortunately, *laissez faire* does not always result in perfect competition.

He went on (*ibid.*: 132–133):

The first condition for the survival of Economic Liberalism...can be achieved by making it the *primary function* of government finance to keep the level of monetary demand for goods and services in every country sufficient to give employment to all who seek it out and yet not more than sufficient – because that would result in inflation ... The first casualty is the principle that over any fiscal year the government must spend no more than it collects in taxes. *The second is any international monetary system that involves the maintenance of fixed rates of exchange between the currencies of different countries* (italics added).

The maintenance of adequate monetary demand could be reconciled with fixed exchange rates if the domestic prices were indefinitely flexible. Any change in the international balance of a country could then be met by adjustments of the domestic price level. But, unfortunately, domestic prices are not flexible.

Economic Liberalism will, of course, do its utmost to remove barriers, but wherever it does not succeed in establishing really effective freedom of movement, fixity of exchanges works unnecessary hardship; and where there is real mobility of labour, it will not be necessary for the exchanges to be fixed by law. There will then be a natural stability through the movement of labour which equalises wages and costs. Stability of exchanges is a *symptom* of the success of Economic Liberalism in making real mobility of goods and of labour effective.

Another difficulty that will have to be overcome before freedom of the exchanges to move is recognised as a part of true Economic Liberalism is the common feeling that, when a country permits its exchanges to depreciate, it gains an advantage at the expense of its rivals—that is a form of economic welfare. This is true in a state of world depression. Any country that depreciates its exchanges will thereby increase its employment at the expense of the other countries.

If a country disregards the foreign value of its currency and increases effective demand at home (to a level which gives it full employment), the increased demand for imports by the newly employed will cause the country's currency to depreciate to the point where the higher prices of imports and exports (in the depreciated domestic currency) have sufficiently discouraged imports and encouraged exports to make them equal to each other again. There is no export balance and the other countries are not harmed. The depreciated exchange is not the *cause* of an increase in employment at the expense of the other countries but a *result* of an increase in economic activity that does not affect the other countries. If all countries completely disregard the effect on their foreign exchanges and create enough effective demand in their domestic markets to give full employment at home, they will all gain in employment, there will be no general depreciation of the exchanges (which by definition is impossible) and international trade will not be hampered in any way.

When full employment has been achieved all-round, it will not be possible for any country to gain at the expense of others by artificially reducing the value of its foreign exchange ... In no case does the country benefit itself or harm others by depreciating its exchange. The only rule needed for adequate stability of foreign exchanges is that each country shall maintain full employment at home (*italics in original*).

Here, then, in the space of a few pages, Lerner gave the outline of what can be called the LSE-inspired eclectic 'Lernerian' policy framework for national and international stabilisation in the post-war world based upon the principles of: (i) Pareto-optimality; (ii) functional finance; and (iii) flexibility of exchange rates; a balance of payments adjustment mechanism similar to the so-called monetary approach; and factor price equalisation, following from Lerner's own 1933 method, as will be seen below.

Lerner's writings concerning post-war economic issues dealt with, among other problems, spending, debt and taxation, planning and full employment, the marginal product of capital and marginal efficiency of investment, money and interest, and monetary and fiscal policies (Lerner 1945a, b, c, 1946a, b, c, 1947, 1948a, b, 1949a, b, 1952b, 1953a, 1959a). In 1953, Lerner published a collection of his papers under the title *Essays in Economic Analysis* (Lerner 1953b). Interestingly, he also focused on the problems brought about by the atomic bomb and its control, and economic aspects of atomic energy, in a series of articles in the *Bulletin of the Atomic Scientists* (Lerner 1946d, 1949c, 1953c).

In 1949, Lerner worked as a consultant at the RAND Corporation, and over the period 1949–1952, a series of working papers and reports emanating from his projects were circulated on the topic of pricing, costing and replacement policies (Lerner 1942, 1949d). During this time, he also continued publishing on employment, inflation and wages, interest and money, capital and investment and social choice (Lerner 1949e, 1951a, b, c, 1953a, c, d). In 1951, Lerner published his second book, *Economics of Employment*, which both restated and emphasised the policy implications and the efficacy, in his view, of functional finance in the effort to attain full employment.

However, perhaps his most important LSE-related paper published in the early post-war period was 'Factor Prices and International Trade', which appeared in the February 1952 issue of *Economica*, but was first presented by Lerner at an LSE seminar in December 1933. Briefly put, in the June 1948 issue of the *Economic Journal*, Samuelson published what became a widely cited paper entitled 'International Trade and the Equalisation of Factor Prices'. A year later, in the June 1949 issue of the *Economic Journal*,

he published another paper, this time entitled ‘International Factor-Price Equalisation Once Again’, as the topic, in his view, needed ‘further amplification’ (Samuelson 1949: 181). In the first footnote to this paper, Samuelson wrote (*ibid.*: 1, fn. 1): ‘I learn from Professor Lionel Robbins that A.P. Lerner, while a student at L.S.E., dealt with this problem. I have had a chance to look over Lerner’s mimeographed report, dated December 1933, and it is a masterly, definitive treatment of the question, difficulties and all’. Meanwhile, in the February 1952 issue of *Economica*, the lead article was Lerner’s 1933 seminar paper ‘Factor Prices and International Trade’. As the editors wrote:

The following article was prepared by Professor Lerner for a seminar at the London School of Economics in December 1933. It is the paper referred to by Professor Samuelson in his article, “International Factor-Price Equalisation Once Again”, *Economic Journal*, June 1949, p. 181. It is reproduced here as it was originally written; and it will be of value to students both for its place in the history of ideas, and also for the geometric technique employed to demonstrate conditions for the equalization of factor prices (editors’ introductory note to Lerner 1952a: 1).

Regarding his role as policy adviser, and the impact of his LSE training, this is best seen during Lerner’s tenure in Israel as a member of the EAS and as an adviser to the Ministry of Finance over the period 1953–1956. Here, Lerner applied his LSE-based eclectic approach to deal with problems including inflation and wages, balance of trade and payments, growth, and the structure and functions of the central bank. Indeed, despite his ‘socialist leanings’, Lerner even came out against the cost of living allowance supported by the Israel General Federation of Trade Unions, as he saw it as a cause of wage inflation beyond productivity gains. His advice, however, was only applied in a limited number of cases, as the electoral interests of his political masters took precedence over his sage pronouncements. (For a detailed treatment of Lerner as a policy adviser in Israel, see Schiffman et al. 2017.) Lerner’s ongoing relationship with Israeli economics and economists continued in the 1960s and 1970s, when he visited and worked at Tel Aviv University, and published two books with Haim Ben-Shahar on problems of economic efficiency and growth (Lerner and Ben-Shahar 1969, 1975).

Returning to the USA in the late 1950s, Lerner taught at Roosevelt, Columbia, Johns Hopkins, Michigan State, Berkeley, Queens College and Florida State. His activities included Congressional testimony regarding wage, price and inflation policies, and the role of the Federal Reserve

(Lerner 1958a, b, c, 1959b, 1963a, 1964a); articles on Keynesian economics (Lerner 1960b, c, 1961a, 1964b, 1974a); his approaches to debt, interest, asset values, liquidity, demand and inflation (Lerner 1961b, c, 1962a, b, c, d, 1963a, 1974b, c); his views on financial institutions, monetary and capital theory (Lerner 1963b, 1965); and public utility regulation (Lerner 1964c). In December 1966, Lerner gave the American Economic Association's invited Ely Lecture which he called 'Employment Theory and Employment Policy', published in the *American Economic Review* in May 1967.

In the 1960s and early 1970s, Lerner's economic writings also included essays on consumer surplus and consumer sovereignty (Lerner 1963c, 1970, 1972a). After OPEC I (1973), and again after OPEC II (1979), he wrote on anti-inflation and anti-stagflation policies (Lerner 1975, 1976a, 1977b, 1978a, 1979a, 1980a and Lerner and Colander 1980). Lerner also wrote about the economics of pollution and the environment (Lerner 1972b, 1974d, 1977c). He provided retrospective views of his life as a 'Keynesian' and 'Post-Keynesian' and the issues involved in a series of papers (Lerner 1976b, 1977d, 1978b, c, 1979b, 1980b). Finally, he also dealt with his time at LSE in his February 1977 paper 'Marginal Cost Pricing in the 1930s' (Lerner 1977a).

5 Conclusion

It is not an easy task to sum up the contributions of an economic polymath such as Abba Lerner. Perhaps it is best to leave this to two other giants of the economics profession, Samuelson and Scitovsky. In the June 1964 issue of the *Review of Economic Studies*, Samuelson surveyed the scope of Lerner's wide-ranging contributions to economics—micro, macro, welfare, international—in his retrospective, 'A.P. Lerner at Sixty'. Samuelson wrote (1964: 169): 'Abba Lerner has been a great theoretical economist in a vintage epoch for theorists. This last third of a century he has poured out one brilliant paper after another—in micro theory and macro, in pure thought and in the realms of policy'. In his survey of Lerner's research, Samuelson stressed the originality of Lerner's work written while at LSE, and how Lerner applied the analytical toolkit he had been taught there. Samuelson identified (ibid.: 170–172) a number of Lerner's papers that deserved special attention in this regard, such as his 1933 and 1934 articles on 'arc elasticity and the elasticity of substitution' (Lerner 1933b, 1934d, e); 'his definitive 1935 paper on

the economic theory of index numbers' (Lerner 1935b); his papers on international trade between 1932 and 1934, including his 1933 LSE seminar paper; the 1934 paper on monopoly and monopoly power; and 'his great *Economics of Control*'. Regarding Lerner's original contributions to macroeconomic policy debate, Samuelson wrote (ibid.: 177):

And certainly no economist can be the same after reading Lerner's *Functional Finance* ... How revealing (even if overly simple) is the notion that we tax only to prevent inflation. Those who don't read Lerner will still enjoy the freedom from the fetters of fiscal orthodoxy that he helped secure.

Samuelson concluded by saying (ibid.: 177–178):

Abba Lerner, the scholar, inhabits the same skin as...Abba Lerner the social reformer. He began with an interest in socialism ... Certainly Lerner was no ordinary socialist. His critique of Marx and Marxists could win an A in a Hayek seminar both for downrightness and content ... Like Knut Wicksell, he stands up to speak for the public interest as his scholarly findings interpret that interest ... The drum beat Abba Lerner listens to is that of science.

Two decades later, in the December 1984 issue of the *Journal of Economic Literature*, Scitovsky posthumously summed up 'Lerner's Contribution to Economics' and astutely noted Lerner's LSE eclecticism when he wrote:

Lerner called himself a socialist but believed, not in the socialist means of the public ownership of the instruments of production, only in what he thought of as socialist ends: democracy, individual freedom, a fair income distribution, full employment, and an optimal resource allocation. He must have shocked his fellow socialists when he extolled private enterprise on the ground that "alternatives to government employment are a safeguard of the freedom of the individual". They were probably even more shocked when he argued against minimum wages, because they interfered with the price mechanism, which he considered "one of the most valuable instruments of modern society". Again, he was a devotee of free enterprise but would have astonished his co-devotees had they known that he defined it as "the freedom of both public and private enterprise to enter any industry on fair terms which, in each particular case, permit that form to prevail which serves the public best". Lerner was also the most ardent of Keynesians, to judge by all he did to clarify, extend and spread Keynes's ideas; yet he must have dismayed other Keynesians when he "spotted the fatal flaw in the Keynesian schema" and warned against the inflationary

consequences of full-employment policies two decades before the rest of us did (Scitovsky 1984: 1549).

To sum up, over the course of his career as an economist, Lerner made many outstanding contributions. As we have tried to show, most, if not all of them, emanated from, or were influenced by, his time as an undergraduate and graduate student, and junior faculty member, at LSE and the approach that he absorbed in its eclectic academic atmosphere.

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17

John R. Hicks (1904–1989)

Harald Hagemann

1 Introduction

John R. Hicks taught at the London School of Economics (LSE) from 1926 to 1935, first as an Assistant Lecturer, whose contract was regularly extended, with Hicks finally becoming a Lecturer. He was born on 8 April 1904 at Warwick where his father Edward was a journalist at a local newspaper. From 1917 to 1922, Hicks was educated at the noted British public school, Clifton College, where he had won a scholarship in mathematics. In 1922, Hicks went up to Balliol College, Oxford, where in his second year he switched from mathematics to the new programme in Philosophy, Politics and Economics (PPE), ‘which was perhaps better devised for the training of politicians than of academics’ (Hicks 1979a: 195). After getting his BA, he secured a one-year scholarship for postgraduate research in which he focused on economics where he could make use of his mathematical skills. Hicks consulted Graham Wallas and Edwin Cannan at LSE but ended up writing his thesis on ‘Skilled and Unskilled Wages in the Building and Engineering Trades’ under the supervision of G.D.H. Cole at Magdalen College, Oxford: ‘Economics at Oxford, was very “social”; so they started me working on labour problems’ (ibid.). Following his father, in 1926 Hicks worked for a

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time as a junior reporter at the *Manchester Guardian* before moving to LSE at the beginning of the new academic year.

It was LSE that made Hicks an economist and he did some of his best work there: ‘Those nine years at LSE fall very sharply, from my point of view, into two parts. They are separated, in 1929, by the arrival of Lionel Robbins as head of department. In the three years before that time I had been working mainly by myself ... After 1929 I was a member of a group, the group which Robbins built up around him’ (ibid.: 196).¹ Until his retirement, economics at LSE in the 1920s was dominated by Cannan, who acted as a counterpart to Alfred Marshall at Cambridge. Allyn Young (1876–1929), who succeeded Cannan in the Chair of Political Economy, came over from Harvard in 1927 but suddenly died after only eighteen months at LSE. In this short period, he had a much deeper influence on Nicholas Kaldor than on Hicks. Robbins, who was a Lecturer at LSE when Hicks arrived but soon after left for New College, Oxford, was only 30-years-old when he returned as Professor in 1929. Robbins struggled to establish both himself and economics at the School. However, his influential *Essay on the Nature and Significance of Economic Science* (Robbins 1932) transformed the teaching of economics away from Cannan’s ‘commonsense approach’ into a more analytical approach, in turn creating a distinct research programme as outlined in Robbins’s January 1930 Inaugural Lecture ‘The Present Position of Economic Science’ in which he referred to the contemporary boom in economic theory and demanded a re-examination of fundamental theoretical questions. This marked a clear watershed compared to Cannan who in his 1933 Presidential Address to the Royal Economic Society was still claiming ‘The Need for Simpler Economics’, arguing against finding contentment in neat equations and elegant equilibria in the higher branches of theory.

Whereas Cannan and Arnold Plant, the teacher of the young Ronald Coase in industrial organisation, were applied economists with a strong institutional interest, Robbins, Hayek and Hicks played ‘a leading role in’ bringing ‘into being, for good or ill, the modern age in economics’ (Coase 1982: 34). However, despite the transition from a common sense to a more professionalised economics, free-market views continued to prevail at the Department of Economics as Cannan had guided it away from the roots of LSE in Fabian socialism.² So the young Hicks ‘became a free market man’ (Hicks 1979a: 197) before he left LSE in 1928/1929 to spend almost a year

¹See also Chapter 1 in Hicks (1982).

²‘It was from Cannan that the LSE “free market” tradition descended’ (Hicks 1982: 4).

teaching at the University of Witwatersrand in Johannesburg. With his LSE training, he quickly recognised that the trade unions in South Africa were functioning as monopolists, reserving skilled jobs for white labour.

Continuity also prevailed in the dispute with Cambridge which intensified in the early 1930s, this time with Robbins (and later Hayek) on the LSE side and Keynes (and later Sraffa) on the Cambridge side.³ The clash between Robbins and Keynes became conspicuous when at the meetings of the Economic Advisory Council in September–October 1930 Robbins, armed with his *laissez-faire* convictions, fundamentally opposed Keynes's turning away from free trade at the peak of the Great Depression, Keynes's argument against money wage cuts as a stimulus to output and employment, and particularly Keynes's 'favourite remedy—the one to which I attach much the greatest importance' (Keynes 1981: 126), namely government investment or public works. In his *Autobiography*, Robbins later regretted his violent disagreement with Keynes and considered it as 'the greatest mistake of my professional career' (Robbins 1971: 154). While he denied that he had actively advocated deflation in the Depression, he conceded that the Keynesian remedies were a proper medicine against the slump rather than a reliance on the self-healing forces of the market:

The trouble was intellectual. I had become the slave of intellectual constructions which, if not intrinsically invalid as regards logical consistency, were inappropriate to the total situation which had then developed and which therefore misled my judgement. I realized that these constructions led to conclusions which were highly unpalatable as regards practical action. But I was convinced that they were valid and that therefore it was my duty to base recommendations as regards policy upon them (Robbins 1971: 153–154).

Robbins's statement points to the fact that he had derived his economic policy conclusions from the Austrian theory of the business cycle as developed by Mises and elaborated by Hayek which in its emphasis on monetary overinvestment as the decisive cause of the slump was diametrically opposed to Keynes's explanation. Thus, the fierce controversies that took place in 1930–1932 were not only due to methodological or political differences but also and primarily due to differences in the underlying *theories* of the causes of the crisis. In the early 1930s, among economists inside

³For more details, see, for example Winch (1969), Robbins (1971), Coats (1982), McCormick (1992), Skidelsky (1992), and Kurz (2000).

and outside England, LSE became increasingly considered as ‘a suburb of Vienna’ (Plant 1974: 170).

However, this special suburb was neither provincial nor parochial but in fact very cosmopolitan. The Economics Department at LSE in the inter-war period acted as a centre of gravity for many bright students from all over the world. Of course, it helped that the School was located in the capital of the commonwealth, but the focus on all areas of the social sciences and a tolerant atmosphere,⁴ despite many fierce controversies as, for example, on socialist calculation, and numerous seminars and lectures by leading foreign economists, also contributed to the international reputation of LSE.⁵ It was a time when most classics in economics that had been written in foreign languages were not yet translated into English nor was English already the *lingua franca* as it developed in the post-war period.

The young Hicks benefited from his excellent language skills and his reading knowledge of French, German and Italian which allowed him to read Walras and Pareto but also Wicksell and Cassel in the original. During his PPE studies at Oxford, Hicks had to do a translation into French. This inspired him to read French literature, with Voltaire becoming one of his favourites following a suggestion from his maternal aunt Winifred Stephens.⁶ Hicks shared a great interest in history and literature with his two sisters, the elder Phyllis and the younger Mary. His favourite author was Dante to whom he had been introduced by his mother Dorothy Stephens. Hicks had started to learn Latin at the age of seven which was a great help in learning Italian. Italy later became the favourite travel destination for Hicks and his wife.⁷ After the Second World War, Hicks at Oxford, together with Piero Sraffa at Cambridge and, somewhat later, Franco Modigliani at MIT, became one of the centres of attraction for promising young Italian economists to acquire a PhD.

‘I managed enough German to read the Austrians, and also Wicksell and Myrdal (at that time only available to me in German). I have never learned Swedish, but...I have been deeply influenced by Swedish economics’

⁴‘There was indeed a substratum of “liberal” political principles which our socialists and our free market men had in common’ (Hicks 1979a: 198).

⁵In his centennial history of LSE, Ralf Dahrendorf (1995: 223), following Harry Johnson (1972: 22), could rightly state: ‘The internationalization of LSE was, and is, one of its greatest strengths’.

⁶For further details, see Chapter 1 of Hamouda (1993). For shorter surveys on the life and work of Hicks, see Bliss (1987) or Hagemann (2016). For critical assessments of various aspects of Hicks’s works, see the collection edited by Wood and Woods (1989).

⁷‘We now feel that a year that does not contain a visit to Italy is a year in which there is something missing. And now, when we come to Italy, we come to see our friends’ (Hicks 1979a: 204).

(Hicks 1979a: 198). One leading Swedish economist, who became increasingly important for Hicks's own work, is missing from the above: Erik Lindahl. Lindahl (1891–1960), who had started his remarkable scientific career in public finance with an important treatise on 'just taxation', was among the early distinguished visitors at LSE in the Robbins period where he came into closer personal contact with Hicks and Ursula Kathleen Webb (1896–1985), herself a renowned economist in public finance and development economics, with whom he became a lifelong close friend. Ursula was of greatest help for the English edition of Lindahl's masterpiece, *Studies in the Theory of Money and Capital* (Lindahl 1939); Lindahl's contributions on the methods of dynamic analysis and his notion of temporary equilibrium had a decisive influence on the work of John. Hicks married Ursula on 17 December 1935 in London, four months after his departure for Cambridge.⁸ For the next fifty years, she became his closest intellectual companion.

Hicks stayed in Cambridge for three years as a University Lecturer in Economics and a Fellow of Gonville and Caius College. In 1938, he moved to the University of Manchester as Stanley Jevons Professor of Political Economy. As the only professor available at Manchester during the war, Hicks had to do mainly elementary teaching, which was not his area of comparative advantage. His former LSE student Ronald Coase in retrospect pronounced that 'Hicks was unsuccessful as an undergraduate teacher', failing 'to inspire his undergraduate audience' (Coase 1982: 32). Hicks's aversion to undergraduate teaching has been confirmed by Hans Singer who as a young lecturer worked closely with Hicks at Manchester in the war years. In an interview with Keith Tribe, Singer remembered that 'Very often Hicks lectured above the heads of the students' (Singer in Tribe 1997: 69). However, Hicks's reputation as a teacher underwent a change when from 1931 onwards he 'began to give lectures on advanced economic theory [at LSE] and his power as a theorist [became] immediately apparent' (Coase 1982: 32).⁹

Nevertheless, it was at Manchester that Hicks did his main work on welfare economics.¹⁰ He found the Manchester period unexciting because of the elementary teaching he had to do, although he 'took advantage of this to write my *Social Framework*' (Hicks 1979a: 201), one of the first

⁸For the letters between Hicks and Webb during September–December 1935, see Marcuzzo et al. (2006).

⁹See Coase (1982: 32, fn. 9) for the list of topics covered by Hicks in his advanced courses.

¹⁰See Hicks (1981: Part I) and for a modern assessment, see Chipman (1994).

textbooks on national income accounting (Hicks 1942) which ran into the fourth and final edition in 1969 and was translated into many foreign languages.

A greater focus on research was a key stimulus for Hicks to return to Oxford in 1946, first as a Research Fellow at Nuffield College and from 1952 to 1965 as Drummond Professor of Political Economy. After taking an early retirement from his Chair and thereby from teaching and administrative duties, Hicks remained at Oxford as a Research Fellow of All Souls College until 1971. This was a very productive period for Hicks who at the end of 1972 became the first British economist to be awarded the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. Hicks won the prize jointly with Kenneth Arrow ‘for their pioneering contributions to general economic equilibrium theory and welfare theory’, although Hicks himself was not particularly happy about it since he thought that he had outgrown his earlier work. He had first met Arrow, and Paul Samuelson, on his visit to the USA in late summer and fall of 1946 when he was surprised to find how deeply their work had been influenced by his *Value and Capital* (Hicks 1939a): ‘But I am afraid I disappointed them; and have continued to disappoint them. Their achievements have been great; but they are not in my line. I have felt little sympathy with the theory for theory’s sake’ (Hicks 1979a: 201–202). Hicks insisted that he had already pointed out at the end of his Preface to *Value and Capital* ‘that the place of economic theory is to be the servant of applied economics’ (Hicks 1939a: iii). Thus, Hicks was never likely to fall victim to Schumpeter’s ‘Ricardian vice,’¹¹ when forty years later he stated ‘that I have also been aware that theory gives one no right to pronounce on practical problems unless one has been through the labour, so often the formidable labour, of mastering the relevant facts’ (Hicks 1979a: 202).

Hicks was President of the Royal Economic Society from 1960 to 1962 and Knighted by Queen Elizabeth II in 1964. He had already become a Fellow of the British Academy in 1942, a foreign member of the Royal Swedish Academy in 1948, of the Italian Accademia dei Lincei in 1952, and of the American Academy in 1958. Hicks, who received an honorary doctoral degree from more than a dozen universities, died at his home in Blockley, Gloucestershire, which he had inherited from his Aunt Winifred, on 20 May 1989.

¹¹See Kurz (2017) for a recent discussion.

2 Hicks the Labour Economist

Hicks's early work as a labour economist culminated in *The Theory of Wages* (Hicks 1932), which 'is in its main lines thoroughly "neo-classical"' (Hicks 1979a: 197). Despite some shortcomings, later openly conceded by the author in his long commentary on his 'juvenile opus' to the second edition of *The Theory of Wages*, the book, which was founded upon marginal productivity theory and influenced by the work of Wicksell, introduced a number of innovative concepts. This holds in particular for the famous Chapter 6 on 'Distribution and Economic Progress' in which Hicks presents the new concepts of the 'elasticity of substitution' and 'Hicks-neutral', 'labour-saving' and 'capital-saving' inventions to discuss how the relative shares of labour and capital (under the assumption of constant returns to scale) will respond to changes in the capital–labour ratio as a consequence of changes in the relative 'prices' of the factors of production, i.e. the ratio between the wage rate and the rate of profits, and changes in the methods of production due to technical progress.¹²

In Section III of Chapter 6 'Inventions must Increase the Social Dividend', Hicks joins forces with Kaldor (1932) and Wicksell's earlier critique of Ricardo's analysis of the machinery problem, in particular, Ricardo's conclusion as to a possible diminution of the gross produce, which Wicksell and Kaldor considered as wrong: 'On the contrary, the machinery will always have the effect of raising the gross produce of the country to its greatest possible amount, and in so far it will provide the *means* for bettering the economic conditions of the working men as well as of their employers' (Wicksell quoted in Jonung 1981: 201; italics in original). In the same fashion, Hicks argued in 1932:

Under the assumption of competition, it inevitably follows that an invention can only be profitably adopted if its ultimate effect is to increase the National Dividend. For if it is to raise the profits of the entrepreneur who adopts it, it must lower his costs of production—that is to say, it must enable him to get the same product with a smaller amount of resources. On balance, therefore, resources are set free by the invention; and they can be used, either to increase the supply of the commodity in whose production the invention is used (if the demand for it is elastic), or to increase the supply of other commodities (if the demand for the first is inelastic). In either

¹²For retrospective views on Hicks's *Theory of Wages*, see Rothschild (1994) and Solow (2008).

case, the total Dividend must be increased, as soon as the liberated resources can be effectively transferred to new uses (Hicks 1932: 121).

In the late 1980s, Samuelson set out to vindicate Ricardo's propositions on machinery,¹³ in particular the analytical question as to whether a viable invention could reduce aggregate output, coming to the conclusion that all those economists who dismissed Ricardo's propositions were erroneous. 'Ricardo is right. Wicksell (and Kaldor and ...) are wrong' (Samuelson 1989: 52). Samuelson added that 'J.R. Hicks (1969) is perhaps an exception but his discussion does not address Wicksell's query about the invention's effect on total output' (ibid.: 48, fn. 2). However, it should be noted that Samuelson himself overlooked Hicks's analysis of the problem in Chapter 6 of *Theory of Wages* in which at the end of the quoted passage above Hicks explicitly referred to the section entitled 'The Influence of Technical Inventions on Rent and Wages' of Wicksell's *Lectures on Political Economy*¹⁴ and Kaldor (1932) for a fuller elaboration of the argument.

Thus, although Hicks shared Wicksell's and Kaldor's opinion that Ricardo's view that the introduction of new machinery can result in a reduction in gross income was erroneous, there is not necessarily a contradiction with the views of Hicks (1969, 1973a). The last sentence of the quoted passage already indicated that this increase will take place under the assumption that employment is maintained, i.e. a successful compensation process has taken place. This conclusion in no way contradicts Ricardo's one of a diminution of gross income in his numerical example of an embryonic form of traverse analysis in which the introduction of new machinery causes a decline in the demand for labour and the output of consumption goods, but, due to Ricardo's numerical example only extending to four periods, the long-run time paths of employment and output are left unresolved: 'Ricardo's theory is a theory of the working of the individual impulse' (Hicks 1983a: 38). Since it is a characteristic feature of Ricardo's example that it abstracts from capital accumulation, his approach contains a kind of *capital shortage* theory of temporary technological unemployment. Nevertheless, Ricardo deserves merit for pointing out that a process of additional saving and

¹³See Ricardo (1821 [1951]: Chapter 31).

¹⁴Hicks (1932: 121, fn. 2) refers to the German edition of Wicksell's *Lectures* (Wicksell 1913: 195–207) which at that time were not yet translated into English. Interestingly, it was Robbins who wrote an insightful Introduction to the English translation in which he rightly stated, 'that Wicksell... must be looked upon as one of the founders of the marginal productivity theory' (Robbins in Wicksell 1934: xiii) and points out that '[t]he final version of the text owes much to Dr. J.R. Hicks, who generously gave much time to the checking and correction of the manuscript' (ibid.: xix).

investing is necessary to assure the compensation of displaced workers. Hicks's emphasis in 1932 was a different one, namely on the functional distribution of income. Thus, he emphasised: 'In every case, however, a labour-saving invention will diminish the relative share of labour' (Hicks 1932: 122).¹⁵

3 Hicks and Hayek

During the 1930s, the research seminars held by Robbins and Hayek were the focus of intense theoretical debates at LSE.¹⁶ Hicks participated in both seminars until his move to Cambridge in 1935. Hayek gave his famous *Prices and Production* lectures in February 1931 and was appointed Professor to the revived Tooke Chair of Economic Science and Statistics the following autumn. In fact, the second part of Hicks's time at LSE itself has to be divided into two sub-periods. 'There is a pre-Hayek stage which can be identified' to which 'my own *Theory of Wages* belongs' (Hicks 1982: 3). Before Hayek's arrival, Hicks had paid little attention to monetary economics. Furthermore, it was Hayek who made Hicks think of the production process as a process in time, a key Austrian element dating back to Böhm-Bawerk's capital theory, which was emphasised in Hayek's business cycle theory.

The economics of Hayek, as with the economics of Keynes, became a life-long challenge for Hicks in his efforts at developing his own theory.¹⁷ In particular, Hayek had introduced him to the work of Wicksell. However, 'Wicksell plus Keynes said one thing, Wicksell plus Hayek said quite another' (Hicks 1967: 204). 'But I did not begin from Keynes; I began from Pareto, and Hayek' (Hicks 1979a: 199). Hicks had always been sceptical about Hayek's claim that the economy would be in equilibrium if there were no monetary disturbances. This scepticism was already manifested in Hicks's early essay on 'Equilibrium and the Trade Cycle' (Hicks 1933 [1980]) which essentially is the result of Hicks's grappling with Hayek's *Prices and Production* and Hayek's 1928 concept of intertemporal equilibrium (Hayek 1928 [1984]). Here, we find Hicks arguing against Hayek's statement, that a change in the effective volume of monetary circulation is to be regarded

¹⁵For a more detailed treatment of Wicksell's analysis of Ricardo's machinery problem and Hicks's view on the subject, see Hagemann (2008).

¹⁶Hayek's presence added great strength to the magnetic attraction of Robbins's seminar ... In the 1930s, J.R. Hicks was one of the outstanding regular attenders at the Robbins-Hayek seminar' (Plant 1974: 170–172).

¹⁷See Hicks (1967: 203–215) and Hagemann (1998).

as an independent cause of disequilibrium. I cannot accept this in its literal sense, though I am prepared to agree that in a world of imperfect foresight monetary changes are very likely to lead to acute disequilibrium (Hicks 1933 [1980]: 526, fn. 8).

Hicks realised that to analyse money one must consider uncertainty and expectations. He endured a long struggle to arrive at an inherently dynamic version of the economy in which agents' present decisions represent attempts to cope with an uncertain future in view of monetary and real constraints imposed upon them by past actions. But although Hicks made important contributions to monetary theory over a period of almost six decades, he never ceased emphasising 'the *real* (non-monetary) character of the cyclical process' (Hicks 1950: 136; italics in original). Indeed, it had been one of the main objectives of his *Contribution to the Theory of the Trade Cycle* 'to show that the main features of the cycle can be adequately explained in real terms' (ibid.). Hicks remained a lifelong critic and modifier of Hayek's original business cycle theory. Hayek's theory essentially is based on monetary overinvestment. But while monetary factors cause the cycle, real phenomena constitute it. Although cyclical fluctuations are caused by monetary factors, in particular excessive credit creation, it is the distortion of the structure of relative prices and their impact on the real structure of production which is most important. Hicks, by contrast, always treated the cycle as fundamentally a real phenomenon reflecting technological changes and the fluctuations in investment that accompany them. Monetary disorders may be superimposed upon real disorders, but they are of only secondary importance.¹⁸

The Hicks–Hayek debate went on for many decades after the LSE seminars. Thus, when Hicks published his 'Hayek Story' (Hicks 1967: Chapter 12), Hayek (1969) reacted with his 'Three Elucidations of the Ricardo Effect', i.e. the effect of a shortage of consumption goods on the production of investment goods, which plays a key role in the explanation of the upper turning point in Hayek's business cycle theory. Hayek had already referred to 'Ricardo's doctrine of the conversion of circulating into fixed capital' in his *Prices and Production* (Hayek 1931 [1935]: 101), but began

¹⁸As such, Hicks was more in agreement with Wicksell who essentially held a real theory of the business cycle. See Boianovsky (1995), Leijonhufvud (1997), and Laidler (1999). For a more detailed comparison of the different views of Hicks and Hayek concerning the major cause of cyclical fluctuations, see Hagemann (1998).

to refer to the ‘Ricardo effect’ only when his focus shifted from money and interest to capital and profit (Hayek 1939: Chapter 1, 1942).¹⁹

Hicks always acknowledged that it was one of Hayek’s major contributions to have shown the importance of the temporal structure of production processes for cyclical fluctuations. The use of Böhm-Bawerk’s theory of capital and Hayek’s emphasis on vertical maladjustments in the structure of production was unfamiliar in England and created a key obstacle to an easy reception of Hayek’s theory: ‘*Prices and Production* was in English, but it was not English economics’ (Hicks 1967: 204). Although Hicks (1973a) took in his ‘neo-Austrian theory’ of *Capital and Time* from the Austrians the idea that production is a process in time with strong intertemporal complementarities and took over from Hayek the idea that the impact of an impulse on the real structure of production is decisive, unlike Hayek, Hicks kept emphasising the priority of real factors, i.e. technological change, over monetary factors as the key underlying cause of cyclical fluctuations:

Where...I do not go along with him [Hayek] is in the view that the disturbances in question have a monetary origin. He had not emancipated himself from the delusion...that with money removed “in a state of barter” everything would somehow fit. One of my objects in writing this book has been to kill that delusion. It could only arise because the theory of the barter economy had been insufficiently worked out. There has been no money in my model; yet it had plenty of adjustment difficulties. It is not true that by getting rid of money, one is automatically in “equilibrium”—whether that equilibrium is conceived of as a stationary state (Wicksell), a perfect foresight economy (Hayek) or any kind of steady state. Monetary disorders may indeed be superimposed upon other disorders; but the other disorders are more fundamental (ibid.: 133–134).

‘One must introduce uncertainty, before one can introduce money’ (Hicks 1982: 7). Hicks had not focused on monetary economics before Hayek’s arrival at LSE. This did not only change but, dating from his 1935 ‘A Suggestion for Simplifying the Theory of Money’, a landmark in the evolution of a theory of liquidity preference, which ‘drew Money into the orbit of marginalist calculation’ (Leijonhufvud 1984: 27). Hicks had established himself as an influential monetary economist. He continued to make contributions to this field through his *Critical Essays in Monetary Theory* (Hicks 1967) and ‘The Foundations of Monetary Theory’

¹⁹For an examination of the use and role of Ricardo effect(s) in Hayek’s business cycle theory, see Hagemann and Trautwein (1998).

(Hicks 1982: Chapter 19) to his last book *A Market Theory of Money* (Hicks 1989) in which Hicks treated money as an integral part of the institutional framework and elaborated a neo-Wicksellian approach for a modern ‘overdraft’ economy in which interest rates and their control play a central role.

From the beginning, Hicks felt uneasy about the ‘terribly unrealistic *perfect foresight*’ concept of equilibrium (Hicks 1982: 7; italics in original). In ‘Equilibrium and the Trade Cycle’, which was a response to Hayek and was first published in German in the Vienna-based *Zeitschrift für Nationalökonomie*, Hicks adopted an early formulation of an Arrow–Debreu–McKenzie concept of equilibrium for which he rightly pointed out that ‘[t]he condition for equilibrium...is Perfect Foresight. Disequilibrium is the Disappointment of Expectations’ (Hicks 1933 [1980]: 526). ‘Thus we cannot escape the conclusion that if the future course of economic data (and the corresponding future course of prices) were exactly foreseen, there would be no demand to hold money as money’ (ibid.: 528). For Hicks, two consequences arose: imperfect foresight causes economic fluctuations, and monetary theory falls outside equilibrium theory.

With increasing age, Hicks emphasised more and more the relationship between economic theory and economic history as of fundamental methodological significance. This becomes very clear in Essay 9, ‘Monetary Theory and History—An Attempt at Perspective’, where Hicks (1967: 156) points out that a larger part of the best writings in monetary economics is topical, i.e. linked to special historical circumstances and institutional settings: ‘Monetary theory is less abstract than most economic theory; it cannot avoid a relation to reality, which in other economic theory is sometimes missing’.²⁰

History was Hicks’s favourite subject at school and occupied a larger part of his library.²¹ He had not only a deep sense of the historical origins and the time-related genesis and content of economic models, thereby also identifying their intrinsic limits, but also made ample use of the materials of economic history and the history of economic thought as necessary tools in the process of economic theorising.²²

²⁰For a more detailed assessment of Hicks’s work on monetary economics, see Leijonhufvud (1984), the contribution by Laidler in Hagemann and Hamouda (1994), Fontana (2004) and the essays in Part III of Scazzieri et al. (2008).

²¹See Hamouda (1993: Chapter 10).

²²For an example of the former, see *A Theory of Economic History* (Hicks 1969), in which Hicks worked out the origins and evolution of the market mechanism, and for the latter, see Part I, ‘Classics and Post-Classics’, in Hicks (1983a).

4 Hicks and Keynes

Hicks's 'Suggestion for Simplifying the Theory of Money' (Hicks 1935), of which Hicks had 'a much higher opinion...than of any other of' his 'early papers' (Hicks 1982: 9), brought him into closer contact with Keynes who in 1935 had already gone most of the way on his journey from the *Treatise on Money* to the *General Theory*, in which his new concept of liquidity preference played an essential role. Hicks (1977: 134) later remembered in 'Recollections and Documents', 'the story of my personal "Keynesian Revolution"', that it had been the first time in Keynes's response to the proofs of his 'Simplifying' paper that he had heard of liquidity preference (see *ibid.*: 142). In the 1970s, Hicks came to appreciate the *Treatise* as 'more genuinely dynamic, and therefore more human' than the *General Theory* which he considered 'a brilliant squeezing of dynamic economics into static habits of thought' (*ibid.*: 148). He now considered Keynes's theory of liquidity preference as 'misnamed. It makes the demand for money depend on Uncertainty, not Liquidity' (*ibid.*: 147).

No wonder that many economists have pointed out similarities between Keynes's views on liquidity in the *Treatise* and Hicks's ideas in his 'Simplifying' article. The two authors originally may have felt the similarity themselves, but in his many writings on 'Liquidity', starting with his Presidential Address to the Royal Economic Society in 1962, Hicks increasingly emphasised their differences and pointed out that 'its ['Simplifying'] message was a Declaration of Independence, not only from the "free market" school from which I was expressly liberating myself, but also from what came to pass as Keynesian economics' (Hicks 1982: 10). The latter he now found more mechanical than he or Keynes had intended. However, Hicks was also not very happy about what was to become of his own approach in the modern mainstream where his 'Simplifying' paper has laid the foundation for a choice-theoretical money demand function or theory of portfolio selection where choice between different assets is a choice between probability distributions. This is due to the fact that he laid the microfoundations of monetary theory just after his joint work with Roy Allen on consumer demand. Hicks's attempt to marginalise the theory of money in his 'Simplifying' paper is surely not what Keynes had in mind.

On the other hand, there are some parallels, so when Hicks points out 'that the use of money is enough in itself to make a free-market system potentially unstable; and that the higher the degree of development, or sophistication, that it exhibits the greater does the danger of instability

become' (Hicks 1982: 9). Hicks considered monetary institutions, in particular central banks, to play an important though imperfect role in safeguarding against instability, while acknowledging that such bodies could themselves become unstable. Furthermore, Hicks like Keynes emphasised the important role of psychological factors which exclude a reliance on simple mechanical remedies. From the very beginning in 'Simplifying' until his final *A Market Theory of Money*, Hicks's emphasis on balance sheet equilibria was a common theme in his writings on monetary economics. Focusing on the assets side and rather neglecting the liabilities side, Hicks argues that balance sheet equilibrium is governed by expectations of the yield of investments and risks, i.e. 'determined by subjective factors like anticipations, instead of objective factors like prices', which 'means that this purely theoretical study of money can never hope to reach results so tangible and precise as those which value theory in its more limited field can hope to attain ... It needs judgment and knowledge of business psychology much more than sustained logical reasoning' (Hicks 1935: 13).

'One is driven back, in the end...from Keynes to Wicksell', Hicks (1982: 237) states at the end of his Prefatory Note to 'The Foundations of Monetary Theory', which is 'meant to represent the substance of my later work on monetary theory' (ibid.: 236). This later work concludes with *A Market Theory of Money* in which Hicks reconsiders modern institutional developments in the money and financial markets. According to Hicks, the modern financial system had entered into a complex form of a Wicksellian credit economy in which overdraft facilities had become an important characteristic: 'If the firm knows that it can get funds when it needs them, it need keep no liquid assets as reserves', Hicks (1974: 50) had already pointed out in *The Crisis in Keynesian Economics*. It had been the pressure of high short-run interest rates in the 1970s and 1980s which gave a strong incentive for economising in money holding and replacing reserve assets, i.e. non-interest-bearing money, with 'an overdraft system, on which interest is *saved* on the part of the overdraft that is *not* used' (Hicks 1982: 265; italics in original). In the Hicksian credit economy, in which the Wicksellian model is extended to include financial intermediaries who have better information on sound investments, the rate of interest is the key instrument of monetary control. Closer cooperation between specialised financial intermediaries and the central bank can reduce instability in case of exogenous shocks. The establishment of well-functioning money and financial markets with the central bank at the centre acting as lender of last resort can contribute to a reduction in short-run liquidity risks faced by individual banks. In such an overdraft system, it is the interest rate set by the central bank

which rules the roost. Writing in the tradition of Thornton and Bagehot, Hicks considers this deposit rate as the decisive controlling factor in monetary policy and as a means to help protect against financial crises. He quotes Bagehot with approval: ‘The best palliative to a panic is a confidence in the adequate amount of the bank reserve’ (Bagehot quoted in Hicks 1989: 97, fn. 4). Hicks’s focus is on interest rates, not the quantity of money. As such, he directly opposed the British Currency School and its emphasis on the exogenous regulation of the quantity of money but also the Keynesian wing of the Credit School (a term Hicks preferred to the British Banking School (see Hicks 1967: viii)), insofar as their representatives backed quantitative easing or ‘monetary socialism’. For Hicks, the Keynes of the *General Theory* was ‘*too* monetarist’ (Hicks 1982: 264; italics in original). Therefore, one has to go back to Wicksell and to the ‘Keynes of the *Treatise on Money*, who was by no means a “modern Keynesian”’ (Hicks 1967: viii). Hicks did not reject the liquidity preference theory as explicitly as Leijonhufvud had done in his ‘Wicksell Connection’, where he posits a ‘Z-Theory’ as the *Treatise* plus quantity adjustment or the *General Theory* minus liquidity preference (see Leijonhufvud 1981: 164–169). Saying this, Hicks sometimes gives the impression of reconciling liquidity preference with the loanable funds theory (see, for example, Hicks 1986). Indeed, it would not be out of place to call Hicks the ‘John Stuart Mill of Keynesian Economics’, a label which Hicks would probably have considered a compliment since he had a strong preference for Mill (see Hicks 1983a: Chapter 5). However, there exist major problems in trying to integrate Keynes’s liquidity preference theory with a (neo-)Wicksellian model in which the credit supply is elastic and the money supply is endogenous (see Chick 1991).

IS-LM (after Hansen’s modification of Hicks’s original SI-LL terminology) are the four letters students of several generations have associated with Hicks after their first basic course in macroeconomics. The IS-LM schedule specifies the combinations of interest rates and levels of national income which ensure equilibrium in the goods and money markets. The point of intersection determines simultaneous equilibrium in both markets. However, the labour market is left out of IS-LM. So, is Keynesian unemployment compatible with a Walrasian interpretation, when in Walras all markets are cleared?

Despite the great influence of his interpretation of Keynes’s *General Theory* through IS-LM and the ensuing development of modern macroeconomic theory—as well as students being trained in the effects of monetary and fiscal policies on the basis of this standard macroeconomic model—Hicks was never convinced that the whole Keynesian theory could be properly represented

within the model he was responsible for establishing in his ‘Mr. Keynes and the “Classics”; A Suggested Interpretation’ (Hicks 1937).

This article, which Hicks first presented to the meeting of the Econometric Society at Oxford in September 1936, was not the first but the second interpretation of the *General Theory* by Hicks, after he had written his review article ‘Mr. Keynes’s Theory of Employment’ for the *Economic Journal* of which Keynes was editor: ‘I was asked because it was hoped that I should be a sympathetic but independent critic; and such, at that date, were not easy to find’ (Hicks 1974: 6). Nevertheless, it was his second article, which captured those parts of Keynes’s theory most accessible to formalisation that exerted the major influence: ‘Keynes’s own version of Keynesian economics is by no means easy to determine. I do not pretend that I can determine it; yet on these matters I think I have something to say’ (ibid.: 5) Hicks stated in the Introduction to his *The Crisis in Keynesian Economics*.

From the mid-1960s onwards, Hicks came back time and again to a reinterpretation of Keynesian economics (Hicks 1974, 1977: Chapter VI, 1980), and he increasingly drifted away from the ‘neoclassical synthesis’ (Samuelson) mainstream he himself had helped to establish in his younger years and which was strongly disliked by Keynes’s disciples such as Richard Kahn and Joan Robinson who rightly argued that IS-LM did not capture the uncertainty that characterises a monetary economy. Keynes also never completely accepted Hicks’s interpretation, as Hicks may have felt at the time,²³ but neither did he explicitly reject the IS-LM approach. At the beginning of his friendly letter to Hicks dated 31 March 1937, Keynes stated: ‘I found it very interesting and really have next to nothing to say by way to criticism’ (Keynes 1973: 80–81). Kahn perceived that ‘Keynes’ rebuke was too mild’ (Kahn 1984: 160) but pointed out that Keynes in his reaction objected that Hicks overemphasised current income in the investment function whereas it is expected income over the investment period which is the relevant variable which he tried to grapple with in his notion of the marginal efficiency of capital. Agreeing with Robinson, Kahn deplored ‘that the elementary teaching of Keynesian economics has been the victim of IS-LM and related diagrams and algebra. It is tragic that Keynes made no public protest when they began to appear’, but also acknowledged Hicks’s increasing distance from what became of his own construction of Keynesian economics as ‘comforting to read’ (ibid.: 160–161.)

²³I think I may conclude from this letter (as I have always done) that Keynes accepted the IS-LM diagram as a fair statement of his position—of the nucleus, that is, of his position’ (Hicks 1973b: 10).

Hicks himself later pointed out that the IS-LM diagram ‘is now much less popular with me than I think it still is with many other people. It reduces the *General Theory* to equilibrium economics; it is not really *in time*’ (Hicks 1982: 289–290; italics in original). Among the three parts he considered as the essential building blocks of Keynes’s theory, the marginal efficiency of capital and liquidity preference is unquestionably *in time*, whereas the multiplier theory is not. In his widely read 1980 article, ‘*IS-LM: An Explanation*’, Hicks accordingly emphasised the hybrid character of his own construction that the IS curve is a flow relation, whereas the LM curve is a stock relation referring to a point in time. The IS-LM analysis therefore could only survive ‘in application to a particular kind of causal analysis, where the use of equilibrium methods...is not inappropriate’ (ibid.: 152). Leijonhufvud (1983) came to the conclusion that the hybrid character of the IS-LM apparatus, which ignores the sequence of events within a period, is due to the fact that it combines a Walrasian element of a simultaneous equilibrium on interdependent markets with Marshallian microfoundations. The problem was that Marshallian economics was *in time*, whereas theory in the Walrasian tradition was not, as Hicks only later came to recognise.

Hicks, unlike Keynes, did not have a high public profile. He was convinced that the third quarter of the twentieth century should be considered ‘as the age of Keynes’ (Hicks 1974: 1) which mainly began, interestingly, after the end of the Second World War and the death of Keynes, although Hicks attested that Keynes had a ‘keen nose for the actual, the current actual’ (Hicks 1973b: 7, fn. 1) which contributed to his ‘win’ over Hayek in the debate over appropriate policy proposals during the Great Depression. Hicks explicitly remained ‘A Sceptical Follower’ of Keynes, as he confessed at the centenary of Keynes’s birth (see Hicks 1983b).

4.1 *Value and Capital*

‘[T]he version of Keynes that is put forward in many modern writings... looks to me more like the *Value and Capital* formulation than like Keynes’s own’ (Hicks 1974: 7). The elder Hicks repeatedly distanced himself from Keynesian economics of the ‘neoclassical synthesis’, descended from Paul Samuelson, Don Patinkin, et al. who themselves had been influenced by Hicks’s own work.²⁴ Although written in Cambridge in the years

²⁴See, for example, Hicks (1983a: 361). On the differences between Hicks’s original SI-LL model and the textbook IS-LM models, see Barends and Caspari (1999).

1935–1938, ‘*Value and Capital* is in essence an LSE book, not at all a Cambridge book. The ideas that went into it were fairly fully formed before I left LSE’ (Hicks 1991: 371; see also Hicks 1983a: 360).

Hicks had moved to Cambridge in summer 1935 accepting the offer which came from Pigou for two particular reasons: his friendship with Dennis Robertson, whose ‘influence on me has been much more personal than that of Keynes’ (Hicks 1967: x), and ‘Beveridge’s insensate hostility to pure theory’ (Robbins 1971: 129) which undermined Robbins’s attempt to keep Hicks at LSE by appointing him as a Reader. However, Hicks did not enjoy his period at Cambridge at all, where only Marshall’s nephew and editor Claude Guillebaud became a friend, because of internal quarrels among the economists and the hostility which came from Kahn and Joan Robinson who were convinced that Pigou and Robertson had invited him in order to stop Joan Robinson getting a Lectureship. So, in his Cambridge years, Hicks focused on writing his magnum opus, the basic ideas of which had, as noted, already been conceived at LSE. In Cambridge, he got only ‘some very useful criticism from Mr. Sraffa’ as Hicks (1939a: iii) points out in the Preface to *Value and Capital* which happily had been published early in 1939 so that it was distributed around the globe before the outbreak of war.

The Swedish Committee cited *Value and Capital* as one of the main reasons why it awarded Hicks the Nobel Prize, with its formulation of modern general equilibrium theory on which subsequent work by Samuelson, Arrow, Hahn, Debreu, McKenzie and others is built.²⁵ In the *Laudatio*, it is emphasised that Hicks gave general equilibrium theory ‘an increased economic relevance’, extending ‘the applicability of the static method of analysis to include multiperiod analysis ... By being deeply anchored in theories of the behaviour of consumers and of entrepreneurs, Hicks’s model offered far better possibilities to study the consequences of changes in externally given variables than earlier models in this field’ (Nobel Prize website 1972). Unlike Arrow, Hicks did not take the existence problem beyond the counting of equations and variables.²⁶ Despite the merits of Walras’s construction of a system of simultaneous equations, Hicks (1939a: 60) identified a ‘certain sterility’ in the approach

²⁵For modern assessments, see also the proceedings of the conference held by the International Economic Association at Bologna in September 1988 to celebrate the fiftieth anniversary of the publication of *Value and Capital*, edited by McKenzie and Zamagni (1991).

²⁶Hicks did most of the general equilibrium theory worth doing. An exact existence proof would be an exception to that view. The existence theorem is important not just because it tells us that an equilibrium exists; more importantly *it shows us what we are assuming when we suppose that an equilibrium does exist ... In this area Hicks left too much unanalysed*’ (Bliss 1994: 94–95; italics in original).

of Walras who ‘did not go on to work out the laws of change for his system of General Equilibrium ... [H]e did not explain what would happen if tastes or resources changed’ (ibid.: 61). In contrast, Hicks undertook a pioneering analysis of the stability of a system involving multiple exchange.

Hicks succeeded in formulating a number of economically interesting theorems. He was the first to apply comparative statics within a general equilibrium framework. However, Hicks was aware that fruitful theorems in comparative statics could only be derived when the equilibrium of the economic system is stable. This was later elaborated by Samuelson in his *Foundations of Economic Analysis* as the ‘correspondence principle’ (see Samuelson 1947: Chapter IX). Here Samuelson pointed out:

The equations of comparative statics are then a special case of the general dynamic analysis. They can indeed be discussed abstracting completely from dynamical analysis ... But the problem of stability of equilibrium cannot be discussed except with reference to dynamical considerations, however implicit and rudimentary. We find ourselves confronted with this paradox: in order for the comparative-static analysis to yield fruitful results, we must first develop a theory of dynamics (ibid.: 262–263).

Hicks struggled with these problems in Parts III, ‘The Foundations of Dynamic Economics’, and IV, ‘The Working of the Dynamic System’, in *Value and Capital* and subsequently (see, for example, Hicks 1956a, 1965: Part I, 1985). In these parts of *Value and Capital*, stronger components from the Marshallian-Keynesian tradition of the short run are merged with ideas originating from Walras and Wicksell and from Austrian capital theory, conceiving production as a process in time. The most important method used is the *method of temporary equilibrium*. Hicks had learned this ‘point of time’ theory, where all decisions are taken on a Monday morning, from Lindahl which he now applied when he ‘was trying to find a way of bringing the behaviour of an economy, over a period, into a formal model’ pointing out that ‘[t]he most obviously Lindahlian chapter in *Value and Capital* is the chapter on Income’ (Hicks 1991: 373). In the short run, markets are in equilibrium. The expectations of actors concerning future developments influence their behaviour in current markets. One of the most important concepts developed by Hicks in *Value and Capital* is the ‘elasticity of expectations’ (Hicks 1939a: 205), which turned out to be fruitful in later macroeconomic theory.

Hicks paid tribute to Lindahl in his contribution ‘Methods of Dynamic Analysis’ to the Lindahl Festschrift (Hicks 1956a) to which he wrote an addendum in Volume II of his *Collected Essays* where he emphasised that the

fixprice method, which he had elaborated in Part I of *Capital and Growth* (Hicks 1965) and which could manage short-run problems rather well, is a disequilibrium method, whereas the flexprice method is a temporary equilibrium method (see Hicks 1982: 232). According to Hicks, dynamic theory may also be cast in the form of stock-flow analysis, with balance sheets capturing most of the expectational influences via capital valuations.²⁷ In his view, the evolution of disequilibrium over time is the key subject of macroeconomics. He later became disappointed with the fixprice equilibrium model of a single period of which his own SI-LL version of Keynes's *General Theory* had been the first prototype: 'Although the fixprice method is a disequilibrium method, it cannot dispense with a concept of equilibrium ... It needs both stock equilibrium and flow equilibrium ... [I]t is stock equilibrium which is fundamental' (Hicks 1982: 233). In other words, Hicks, who from the beginning considered perfect foresight models as essentially static and later came to dislike steady-state models as they became fashionable in post-war growth economics, in his own analysis had to pay attention to inventories of goods and buffer stocks of liquid assets, not least including money, as the consequence of risk and uncertainty and the disappointment of expectations.

5 Welfare Economics

By the time *Value and Capital* was published, Hicks had already moved to Manchester. It was here that most of his important contributions to welfare economics originated: 'The Foundations of Welfare Economics' (Hicks 1939b), 'The Valuation of the Social Income' (Hicks 1940), 'The Rehabilitation of Consumers' Surplus' (Hicks 1941) and 'The Four Consumer's Surpluses' (Hicks 1943).²⁸

Some important ideas developed during this period go back to his earlier collaboration with Roy Allen at LSE which culminated in their two papers 'A Reconsideration of the Theory of Value, Parts I and II' (Hicks and Allen 1934), when the authors (and others) were still unaware of the classic paper

²⁷For Hicks's later recantation of the temporary equilibrium method due to its elimination of dynamics and lags from analysis, i.e. the impermanence problem, see Petri (1991). For a critical assessment of Hicks's capital theory in *Value and Capital*, see Garegnani (2012).

²⁸All of these essays plus further contributions and some comments by Hicks are also included in the collection *Wealth and Welfare* (Hicks 1981). For extensive comments by a modern specialist, see Chipman (1994).

‘On the Theory of the Budget of the Consumer’ (Slutsky 1915) written in Italian by a Russian who had independently derived similar results.

Allen (1936) and Hicks, who considered his papers with Allen as the starting point for *Value and Capital*, soon acknowledged Slutsky’s pioneering work. Thus, Hicks wrote in the section ‘Need for a Theory Consistently Based upon Ordinal Utility’ of Chapter I ‘Utility and Preference’ that ‘[t]he theory to be set out in this chapter and the two following [‘The Law of Consumer’s Demand’ and ‘Complementarity’] is essentially Slutsky’s ... The present volume is the first systematic exploration of the territory which Slutsky opened up’ (Hicks 1939a: 19). Hicks²⁹ and Slutsky both showed that the effect of a price change on the quantity demanded can be divided into two effects: *income effects* and *substitution effects* (residual variability in Slutsky). The algebraic sum of these two independent effects gives the ‘Fundamental Formula’ of value theory or ‘Slutsky Equation’ which ‘is clear of any reference to measurable utility’ (Hicks 1981: 4).

Welfare theory is the second field explicitly mentioned in the Nobel *Laudatio* for Hicks. Starting with Kaldor’s short but famous article ‘Welfare Propositions of Economics and Interpersonal Comparisons of Utility’ (Kaldor 1939), Hicks soon became one of the most important contributors to ‘New Welfare Economics’. At a time when cardinal utility was not accepted by many economists, Hicks, like Kaldor, proposed the compensation test, according to which Pareto improvements are possible when the welfare beneficiaries of a move from state A to state B could fully compensate any losers and still be better off. It was a distinctive feature of the Kaldor–Hicks criterion that it was enough that Pareto improvements were hypothetical, i.e. even if compensation did not actually take place. Hicks (1981: xiii) later openly conceded that at the time of formulating the compensation principle, he was not aware of the ‘Scitovsky paradox’, i.e. the criterion for an improvement is not necessarily reversible. The decisive point of the paradox is the fact that the relative valuations of a basket of goods depend on the way in which the basket is distributed.

The issue of welfare improvements is closely related to the problem of the measurement of real national income as an index of economic welfare. Hicks concluded that the two types of measurement of income, in terms of utility and in terms of cost, are quite different, and he rejected the utility approach to measure welfare. Unlike Arrow, Hicks never developed an interest in the formulation of a social welfare function, this probably also

²⁹See Hicks (1939a: appendix to Chapters II and III).

due to a lack of faith in the optimality of market processes and their results. A particularly controversial question is the measurement of capital, a problem to which Hicks made his most important contribution at the 1958 Corfu Conference of the International Economic Association on capital theory (see Hicks 1981: Chapter 8). Hicks took this problem up again in his subsequent books on capital (see Hicks 1965: Chapter 24, 1973a: Chapter 13).

In the 1940s, Hicks aimed at the rehabilitation of the Marshallian concept of consumers' surplus, i.e. the area under an individual's demand curve between two prices. In its revised Hicksian formulation with the famous compensating and equivalent variations, it had a great impact in subsequent cost–benefit analysis and other areas of applied economics aimed at measuring (approximately) changes in welfare. Hicks later noticed that the biggest shortcoming in his contemporary work on welfare economics was that it fell short of the 'revealed preference' theory developed by Samuelson (1948). It was Samuelson's approach which prompted Hicks to write his *A Revision of Demand Theory* (Hicks 1956b).

6 Sir John Versus J.R.

Clearly, I need to change my name. Let it be understood that *Value and Capital* (1939) was the work of J.R. Hicks, a "neoclassical" economist now deceased, while *Capital and Time* (Hicks 1973a)—and *A Theory of Economic History* (Hicks 1969)—are the work of John Hicks, a non-neoclassic who is quite disrespectful towards his "uncle" (Hicks 1975: 365).

With these words, Hicks made his conversion from J.R. to Sir John public.³⁰ Hicks himself dates his 1956 contribution 'Methods of Dynamic Analysis' to the Lindahl Festschrift as the 'turning point' (Hicks 1982: 9) of his own thinking. Thereafter, he increasingly kept his distance from the use that American and other neoclassicals, who never made friends with the work of Sir John, made of his earlier works rather than for the ideas he had developed himself and continuously re-examined and modified. Thus, he emphasised 'how important it (Hicks 1956a) is in explaining the development of my thought' (Hicks 1979b: 991). It therefore does not make much sense to distinguish between 'Hicks I', i.e. J.R. as the bad guy from the Dallas soap opera, and 'Hicks II' as the good guy, from a more heterodox perspective.

³⁰See also Pasinetti and Mariutti (2008) who clearly favour the work of the elder Hicks, the 'nephew', over the work of the younger Hicks, the 'uncle'. They rightly point out: 'He remained Hicks, in the sense that his independent mind always refused to be part of any school of thought' (ibid.: 66).

Although it is characteristic that Hicks himself dedicated his Nobel Lecture to ‘The Mainspring of Economic Growth’ (Hicks 1973c) and not to general equilibrium theory and/or welfare theory for which the Prize was granted, it is more rewarding to look at the work of Hicks from an evolutionary perspective. Thus, he was wrestling with the concept of time in economics during his whole life as an economist. Time plays a particular role when the economist thinks about how to handle dynamic problems. Hicks became increasingly dissatisfied about his own method ‘which ruined the “dynamic” theory of *Value and Capital*’ (Hicks 1977: vii). An excellent account of Hicks’s later thought is his 1976 contribution ‘Some Questions of Time in Economics’ to the Festschrift for Nicholas Georgescu-Roegen who himself had emphasised the irreversibility of time in his writings on the entropy law. Here, Hicks points out that, ‘It is because I want to make economics more human that I want to make it more time-conscious’ (Hicks 1976: 151). Whereas for a hardcore neoclassical general equilibrium theorist this view may end up in analytical nihilism, since in historical time the evolution of the system is unknowable in principle, Hicks struggled to find analytical ways to deal with time in economic theory.

In a similar way, Sir John continuously expressed his strong interest in methodology as in his late *Methods of Dynamic Economics* (Hicks 1985), a revised and enlarged version of the first part of *Capital and Growth*. In this line of thought, *Causality in Economics* (Hicks 1979c) plays a central role. Hicks elucidates that sequential causality, in which cause precedes effect in a causally relevant way, provides the decisive explanatory structure for his dynamic theories. Over the years, Hicks became more radical in his approach to economics. A characteristic example is the concluding chapter ‘A Discipline Not a Science’ of the three volumes of his *Collected Essays*. Here, Hicks (1983a: 375) expresses his agreement with Keynes that economic theory ‘is a method rather than a doctrine, a technique of thinking which helps its possessor to draw correct conclusions’. The elder Hicks had a quest for lessons of history and questions of economic substance much more than he was seeking for theorems in pure logical reasoning.

Throughout his life, Hicks kept a deep interest in capital theory: ‘Capital...is a very large subject, with many aspects; wherever one starts, it is hard to bring more than a few of them into view’, Hicks (1973a: v) writes in the Preface to the last volume of his famous trilogy *Value and Capital*, *Capital and Growth* and *Capital and Time*. He had finalised the manuscript of *Capital and Time* around the same time he received the Nobel Prize. Implicit in *Capital and Time* is the concept of the ‘Impulse’, which is elaborated in his Nobel Lecture (Hicks 1973c) and particularly in his subsequent essay on ‘Industrialism’ (Hicks 1977: Chapter 2).

The relevance of the time dimension is particularly important in the taking-up process of a new technology. In Chapter 16 of *Capital and Growth*, Hicks had pioneered a theory of the *traverse* focusing on dynamic impulses, such as changes in population growth or technology, causing out-of-equilibrium processes. The resulting structural change is a time-consuming process in which rigidities or bottlenecks, particularly in the production sphere, play an important role. The criticism raised by Charles Kennedy (1968) against the inadequate treatment of time in his embryonic traverse analysis based on a two-sector fixed coefficient or horizontal model in *Capital and Growth* was one major reason why Hicks switched to a ‘neo-Austrian’ or vertical model in *Capital and Time*.³¹ The decisive Austrian elements in Hicks’s ‘neo-Austrian’ theory are a focus on the time structure of the production process and the special treatment of capital goods as intermediate products in a vertical model. Capital goods are a medium for sequential production. By dealing explicitly with fixed capital goods,³² Hicks, in his neo-Austrian approach, in contrast to Böhm-Bawerk and Hayek, considers production processes to be of the flow input–flow output type. He saw the decisive advantage of his neo-Austrian method in its ability to cope with the important fact that process innovations normally involve the introduction of *new* capital goods. Horizontal approaches, on the other hand, illuminate intersectoral interdependencies, as best represented in input–output models. Both ways of disaggregating production structures in an economy, the vertical and the horizontal models therefore have their comparative (dis-)advantages. This led Hicks to explore both routes in which the economic system can adjust to dynamic impulses when it faces rigidities and bottlenecks. Iterating between the two approaches, Hicks finally took a complementary perspective, as indicated in Chapters 13 and 14 of his *Methods of Dynamic Economics* (Hicks 1985).

Beginning in the late 1960s, Hicks had become fascinated by the Ricardo machinery effect, i.e. the employment consequences of a different, more mechanised method of production. His traverse analysis in *Capital and Time* is an important attempt by a leading modern theorist ‘to clear up an ancient controversy’ (Hicks 1973a: 97): ‘The 1973 Hicks Fixwage model provides almost an exact replication of Ricardo’s assumptions; the real wage is fixed, labor supply is perfectly elastic, and employment...varies positively with saving’ (Burmeister 1974: 435). Hicks defended what he considered the core of Ricardo’s analysis. There exist important cases, ‘strongly forward-biased

³¹For a more detailed analysis, see Hagemann (2009).

³²See Burmeister (1974) for a more elaborate analysis.

innovations' in Hicks's terminology, in which the introduction of a new type of machinery may reduce both real output and employment in the short run. The detrimental effects continue to exist, but after a while the stronger investment spending, due to higher profits as a consequence of the more efficient new method of production, generates a higher rate of growth that eventually leads to an output and employment path above the reference path of the old equilibrium.³³ The Hicks of the 1973 vintage clearly is a precursor of the Samuelson of the 1989 vintage showing that 'Ricardo was Right!'

7 Conclusion

'Already, before I left LSE, I had done what I still feel to be some of my best work' (Hicks 1983a: 356). Hicks felt a lifelong commitment to LSE, the institution which made him an economist. This loyalty was confirmed by the fact that he donated his Nobel Prize money to the School's Library Appeal in 1973 as a gesture of gratitude: the donation was an important contribution towards the purchase of Strand House as the new location for the expanding LSE Library (see Dahrendorf 1995: 480). For its part, LSE decided to establish a Sir John Hicks Professorship of Economics, a position currently held by John Sutton. Previous holders include Lord Stern (1989–1993) and Michio Morishima (1982–1988), the latter of whom had help influence Part III, 'Optimum Growth', of Hicks's *Capital and Growth* in the early 1960s (see Hicks 1965: vii).

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³³For a more detailed analysis, see Hicks (1973a) and Hagemann (1994).

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Henry Phelps Brown (1906–1994)

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1 Introduction

Henry Phelps Brown was a twentieth-century labour economist, so it is necessary to begin with an explanation of the institutional context within which he was working during his LSE years. His research on trade unions and industrial relations, and on the historical movements of money wages, productivity, real wages and the wage share is outlined. Also, Phelps Brown's role as a precursor of the Phillips curve and as a devastating critic of the Cobb-Douglas production function is explained. After retiring to Oxford in 1970, he produced major studies on the inequality of pay, on the origins of trade union power and on egalitarianism, which are examined as a major component of his *oeuvre*. In conclusion, the critical acclaim, with which his work was received, is surveyed.

2 Brief Biography

Henry Phelps Brown was born in 1906, the son of a Wiltshire ironmonger. He won a scholarship to Wadham College, Oxford, and took First Class degrees in Modern History and in Philosophy, Politics and Economics (PPE).

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Subsequently, he travelled to the USA on a Rockefeller Travelling Fellowship and visited the universities of Michigan, Columbia and Chicago. He returned to a Lectureship in Economics at New College, Oxford, where, with the assistance of George Shackle, he investigated the historical statistical record to throw light on British business fluctuations.

At the outbreak of the Second World War, Phelps Brown enlisted in the Royal Artillery and went to France in November 1939. After evacuation from Dunkirk, he served as an anti-aircraft gunner during the Blitz. Later, he commanded a mobile battery, which fought its way up from Tunis to Monte Cassino, and ended the war as a Lieutenant Colonel in command of a regiment.

When demobilised, Phelps Brown returned to New College, before, in 1947, Lionel Robbins invited him to a Chair in the Economics of Labour at LSE. For the next twenty-one years, he conducted the research discussed below. He also devoted unstinting time to the diligent and kindly supervision of his graduate students. His reputation as a labour economist ensured his appointment, first, to the Committee on Prices and Productivity and later to the National Economic Development Council. In 1963, Phelps Brown headed a commission of inquiry to deal with a very disruptive industrial dispute involving London buses; its resolution led to higher pay and the introduction of one-man [sic] buses.

Phelps Brown retired from LSE at the end of 1968 and spent the next year in Australia visiting most of its universities. Afterwards, he returned to his Oxford home and produced four books and many articles, including five published in 1990 at the age of eighty-four. From 1974 to 1978, he served as a member of the Royal Commission on the Distribution of Income and Wealth. He was a Fellow of the British Academy and a President of the Royal Economic Society. His knighthood was conferred in 1976. Phelps Brown died in 1994. He was survived by his wife Evelyn and their three children.

3 Time at LSE

The Historical Perspective: It is vital to assess Phelps Brown's work in historical perspective. He was at LSE at a time when Keynesianism was in the ascendency; when exchange rates were fixed and controls imposed on foreign currency transactions; when fiscal policy and direct government intervention in economic outcomes were accepted by both Tory and Labour Parties; and when it was customary for economists to embrace economic history as part of their research programmes. It was before 'flexible labour markets'

were enthusiastically embraced by both political parties; it was before ‘globalisation’; it was before trade union power in Britain was dramatically constrained; and it was before mathematics and econometrics replaced economic history as principal complementary studies to economics.

Phelps Brown’s training as a historian naturally led him to accept the economic, social, industrial relations and political realities of the time for what they were and to apply his training as an economist to an understanding of them. His approach was exactly that specified by Paul Davidson: ‘[T]he purpose of the theory is to make the real world more intelligible, not to substitute an ideal world in place of it’ (Davidson 1990: 285). It was at a time when the Phillips curve was ‘discovered’ and came to play a key role in the macroeconomic policy debate. At that time, the theory of the distribution of national income between wages and profits was a central issue in economics, especially because of the unexplained stability in the wage share. This theory, though, was considered to be seriously wanting: writing in 1959, Kravis stated: ‘Despite the prominence of functional income distribution in the history of economic thought and the recent evidence of a revival of interest in the subject, the theory of distribution remains in a parlous state’ (Kravis 1959: 917). Speaking shortly afterwards, Scitovsky asserted that ‘The theory of income distribution is in a highly unsatisfactory and controversial state’ (Scitovsky 1964: 15).

The Cambridge controversies in capital theory also occurred at this time. This was a dispute over the meaning and measurement of capital and the validity of the neoclassical production function. The principal adversaries were located at the University of Cambridge and the Massachusetts Institute of Technology.

Phelps Brown made important contributions in all of the above areas of economic research.

The Institutional Framework of the (Non-Flexible) Labour Market:

A great part of the economics of labour consists accordingly of information about institutions and procedures through which the labour force is nurtured and trained, deployed between occupations and industries, and organized and directed at the place of work, and those through which the rates of pay are administered and negotiated (Phelps Brown 1962: 5).

Phelps Brown set about examining the development of trade unions, as a major labour market institution, in his book *The Growth of British Industrial Relations* (Phelps Brown 1959). This volume examines the growth and development of British unions and their interaction with employers’ organisations and the State. The starting point is the households of the workers: their

income, housing, education and working conditions. He was concerned to explain the origins of the industrial turmoil in the UK during the years 1906–1914, ‘one of the critical phases of tension and transition in our social history’ (ibid.: xxxv).

He noted the subdivision of work processes, which accompanied the doubling in the amount of equipment per worker between 1870 and 1914. This depreciated the versatility of craftsmen’s work, so that it could be performed by unskilled ‘machine men’ (ibid.: xv). Such repetitive work was the ‘most galling to human nature’ (ibid.: xvi). There had been some introduction of American scientific management: job analysis, time study, incentive methods of wage payment. Railway companies refused to recognise trade unions and employers resorted to blacklegs when their employees took strike action. The Taff Vale judgement of 1901 had ruled that a union was liable in tort for the damages incurred by a company because of a work stoppage. Consequently, unions thereafter faced the prospect that strike action could involve them in heavy damages. Another turning point came in 1906 with the election of a majority Liberal government. In the same year, a Trades Disputes Act declared that a trade union could not be sued ‘in respect of any tortious act alleged to have been committed by or on behalf of a trade union’ (Trade Disputes Act 1906: Section 4).

Phelps Brown regarded the prevailing practice of industry-wide bargaining with concern because it dealt almost exclusively with the determination of a standard rate of pay and provided little incentive for wage increases coupled to productivity-boosting changes in working practices. He saw firm-level bargaining as providing the appropriate arena for wage increases to be linked to removal, or modification, of restrictive labour practices. In a classic example ‘of practising what is preached’, he resolved a very disruptive London bus strike by awarding a pay increase in return for the agreement to introduce ‘one-man buses’ (Phelps Brown 1964: passim). This practice of linking increases in pay to the productivity-boosting relaxation of restrictive labour practices became known as ‘productivity bargaining’ and played a prominent role in the Wilson government’s incomes policy.

Finally, Phelps Brown was supportive of the British tradition of voluntarism in industrial relations: a laissez-faire approach with little legal regulation compared with American and European counterparts. In *The Growth of British Industrial Relations*, Phelps Brown stated:

[S]anctions are hard to apply to tens of thousands of men at once, and were they applicable would still create no willingness to work on terms regarded as unfair. But it is on willingness that good industrial relations depend:

if employers and employees are decent and reasonable there will be little trouble, and if they are not, Acts of Parliament will not make them so (Phelps Brown 1959: 358).

The volume was reviewed in the *Journal of Political Economy* by Simon Rottenberg, who considered it ‘a piece of meticulous history’ (Rottenberg 1961: 307).

Historical Analysis of Wage Movements: In conjunction with Sheila Hopkins, his long-term research assistant, Phelps Brown published five articles in *Economica* between 1955 and 1961, which have become known, collectively, as ‘Wages through the Ages’. His interest had been stimulated by a work of Meredith (1908), which showed the wage rates of a carpenter and an agricultural labourer from 1270 to 1880. Meredith had also calculated the amount of wheat which could be bought from these wages. In Phelps Brown and Hopkins’s first article (1955), they calculated the money wages of craftsmen and labourers in the building trades for the period 1264–1954. The coverage of the data was Oxford, London and Maidstone. They reported that at virtually no time over the seven centuries had money wages fallen and that there had been a stable relativity between the wages of craftsmen and labourers until the First World War. In the next article, Phelps Brown and Hopkins (1956) calculated ‘a composite physical unit of consumables’ to determine the purchasing power of the money wage series previously published. This series for ‘real’ wages showed rising prosperity from 1380 to 1510, followed by a steep decline during the sixteenth century: a craftsman’s money wage in 1630 could only buy two-fifths of what it had commanded in the fifteenth century. The next two articles (Phelps Brown and Hopkins 1957, 1961) extended the investigation to France, Alsace, Italy, Germany, Spain and Sweden. It was suggested that the fall in real wages during the sixteenth century was an international phenomenon, and was attributed to population pressure on food prices.

Malcolm Falkus (1996) notes that these articles played an important role in the reinterpretation of British medieval economic history, as evidenced by the work of Bridbury (1962), and that they were also influential in the work of Continental scholars such as Hammarström (1957) and Cipolla (1976).

Historical Analysis of Labour Productivity Movements:

We can think of the movement of real wages in an economy as dependent on two groups of factors: those which govern the movement of real income per head of the whole occupied population, which is the broadest measure of productivity, and those which decide the division of that income between wages and other shares (Phelps Brown 1957a: 48).

Phelps Brown set out to investigate both these determinants of the historical movement of real wages. He conducted this research on productivity with Stephen Handfield-Jones. Indexes of labour productivity and product wages were calculated for the period 1860 to 1920. This ensured that the reliability of the original data was cross-checked. 'For the most part, then, the two series bear each other out' (Phelps Brown and Handfield-Jones 1952: 273). There had been a check to product wage and productivity growth late in the nineteenth century, which they designated as a 'climacteric'. It occurred in Belgium, France, Germany, the UK and the USA.

The authors used Hoffman's index of British industrial production, which covered more than 40 industries from 1861 to 1913. There was declining output per operative in the late nineteenth century in coal mining and rail transport while cotton, beer, iron ore mining and iron and steel smelting experienced stagnation in output per operative at the end of the century. At the time, explanations stressed the inadequacies of British management and labour. Britain was seen to be disadvantaged relative to the USA because it lacked meritocratic management and suffered from virulent restrictive labour practices. Phelps Brown and Handfield-Jones rejected these hypotheses because substantial British productivity gains were subsequently achieved when the contrast between British and US labour and management practices was fundamentally unchanged. These differences kept productivity lower at all times, whereas it was necessary to identify recent changes which impacted on productivity growth, to find 'the active causes of the check to British productivity in the 1890s, which are more likely to be found in factors affecting Britain in common with the other countries, which experienced something of the same check' (ibid.: 281). They concluded that this common cause was that the application of inventions provided by steam and steel had reached their terminal stage:

[T]he previous rise had been carried forward by the massive application of Steam and Steel, which now had not much scope for extension; while the new techniques, especially of electricity, the internal combustion engine, and the new chemical processes, did not attain massive applications until after the First World War (ibid.: 283).

Phelps Brown and Handfield-Jones drew conclusions which represent a cautionary tale for model-builders and chancellors of the exchequer: 'We cannot count upon the advance of productivity along a constant growth curve. Should at any time our standard of living not much be raised for 20 years, that would be only what has happened in the lifetime of many people now living' (ibid.: 288). This stricture was reinforced in a subsequent paper by Phelps Brown and Weber:

[C]apital accumulation bears an uncertain relation to industrial productivity, the movements of which may be dominated for a quarter of a century at a time by the course of technical development, with little dependence on the contemporary rate of accumulation ... [I]t is not realistic in models of the expanding economy, to postulate any constant relation between the rates of growth of capital and output. It also shows the danger, in practical planning, of counting upon any sustained rate of rise of productivity (Phelps Brown and Weber 1953: 271).

Economic historians acclaimed this work:

[W]e return to the world of leading sectors—to Kuznets’s primary secular movements, to A.F. Burn’s study of sectoral retardation, and to Schumpeter’s heroic innovations—linked this time to trend movements in production growth rates and productivity rather than to prices. E.H. Phelps Brown, S.J. Handfield-Jones and Bernard Weber have led the way in this mode of analysis (Rostow 1975: 728).

Moreover:

The debate was...raised to a new level of both analysis and information in a succession of seminal contributions in the early 1950s by Phelps Brown. These added a striking new biological metaphor—climacteric—to the vocabulary of economic historians, and provided the first full and systematic analysis of the topic ... These early studies stimulated numerous distinguished contributions to the debate from both sides of the Atlantic (Feinstein 1990: 333–334).

Prelude to Phillips: Phelps Brown and Sheila Hopkins constructed indexes of money wage rates for France, Germany, Sweden, the UK and the USA over the period 1860–1939. The cyclical sensitivity of money wage rate movements was noted, especially in the UK: ‘The general pattern is a sharp rise in the average money wage-rate in the cyclical upswing, followed by a slower rate, a halt, or a fall, in the succeeding depression’ (Phelps Brown and Hopkins 1950: 233) and ‘Of all our countries, the UK is the one in which...its money wage rate shows more effect of the trade cycle than do those of other countries’ (ibid.: 231). The indicator used to determine the stage of the trade cycle was the percentage of trade union members who were unemployed. This means that Phelps Brown was reporting, for the UK, a relationship between the level of recorded unemployment and the rate of money wage increase. Does this sound familiar? Phillips’s classic article, published in 1958, was ‘The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957’

(Phillips 1958). Phelps Brown was a fundamental precursor of the Phillips curve and, along with James Meade and Richard Lipsey, is acknowledged by Phillips in a footnote on page 283. Phillips's money wage data were taken from the Phelps Brown/Hopkins work cited above. This research in economic history underpinned an econometric study which generated a flurry of empirical and theoretical research and played an important role in the anti-inflationary policy debate during the 1960s.

Significantly, Phelps Brown, the economic historian and industrial relations scholar, did not draw the conclusion that unemployment was the means to control inflation:

[T]he implications, that the inflationary rise of labour costs could be checked by restraining demand and increasing unemployment, by no means carried conviction with those who were following the course of negotiations in detail ... [T]he underlying analysis failed to correspond with what these observers saw going on under their eyes—such as the way in which claims came to be formulated, the considerations that activated employers, and the influence of a key bargain on the level of settlements throughout an annual wage round ... The presence of a greater number of unemployed men could inhibit the rise of wages directly only if there were a possibility of the employer substituting some of the unemployed at a lower wage for the existing employees, or using the threat of this to make those employees accept lower wages; but only in certain cases, mainly of unskilled labourers in the years before 1914, did that possibility exist (Phelps Brown 1983: 215).

The Neoclassical Production Function: The Cobb-Douglas production function had its origins in the objective of the American labour economist, Paul Douglas, to test the empirical validity of the marginal productivity theory of income distribution. He sought the assistance of the mathematician Charles Cobb to specify the form of production function which would produce constant shares of wages and profits. They then fitted this function to time series and cross-section data to test if the exponent on labour approximated the wage share, in which case, it was considered to be a verification of the marginal productivity theory of wages. A function, whose purpose was to show how the output of a given product would respond to different factor combinations at a given time, was fitted to output and input data relating to different points in time or different products.

The definitive criticism of this econometric pastime was provided by Phelps Brown (Phelps Brown 1957b). He pointed out that when the Cobb-Douglas production function is fitted to time series data the statistical series for labour, capital and output are quite likely to follow constant growth trends:

‘Thus each exponent simply expresses a relation between two differential rates of growth ... [T]hese rates are historical. The differences between them will not directly have the significance of exponents in a production function’ (ibid.: 550). Phelps Brown concluded that the fitting of the Cobb-Douglas function to a time series has not yielded, and cannot yield, the statistical realisation of a production function. It can describe the relations between the historical rates of growth of labour, capital and the product, but the coefficients that do this do not measure marginal productivity (ibid.: 551).

When the Cobb-Douglas production function is fitted to cross-section data, inter-industry differences in factor intensity determine the differences in net value product per unit of output. The difference in productivity as we move from labour-intensive industries to capital-intensive industries does not tell us anything about the consequences of varying factor intensity *within* an industry. Phelps Brown explained that there is a simple explanation for the marked empirical agreement, frequently found, between the share of wages and the exponent on labour in the fitted Cobb-Douglas production function. The output figures in these econometric functions are net value products that are composed of aggregate returns to capital plus aggregate labour earnings. It follows that if there is, across industries, a reasonable uniformity of wage and profit rates, the inter-industry in net value products simply reflects the inter-industry differences in factor proportions:

[T]he net products to which the Cobb-Douglas function is fitted would be made up of just the same rates of return to productive factors, and quantities of those factors, as also make up the income statistics; and when we calculate k by fitting the Cobb-Douglas function we are bound to arrive at the same value as when we reckon up total earnings and compare them with the total net product ... The Cobb-Douglas k and the share of earnings in income, will only be two sides of the same penny (ibid.: 557).

Opponents of neoclassical orthodoxy acclaimed his critique of Phelps Brown: ‘It must have taken an even tougher hide to survive Phelps Brown’s article on “The Meaning of the Fitted Cobb-Douglas Function”, than to ward off the Cambridge criticism of the Marginal Productivity Theory of Income Distribution’ (Robinson 1973: 154). In his Nobel Prize Lecture, Herbert Simon noted that the finding of labour exponents of ‘about the right magnitude’ in fitted Cobb-Douglas production functions cannot be taken as strong evidence for neoclassical theory: ‘[F]or the identical result can readily be produced by mistakenly fitting a Cobb-Douglas function to data that were in fact generated by a linear accounting identity (value of

goods equals labour cost plus capital cost)' (Simon 1979: 597). The reader is referred to Phelps Brown for elucidation.

The Wage Share Analysis: Phelps Brown developed his work on income shares principally in papers written with Peter Hart (Phelps Brown and Hart 1952) and Bernard Weber (Phelps Brown and Weber 1953). These analyses were later synthesised in the paper 'The Long-Term Movement of Real Wages' (Phelps Brown 1957a). Once again, his historian's background came into play. He did not start with a set of abstract assumptions and proceed to model building in the style of economists such as Kalecki (1954) or Kaldor (1955–1956). Instead, Phelps Brown started by establishing the historical record for the wage share. The articles with Hart and Weber involved data solely for the UK, whereas data for Belgium, France, Germany, Sweden and the USA were also presented in the 1957 paper.

For analytical purposes, income categories are classified into two groups: the rigid or stable sector, comprising salaries and rent, which 'follow general movements only with resistances and lags' (ibid.: 61), and the flexible sector, comprised of wages and profits, which can change readily in the short run but vary considerably over the trade cycle. Also, he calculated historical series for the wage-income ratio. This is the ratio of the average earnings of wage earners to national income per head of the working population. The share of wages in national income may vary because of a change in this wage-income ratio or simply be the consequence of a change in the proportion of wage earners in the working population. The wage-income ratio isolates the component of any wage share change that represents a movement in relative class incomes and not in relative class numbers. Also, if aggregate wage statistics are unavailable, it is a valuable technique for producing lengthy historical series indicating the course of distribution.

The wage-income ratio exhibited a regular countercyclical pattern. Phelps Brown explained that this was the outcome of a 'ratchet' effect whereby money wages were downwardly rigid in recession. This phenomenon was present long before 1914 and therefore was independent of trade unionism (ibid.: 51). Shifts between the rigid and flexible sectors were generally the outcome of large and rapid changes in money income. In particular, the combined share of wages and profits in UK national income rose during both world wars.

The important contribution to wage share analysis which Phelps Brown made is contained in the explanation of displacements within the flexible sector. The wage/profits relationship was found to exhibit considerable historical inertia but interrupted by minor and major displacements (ibid.: 63–64). This was observed in the behaviour of the wage-income ratio.

The explanation put forward for these occasional displacements in basically stable series arises from Phelps Brown's work with Weber: 'The estimates show three relations with some stability: (a) capital has grown at about the same rate as income; (b) over each year 1870–1913 and 1924–38 earnings remained much the same proportion of national income; (c) in each span the rate of return on capital was stable' (Phelps Brown and Weber 1953: 282). In other words, the capital-output ratio, the profit rate and the profit share were stable in the UK over the period 1870–1938. If any two of these ratios are stable, it means that the third is also stable. Stability in the capital-output ratio produces a simple direct relationship between movements in the profit share and movements in the profit rate. Phelps Brown suggested that entrepreneurial decisions are guided by two conventions, 'one prescribing the reasonable or normal rate of profit which firms will endeavour to realise and the other guiding its opinion on the probable consequences of a change in selling price and constituting a market environment' (Phelps Brown 1957a: 63). Following Keynes, he suggested that a natural response to uncertainty is that a conventional judgement coalesces about what is a normal rate of profit, and that pricing and output decisions are made to this end. Keynes quoted thus:

Knowing our own individual judgement is worthless, we endeavour to fall back on the judgement of the rest of the world which is better informed. That is, we endeavour to conform with the behaviour of the majority or the average ... [This] leads to what we may strictly term a conventional judgement (Keynes quoted in *ibid.*: 60).

A second convention: the 'market environment' explains displacements in income shares, profit rates and wage-income ratios. The expression 'market environment' was taken from Alfred Marshall's evidence to the Gold and Silver Commission (Phelps Brown and Hart 1952: 272, fns. 1 and 2). It concerns the consequences of changing selling prices and explains why shifts in income share are sometimes related to trade union strength or weakness:

If union strength tends to raise the share of wages in national income, we have to explain why it did this in 1870–1872 and 1888–1899, but not in 1909–1913 and 1946–1950. If union weakness tends to lower the wage share we have to explain why it did so in 1903–1905 and 1926–1928, but not in 1879–1881 (*ibid.*: 269).

The explanation arises from the state of the 'market environment': if it is 'soft' or 'hard'. This convention, concerning the possibility of passing on

higher costs in higher prices, is the outcome of several factors, such as 'the rate of massive application of new techniques; alternations between development of sources and "reaping the harvest" of primary products; the supply of money; the extent of combination; and the "policy of Europe"' (ibid.: 272). The application of convention in an uncertain world, combined with an elastic supply of capital, accounts for inertia in profit rates and income shares; however, displacement occurs if the right circumstances of trade union activity and product market environment coincide. When trade union strength coincides with a 'hard market environment', as in 1870–1872 and 1889–1891, this will squeeze profit margins. On the other hand, if union weakness is coincident with a 'soft market environment', profit margins are able to widen, as in 1903–1905 and 1926–1928. The other two combinations of strong trade unions with a 'soft market environment' and weak trade unions with a 'hard market environment' will result in inertia in income shares.

An extensive survey of income distribution theory was published by Rothschild in 1961 (Rothschild 1961). He covered the theories of Boulding, Kaldor, Kalecki, Krelle, Marchal and Schneider. His assessment was that the analysis of Phelps Brown and Hart was consistent with the Kaleckian framework. Trade union action is present in both theories, and Phelps Brown's 'market environment' encompasses a number of phenomena which are included in Kalecki's 'degree of monopoly': technical change, cartel agreements, monetary policy (Kalecki 1954: 17–19). Both assume 'cost determined' prices: 'The firm must make sure that price does not become too high in relation to prices of other firms, for this will drastically reduce sales, and that the price does not become too low in relation to its average prime cost, for this would drastically reduce the profit margin' (Kalecki 1971: 44–45) and 'A manufacturer whose selling prices yield him for a time profits so big as to attract greater competition brings much trouble on himself; but if he sets his price low he may well be leaving too little margin for contingencies, or failing to earn as much as shareholders expect' (Phelps Brown 1957a: 59). There is a clear parallel in the two approaches, and Kalecki, like Phelps Brown, made a point of applying his analysis to the historical record (Kalecki 1954: 32–41).

The Phelps Brown analysis is more in the nature of a classification to explain past events rather than a guide to the future. It can, however, be a valuable tool in analysing wage share shifts in other times and other places. One example was the 'real wage overhang', which was a cause celebre in Australia in the mid-1970s. This term referred to an increase in real wages relative to labour productivity, so it was simply another way of stating that the wage share in national income had risen. In 1974, the share

of wages in the non-farm sector of national income rose by approximately five percentage points and remained at a higher level for several years. This real wage overhang was accompanied by an increase in unemployment, so many Australian economists saw it as a vindication of their neoclassical model, i.e. attributing a direct causal link from higher real wages (relative to productivity) to reduced employment: ‘One must be willing to change one’s model with the times. It is Pigou rather than Keynes that [sic] is relevant now’ (Corden 1977: 30). The current writer, having been a student of Phelps Brown, was aware that sudden displacements in the wage share were rare and needed to be scrutinised for their origins. In other words, it was wise to investigate the *origins* of the real wage overhang before attributing a *direct causal connection* to reduced employment. In December 1972, a Labor government was elected in Australia for the first time in 23 years. Money wages rose substantially in 1974, very much at the instigation of the new administration and, in particular, the Minister for Labor, Clyde Cameron: ‘[A]s a Government, we have taken a vigorous role in successfully arguing for substantial increases in the Minimum Wage and its extension to adult females, for equal pay for women and for flat-rate-across the board National Wage Case increases’ (Cameron 1975: 4). Here, we have Phelps Brown’s ‘trade union strength’ manifested through the political process, coinciding with a ‘hard market environment’, which was likewise instigated by the new Labor government. This involved increased competitiveness for the import-competing sector, contraction in the money supply and intervention in product price setting. In July 1973, there was a 25% across-the-board cut in tariffs, and between December 1972 and September 1974, the exchange rate was appreciated by more than 20%. In the four-quarter period Q4 1973 to Q3 1974, the money supply, as measured by M1 (seasonally adjusted), fell and in the last quarter M3 (seasonally adjusted) also contracted. When deflated by the non-farm GDP implicit price deflator, this decline in M1 represented a reduction in the money supply in real terms of over 20%. A Prices Justification Tribunal began to operate in August 1973. Firms with annual sales in excess of \$20 million had to inform the Tribunal of proposed price increases, after which the Tribunal decided if a public inquiry was necessary. A total of 500 major companies and their subsidiaries were under the Tribunal’s supervision, and in the first 20 months of its operation, none defied its decisions.

So, we have a situation where wage pressure produced by the political wing of the trade union movement coincided with a ‘market environment’ in which product prices were constrained by government policy initiatives. These actions which created a ‘hard market environment’ would also have

impacted *directly* on aggregate demand: depressing exports and investment while facilitating imports. The reduction in employment was all perfectly explicable in Keynesian terms and there was no need to disinter Pigou (see Riach and Richards 1979).

In lectures given at the University of Manchester in 1968 and published as *Pay and Profits: The Theory of Distribution Reviewed in the Light of the Behaviour of Some Western Economies over the Last Hundred Years*, Phelps Brown summed up the implications of his empirical investigations:

[A] theory of distribution that finds its mainspring in the prevailing pressure of enterprise and investible funds to enter production at rates of profit accepted and in part upheld by convention. On one view, it might be seen simply as inverting the Marxian theory of distribution: instead of the elasticity of supply of labour holding down real wages, so that the increase in the output per worker accrues to the capitalist, in the present theory it is the elasticity of supply of capital that holds down the rate of profit, and real wages rise in proportion to the output per worker. At any rate, that is what has happened (Phelps Brown 1968: 53).

It is curious that in a book of around 700 pages, Piketty (2014) does not once make reference to Phelps Brown.

4 In 'Retirement' at Oxford

Phelps Brown continued his interdisciplinary research during his years at Oxford. *The Inequality of Pay* was published in 1977. Phelps Brown's approach, reflecting his historian's training, is a detailed examination of international evidence of pay structures and determinants. Economic, sociological, anthropological and genetic data are reviewed in the quest to explain differences in pay. In particular, he assesses the relative usefulness of economic and sociological explanations of such differences. His conclusion is that these two approaches are complementary: '[E]conomists who have studied labour questions have never doubted the influence of custom and notions of fairness, or the power of combinations, and sociologists likewise have been well aware of the effects of scarcity on the relative pay of different groups' (Phelps Brown 1977: 21).

There was a notable uniformity in the occupational wage structure of countries in the capitalist West and those in the Eastern Bloc, the one exception being the Israeli kibbutzim. Phelps Brown makes an important distinction 'between factors affecting the pay structure *before* the market

and those affecting it *within* the market' (ibid.: 322; italics in original). Differences in the socio-economic class of parents affect (*before* the market) the potential occupational earnings of children: the development of pre-school children's cognitive ability is better stimulated in the homes of non-manual workers; parents with higher incomes are better able to support their children through extended education and professional training. Such parents and their contacts can acquaint children with careers beyond the purview of poorer families. Once the supply to the labour market is so influenced, then (*within* the market) 'the relative rates of pay that given levels of qualifications can command are determined for the most part by the market forces of supply and demand' (ibid.: 324). He examines evidence on rates of return to education in order to assess the validity of human capital theory and concludes 'that the analogy between educating a human being and building a machine is defective' (ibid.: 245).

Andrew Glyn reported that *The Inequality of Pay* was 'an excellent book, covering the whole field of the structure of pay...and is convincing in [its] explanations of how many apparently customary differentials are really based on market factors' (Glyn 1978: 180).

The Origins of Trade Union Power, which was published in 1983, represented a return to the field of industrial relations/trade union history which Phelps Brown had previously pursued in his 1959 publication, *The Growth of British Industrial Relations*. Much had happened in the twenty-four-year interim: there had been the Wilson government's excursion into incomes policy; there had been the industrial turmoil of the Heath years, culminating in the three-day week; there had been the social contract and Winter of Discontent during the government of James Callaghan. The time was ripe for a rethink.

There had been a substantial increase in the power of trade unions, and their propensity to use it, between these two dates. In *The Origins*, Phelps Brown devotes a chapter to cost push: the thesis that inflation can be generated from the supply side, principally by trade unions, independently of the state of demand. In the 1950s, an annual wage round emerged in the UK, and with it the acceptance of a going rate. Hence, an individual employer was secured in settling for it in the knowledge that he would not be put at a competitive disadvantage. Phelps Brown posed the following questions to the sceptics of cost push, i.e. those who saw inflation inevitably originating from the demand side of the labour market:

If the rise of pay was due to the pull of excess demand, why did pay rise as much in the contracting as in the expanding industries? If the rate at which

pay rose must be expected to vary with the margin of unemployment in the labour force concerned, why did pay rise just as much—or more—in the regions of the United Kingdom where considerable unemployment remained, as in those where it was slight? (Phelps Brown 1983: 153).

Trade unions were at a tactical advantage where firms, in computer-aided efficiency mode, carried limited stocks of intermediate products and cash. Any interruption to production, when strike action was threatened, could put firm survival at risk, so the seeds for inflationary wage increases were sown. The monetary authorities were also implicated as they validated this cost pressure by increasing the money supply, so as to maintain employment (*ibid.*: 154–155). Several European countries showed an acceleration in wage growth around 1969. Phelps Brown puts this down to the strikes of 1968–1970, originating from younger members of the union rank and file, whose actions were not moderated by the experience of unemployment, as was the case for their elders. These strikes took the form of unofficial action: ‘Even in Germany there was an outbreak of unofficial action in the iron and steel industry at this time’ (*ibid.*: 160). Phelps Brown confronts the issue of stagflation and the difficulty of dealing with it via traditional monetary and fiscal policy; he sees virtue in Dutch and Scandinavian practices to contain wage pressure. This involves trade unions and employers meeting collectively at national level to determine a norm for a general pay rise and exceptions from this norm to meet special circumstances of particular sectors or industries. In this way, it is hoped to avoid the competitive leapfrogging inherent in disaggregated wage bargaining (see *ibid.*: 163).

In *The Origins of Trade Union Power*, Phelps Brown draws extensively on international experience to formulate his recommendations for industrial relations reform in the UK. There is frequent reference to the experience of European countries and specific chapters on Australia, Canada and the USA. He is supportive of the role of unions in a capitalist world:

[T]he workings of the market and the organization of production compose a system of forces which, if left to themselves, would often bear hard on the worker. The function of the trade unions, seen in this setting, is to protect their members from the bruising impact of forces impinging upon the working life (*ibid.*: 289).

Nevertheless, he is forced to conclude that in the quest for full employment without inflation ‘incomes policy is inescapable: but it also proved impracticable. Again and again, after a time it has broken down. Behind the circumstances of particular episodes, a general cause can be discerned. A number

of trade unions always have the power to breach the line that policy is holding' (ibid.: 295). This is the 'Jeremiah-style' conclusion of a civilised labour economist who regarded the existence of trade unions and the maintenance of full employment as a *fait accompli*. Instead, what came to be embraced in Britain was 'the flexible labour market' and 'the incomes policy of Karl Marx' (Balogh 1982: 178). Phelps Brown himself lamented: 'This work had the misfortune to appear at a time...with social changes that reduced trade union power, both industrially and as a part of the political labour movement, in a number of western countries, not least, in the United Kingdom' (Phelps Brown 1983: 138).

Egalitarianism and the Generation of Inequality was published in 1988, Phelps Brown's eighty-third year. It combines a detailed history of egalitarian thought with an extensive survey of the data on the distribution of income and wealth. In Part I, 'The Rise of Egalitarianism', the development of views on equality is traced from Plato and Aristotle to the mid-1940s. Along the way, the reader encounters the arguments and beliefs of, amongst others, Aquinas, the Gospels, Machiavelli, More, Locke, Rousseau, Jefferson, Paine, Condorcet, Adam Smith, Marx and Henry George. In Part III, 'Egalitarianism Analysed and Assessed', the arguments of Rawls are outlined and assessed.

Phelps Brown's own views on the achievement of equality for half of humankind are exemplified by the following quotations:

Whatever it is that IQ tests measure, women have been found to have as much of it as men, and that lays to rest an old presumption of masculine superiority. The strictures of St Paul carry less authority than they did. In two world wars women showed their ability to perform tasks that it was once thought only men could do (Phelps Brown 1988: 520).

Referring to women's greater freedom and independence subsequent to the world wars, and the availability of reliable contraception: 'If one outcome has been the more frequent breakup of marriage, this may indicate the extent to which women had been held in subjection within marriage before' (ibid.: 239). And: '[T]he changes that did in fact occur in the hundred years after Mary Wollstonecraft wrote her "Vindication of the Rights of Women" effected a great reduction in the sum of total human inequality, a sum we underestimate altogether if we reckon it only in money' (ibid.).

Part II is an examination of the data on income and wealth which is prodigious in its geographical and historical coverage. Data on the distribution of personal income are provided, and analysed, for European, African, South America, Asian and Soviet countries, as well as Australia, Canada and the USA. As an example:

The fact remains that the three Soviet-type distributions we have sampled (Czechoslovakia, Hungary and the USSR) are much more egalitarian than the Western type ... [T]he more skilled manual occupations and still more the higher clerical, the professional and administrative are paid less than in the West relatively to the bulk of manual workers ... The effect arises not from egalitarian conviction...but from giving priority to raising the standard of living of the main guard of manual workers (*ibid.*: 303–304).

The historical coverage is no less impressive. For the UK and its components, distributions in various formats are presented from 1688 to 1978–1979. The methods and difficulties involved in estimating the distribution of wealth are outlined, and the problems for any meaningful international comparison are stressed. Phelps Brown finds differences in the wealth distributions of Canada, the UK and the USA, but nevertheless concludes: ‘[T]he distribution as a whole is of the same highly concentrated form as in Britain. The degree of concentration of wealth is everywhere far higher than that of income’ (*ibid.*: 361).

After his extensive investigation of the statistical record, Phelps Brown concludes: ‘[T]he distribution of income and wealth are resistant to great imposed changes’ (*ibid.*: 475). But, significantly, he has a suggestion: ‘[H]uman nature can be changed, and life-paths can be smoothed. People change, in their personality and capability, with the conditions of their upbringing, education and training. Here are many possibilities for intervention ... [W]e need to consider where intervention promises to be most effective in raising life chances’ (*ibid.*: 476). It is in a child’s early years that he identifies the most fruitful place for action: ‘What stands out here, when we consider the possibilities of intervention, is the relative importance of the home’ (*ibid.*: 477). ‘Any teacher meeting a class of entrants to a first school at five years of age can have no doubt of the wide range of character, and of ability to learn, already present there’ (*ibid.*). ‘But it is harder to see how to raise the potential that is formed in early childhood’ (*ibid.*). He sees some potential for the health services in spreading information about children’s needs, but in particular, he applauds the role of playgroups in changing parental attitudes as well as in developing children. Phelps Brown acknowledges that, ‘On traditional ways of thinking, such matters lie outside the purview of economists; but they are in fact basic to the issue of how to enable each person to develop his or her full economic potential’ (*ibid.*: 478). This is the interdisciplinary man speaking: the interdisciplinary man who is apparent in all his writings on the labour market. Phelps Brown would be the first to acknowledge that his thinking on this issue had benefited hugely from life with his wife, Evelyn Phelps Brown (née Bowlby). For example,

The Inequality of Pay is dedicated to her: ‘To Evelyn; whose compassionate understanding of the under 5s and unceasing work for them is helping to remove a major source of needless inequality’.

Once again, however, Phelps Brown had to lament the timing of publication, this time with respect to *Egalitarianism and the Generation of Inequality*: ‘As I write in September 1987, it appears that this work...will be published in 1988. Once more it seems that I shall have been left stranded by a turn in the tide of public opinion, so rapid has been the recession of egalitarian sentiment in this country since 1979’ (Phelps Brown 1988: 138–139). This aside, ‘The origins of the West’s fascination with equality are portrayed brilliantly, emphatically, and succinctly in Phelps Brown’s magisterial study’ (Lebergott 1990: 254), was Stanley Lebergott’s assessment in his review of the volume.

5 Conclusion

In his review of *The Inequality of Pay*, Martin Bronfenbrenner observes: ‘My academic generation met Professor Phelps Brown as a general equilibrium theorist, author of what was then the only usable introduction in English for the struggling graduate student to the special insights of the Lausanne School’ (Bronfenbrenner 1979: 608). Subsequently, Phelps Brown turned from abstract theory and mathematics to the applied economics of the labour market, which he complemented with history. He served LSE as an academic labour economist of the 1950s and 1960s and was widely regarded as a leading member of that cohort: ‘[H]e was one of the most outstanding economists of his time’ (Cairncross 1996: 141).

We saw above that Phelps Brown’s contribution to economic history was acknowledged by history scholars such as Rostow and Feinstein; Phyllis Deane, reviewing a volume which reprinted a number of his research papers, wrote ‘[T]here could be no better manual to put in the hands of students contemplating research in economic history’ (Deane quoted in Worswick 1996: 155). Among his work on economic history, Phelps Brown demonstrated that real wages fell in several European countries during the sixteenth century, and that there was a check to their rate of growth late in the nineteenth century, which he termed the ‘climacteric’. Also, he recognised the relationship between the rate of money wage increase and the level of unemployment several years before Phillips published his classic article.

Eminent theoreticians, Joan Robinson and Herbert Simon, acclaimed his critique of the fitted Cobb-Douglas production function, which fundamentally undermined the neoclassical explanation for stable factor shares.

Rothschild and Scitovsky both included Phelps Brown's neo-Kaleckian explanation for wage share determination prominently in their surveys of income distribution theory. Meanwhile, Tawney said of Phelps Brown's writings on inequality: '[H]e combines an unpretentious brevity with depth and thought and brings to his lectures an unassuming subtlety and insight' (Tawney quoted in Cairncross 1996: 144). Moreover, Phelps Brown's profound knowledge of trade union/industrial relations history, his research on the distribution of income and his contribution to the debate on anti-inflationary policy led to his appointment to several important government commissions.

During his Oxford years, after his retirement from LSE, Phelps Brown published two papers highly critical of the state of contemporary economic theory, which rested on assumptions about economic agents he considered arbitrary (Phelps Brown 1972). He did not approve of the pursuit of pure theory for its own sake and objected to economists who were 'more attracted by the intellectual difficulty of problems than by any application to practice' (Phelps Brown 1980: 4). In 2008, during a visit to LSE, the Queen asked why no one had foreseen the financial crash. One answer to this query is that economists no longer have economic history as part of their training. Joseph Schumpeter's advice of 1939 has been ignored. In *Business Cycles*, he advised about the importance of an historical approach to problems of the cyclical process: 'It is obvious that only detailed historic knowledge can definitively answer most of the questions of individual causation and mechanism and that without it the study of time series must remain inconclusive, and theoretical analysis empty' (Schumpeter 1939: 220). Phelps Brown was firmly in the Schumpeterian tradition.

Henry Phelps Brown's writings are not currently fashionable, but if public policy ever returns to a pursuit of full employment, more equality in the distribution of income and an acceptance of the role of trade unions in the protection of workers' rights, it will be time for a comeback.

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19

Evan Durbin (1906–1948)

Catherine Ellis

1 Introduction

Evan Durbin was born in 1906 into a devout Baptist family in Devonshire, the son of the Reverend Frank Durbin and his wife, Mary Louisa Mellor Mottram, daughter of William Mottram, a well-known Congregationalist and temperance campaigner. He enjoyed a happy childhood and grew up confident and articulate in a home where politics and religion were discussed both passionately and ‘without claim of prescriptive wisdom by the elders’ (Phelps Brown 1951: 91). After attending Taunton School, Durbin won an Open Scholarship to New College, Oxford, where he shared rooms with two other young economists, Reginald Bassett and his former schoolmate Henry Phelps Brown, both of whom were also later employed at LSE. Durbin completed a Second Class degree in Zoology in 1927 and was relieved to reach the end of this foray into the sciences. He transferred with greater enthusiasm into Philosophy, Politics and Economics—Modern Greats—from which he graduated with a First Class degree two years later.¹

¹Biographical details throughout this chapter are drawn from Phelps Brown (1951), Ellis (2004a), Durbin (2008) and ‘Marjorie Durban [sic]’, transcript of interview by Keith Hancock for *Economic Journal*, 1995, Durbin Papers, BLPES Archives: COLL MISC 0978.

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Although ‘born and bred a Liberal’, Durbin was firmly committed to socialism by the time he arrived at Oxford.² He was active in the Oxford Union, where he and Phelps Brown were among the few socialists, as well as the Adam Smith Society and the Labour Club, which together facilitated his introduction to many of the people who strongly influenced his later work, including Hugh Gaitskell, Margaret and G.D.H. Cole, John Bowlby and Lionel Robbins, who was Durbin’s economics tutor at New College (see Durbin 1985: 99; Howson 2011: 128–129; Mayhew 2006: 24–25). Although his widow recalled that Durbin was later ‘horrified’ by the rightward shift in Robbins’s thought, as an undergraduate Durbin greatly admired his tutor and thanked him for the gift not only of ‘tuition as good as tuition can be; but an enthusiasm for your subject—my subject—and a personal interest that has been no part of your official duties’ (Howson 2011: 152; Durbin to Robbins, 24 August [1929?], Robbins Papers, BLPES Archives: 3/1/1).

The admiration was mutual. By 1928, Robbins was pleased to recommend his student as an adult education teacher, noting both Durbin’s academic success and the kindness and fondness for debate for which Durbin was known throughout his life: ‘He is quick to understand and very sympathetic, and the sort of argumentation so beloved of extra mural students, is the breath of life to him’ (Robbins to unknown, 3 November 1928, Durbin Papers, BLPES Archives: 3/4). Robbins’s support helped Durbin secure a Ricardo Scholarship to study under-consumptionist theories at University College London, in 1929, the same year in which Robbins was appointed head of LSE’s Economics Department. During this period of rapid expansion at the School under William Beveridge’s leadership, Robbins wasted no time recruiting economists who contributed diverse perspectives on the field. In the autumn of 1930, Robbins appointed Durbin to a lectureship in his department, where the new recruit worked alongside R.H. Tawney, Harold Laski, Friedrich Hayek and Eileen Power, among others (see Howson 2011: 170–171; Durbin 1985: 100–101).

The School remained Durbin’s professional home until he joined the war-time civil service in early 1940. He worked in a variety of posts throughout the war, most notably as assistant to Labour leader and Deputy Prime Minister Clement Attlee. In 1945, Durbin was elected Labour MP for

²Durbin Papers, BLPES Archives: 4/7, ‘Socialism and the Liberal Tradition’, n.d. [1935–1936]; Brooke (1996: 32). Durbin’s widow, Marjorie, believed it was Phelps Brown who persuaded Durbin to join the Labour Party, a decision that greatly upset Durbin’s mother who ‘thought she was breeding young liberals’ (Durbin Papers, BLPES Archives: ‘Marjorie Durban [sic]’, COLL MISC 0978: 88, 98).

the London constituency of Edmonton. In government, he served first as Parliamentary Private Secretary to Hugh Dalton, the Chancellor of the Exchequer, and then as Parliamentary Secretary in the Ministry of Works. He was tipped for higher office but drowned accidentally in September 1948 at the age of 42, leaving behind a widow and three young children.

Throughout his career, Durbin was a socialist first, an economist second. His commitment to democratic socialism shaped every facet of his economic thought and he considered economic theory to have little value or utility unless it was directed towards the creation of a more humane and equitable society. As he observed in one of his final publications, economists ‘must realise that they are studying human behaviour and not the formulae of logic and mathematics’ (Durbin 1949a: 175; see also Durbin 1949b). Despite his tragically early death, Durbin left an indelible mark on the Labour Party. In the 1930s, his work was central to Labour’s adoption of economic planning in lieu of large-scale nationalisation. During and after the war, Durbin championed a socialist planned economy that maximised individual liberty and rejected sectional interests such as those of trade unions. His distinctive formulation of socialism melded economics with ethics and insights drawn from psychology and psychoanalysis, underpinned by a strong belief in the superiority of English values and institutions. Durbin also drew attention to issues that would plague socialists well into the 1950s and beyond, particularly affluence, voter psychology and managerialism.

2 Planners and Planning

Durbin’s decade at LSE before the war was the most fruitful of his career, both in the breadth of his economic research and in the scope of his contributions to the Labour Party. He rapidly established his credentials as an academic and socialist economist with the publication of three books: *Purchasing Power and Trade Depression* (Durbin 1933a), *Socialist Credit Policy* (Durbin 1933b; revised 1935d) and *The Problem of Credit Policy* (Durbin 1935a). *The Problem of Credit Policy* sold moderately well for the remainder of the decade and Durbin developed a strong reputation as both a colleague and a lecturer.³

Notwithstanding his early success at the School, Durbin’s most significant contributions were more practical and political than theoretical or academic.

³Durbin Papers, BLPES Archives: 2/2, handwritten sales figures and graph, n.d.

He recognised that the second Labour government (which collapsed acrimoniously in August 1931) had been fatally reliant on orthodox finance, but unlike some socialists, such as Harold Laski and John Strachey, Durbin rejected Marxism as a viable alternative. Instead, he was instrumental in convincing the Labour leadership to reconfirm the party's commitment to democratic socialism supported by a programme of comprehensive economic planning and limited public ownership.

Much of Durbin's research in the early 1930s focused on the mechanics of planned economies, which he developed through three interconnected groups of economists and intellectuals.⁴ The first was the New Fabian Research Bureau (NFRB), which was founded in March 1931 in response to frustration with Labour in government and the impotence of the original Fabian Society. The NFRB's papers covered a range of topics but their focus was most often economic, a reflection of both its early membership, which included Durbin, Dalton, Gaitskell, Barbara Wootton, James Meade and Colin Clark, and the pressing need to revisit Labour's economic policies after 1931. The second group, the XYZ Club, began to meet regularly in January 1932 in rooms above a London pub. XYZ membership overlapped substantially with that of the NFRB but added emerging figures from the press and business worlds such as Douglas Jay, Nicholas Davenport and Vaughan Berry, who filled gaps in Labour's expertise on finance and the workings of the City. Finally, Dalton, chairman of Labour's influential Finance and Trade Committee, invited Durbin and several other young economists to develop policies that became the basis of Labour's new programmes in 1934 and 1937 and supported Durbin's contention that by the middle of the decade, Labour had become 'unquestionably a planning Party' (Durbin 1985: 80–83; Brooke 1992: 28; Durbin 1949a: 41).

In an essay first published in George Catlin's collection, *New Trends in Socialism* (Durbin 1935b), Durbin outlined the case for centralised controls as an essential step on the road to a socialist society. He anticipated that a future Labour government would enact controls in two stages, first 'grouping...production units making the same or closely related products into one corporation' and then bringing together groups of economic activities and industries under a new 'Supreme Economic Authority'. The result would be both greater efficiency and more equitable distribution of resources.

⁴As Ann Oakley has observed, it is 'impossible to read the intellectual and political history of the 1930s and 1940s without being impressed by the overlapping membership of the different circles participating in the debates and decisions which produced post-war Britain' (Oakley 2011: 162).

Democratic socialist planning should not, however, be confused with ‘*a Plan*’. In the absence of an ‘economic astronomer’, Durbin rejected any programme based on precise predictions of human activity and industrial production (Durbin 1949a: 43–44; italics in original). Moreover, in Durbin’s model, surpluses in socialised industries must belong to the State, not the workers. This was a contentious issue for the labour movement, but Durbin insisted from the outset that planning required workers to put national above sectional demands:

The organised workers who claim with justice that the interests of the community should not be over-ridden for the profits of the few should go on to add that those same interests should not be overridden for the wages of the few. The interests of the whole are sovereign over the interests of the part (ibid.: 56–57).

For Durbin, planning placed the onus for creating a socialist community on the State, but the State must in turn take responsibility for ensuring the cooperation of all its constituent parts. He would return to this issue after the war.

3 Economics and Ethics

Durbin believed that planning was essential to economic efficiency, but he also recognised that simply shifting power mechanically from private hands to the State was not sufficient to create a socialist society. Economic controls were merely ‘a means to an end’ (ibid.: 45). As Durbin elaborated with increasing vigour for the remainder of the 1930s, planning was much more than an economic endeavour; it was the foundation of a democratic community based on common humanity, fellowship and equality. As Jeremy Nuttall and Mathew Thomson have shown, Durbin’s work demonstrates that the ethical imperatives of British socialism remained very much alive during the Depression years despite the gradual shift to a more technocratic, Fabian-led approach throughout this period. Economics and ethics did not become ‘alternative creeds’ in this period, as earlier studies often argued (e.g. Macintyre 1980: 52–53), but continued to play a vital role in both economic and political spheres, and socialist moralism was ultimately strengthened by the popularity and success of large-scale economic planning (see Nuttall 2003; Thomson 2006).

The distinctively ethical vision within Durbin’s economic thought was evident in his responses to Keynes’s work. In the first half of the 1930s, Keynes’s theories aroused considerable controversy among economists of

many political persuasions, and the 'Hayek-Robbins nexus' at LSE became a focal point of opposition. Durbin and others in the NFRB and XYZ Club engaged actively in the debates. Douglas Jay became an early convert to demand management as a riposte to calls for greater public ownership but he was not joined by other socialist economists until *The General Theory* (Keynes 1936 [1973]) converted many earlier sceptics, including Meade and Clark (see Howson 1988: 547–548; Durbin 1985: 69–70, 106, 149–150).

Durbin was not among them. He remained unconvinced on both technical and moral grounds. On a theoretical level, he found that Keynesian models could not explain the phenomenon of trade cycles and were therefore far less effective than centralised planning to achieve long-term economic growth. More significantly, Durbin was suspicious of Keynes's Liberal roots and troubled by his apparent indifference to ethical concerns (see *ibid.*: 152–156; Brooke 1996: 34–35). Durbin was a strong critic of the roles that competition, private banks, property ownership and inheritance played in promoting deep class divisions and economic inequality. As he concluded in a 1934 NFRB memorandum: 'Capitalism is to be condemned far more on grounds of the social system to which it leads than on any inherent weakness in the institutions by which an active capitalism attempts to solve... economic problems'.⁵

In a speech to the Ethical Union the following year, Durbin strongly criticised economists who defended institutions that promoted inequality. He argued that John Stuart Mill's separation of 'the direct moral evaluation of equality' from an analysis of 'the beneficial consequences believed by economists to spring from the existence of inequality' had enabled economists to see 'the moral evil of inequality'. Therefore, there was no need for them to eliminate the possibility of a 'prosperous equalitarian state'. Durbin rejected his colleagues' claims both that government controls would be inefficient and that humans were motivated solely by 'private gain'. Indeed, the latter argument, he insisted, was a '*psychological* assumption' and therefore 'outside the realm of the science of economics' (Durbin 1935c: 17, 21, 23; italics added). Within another year, Durbin's emerging interest in human psychology would cause him to change his mind entirely on the proper scope of economics, but he remained a staunch critic of Keynes's commitment to ameliorating capitalism without addressing its fundamental flaws. While many of his colleagues considered *The General Theory* to be transformative

⁵Fabian Society Papers, BLPES Archives: J/25/3, Labour Party Policy Committee, Policy No. 197, 'Memorandum on the Principles of Socialist Planning', by E.F.M. Durbin (January 1934): 3.

for the field, Durbin wrote to Keynes expressing disbelief that the author of *The Economic Consequences of the Peace* continued to support an economic system based on private enterprise that freed ‘certain privileged persons to exercise their sadistic impulses in the control of industrial workers’ (Durbin to Keynes, 29 April 1936, quoted in Durbin 1985: 159). Durbin admired aspects of Keynes’s work but his commitment to rational argument was never swayed by ‘eminence’ or ‘authority’ (Phelps Brown 1951: 92).⁶ By the mid-1930s, Durbin’s insistence on prioritising human welfare within capitalism and his continued criticism of Keynes set him apart from other democratic socialist planners.

Durbin shared many of his ideas with his students at the School, where his lectures demonstrated both the breadth of his interests and the development of his thinking prior to the war. In a series on ‘English Civilisation’, for example, he explored the historical origins and characteristics of ‘English consciousness’ from the religious and ethical traditions of the nineteenth century through to the aftermath of the Great War. Durbin highlighted the distinctive role of the Protestant faith, which incorporated both the ‘quiet rational traditional conformism’ of the Church of England and the ‘rebellious, vigorous...more mystical’ dimensions of Nonconformism that had shaped his childhood. England’s moral and democratic foundations were strong, Durbin told his students, but the country was threatened by the growing inequalities of ‘advanced capitalism’, which could—of course—only be reversed by ‘the direction of economic life from the centre’, or ‘planning’. Durbin observed that planning was compatible with many political ideologies, not only socialism, and insisted that, following Roosevelt’s example, ‘all young Conservatives in this country want to plan’. Within the Labour Party, Durbin found that reactions to planning were shaped by the coexistence of two groups: a ‘traditional element’ dominated by trade unions who favoured ‘moderation’ and the ‘bleeding of capitalism’ without any coherent replacement, and a ‘new element’ that reflected Labour’s openness to middle- and upper-class ‘intellectuals’ such as Cripps, Tawney, Laski, Cole, Attlee and Dalton, who supported a ‘fully planned and socially equalitarian State’.⁷ Both Durbin’s faith in planning and his scepticism about reformed capitalism and organised labour remained recurring themes in his work.

⁶See also Durbin Papers, BLPES Archives: 3/14, E.F.M. Durbin, ‘The Great Lord Keynes’, *Daily Herald*, n.d. [22 April 1946].

⁷Durbin Papers, BLPES Archives: 1/1, lecture notes on ‘English Civilisation’, n.d. [early 1930s].

Although he clearly enjoyed teaching, Durbin appreciated the flexibility his work at LSE allowed for the pursuit of his political ambitions (see Brooke 1996: 34). Building on his student involvement in Labour politics, he stood unsuccessfully as the Labour candidate first for East Grinstead, West Sussex, in the 1931 election and then in Gillingham, Kent, in 1935. Labour fought the 1935 election on a new programme, *For Socialism and Peace*, that both maintained the party's commitment to the public ownership of essential services such as water supply, iron and steel, and land, and also bore the imprint of NFRB and XYZ Club thought through the introduction of limited economic planning, most notably the establishment of a National Investment Board (NIB). This programme did not resonate with the electorate and the victory of Stanley Baldwin's National Government in 1935 spurred Dalton, Durbin, Attlee and Arthur Greenwood to begin work on a new blueprint for socialist government. The result, *Labour's Immediate Programme* (1937), reinforced the party's commitment to economic planning by pledging reforms to the ownership and organisation of finance, land, transport, and coal and power, as well as the creation of an NIB.

Labour's Immediate Programme was the culmination of the party's pre-war adoption of economic planning for socialism. As Attlee told the party conference in 1937, 'A Labour Government coming in will proceed to plan this country ... We have already got into an era of planning' (Labour Party 1937: Appendix X, 181–182). Even earlier sceptics were convinced, including Durbin's LSE colleague and admirer R.H. Tawney, who credited Durbin for convincing him that 'the central organization and control of economic life is essential' (Tawney 1931 [1964]: 127). XYZ Club members continued to develop the economic elements of Labour's programme for the remainder of the decade by fleshing out the party's monetary policy, particularly the nationalisation of the Bank of England, exchange control and the creation of the NIB. They also created a War Finance Group, whose work resulted in the publication of *How to Pay for the War* at the end of 1939 under Durbin's name. Unsurprisingly, the group prioritised equality in the distribution of economic burdens across social classes and the creation of 'a wide and efficient machinery of industrial control' (Howson 1988: 549–552).

4 Socialism and Psychology

In the later 1930s, the scope of Durbin's thought widened as he immersed himself in new psychological and anthropological research and attempted to develop both political and economic strategies to address the growing threat of war. From this point onwards, his work was strongly influenced by Freudians such

as John Bowlby, founder of the Tavistock Children's Clinic, as well as by studies of child development and animal behaviour by researchers, including Susan Isaacs and Solly Zuckerman (see Thomson 2006: 221). Indeed, Durbin was so impressed by their insights into human behaviour that he claimed the work of 'analytical psychologists' was 'the greatest single achievement of science in the twentieth century' and vital to virtually every field of study (Durbin 1940: 37).

Bowlby encouraged Durbin to join a study group on psychoanalysis, and the two men became close friends as well as research collaborators (see Brooke 1996: 37–38). Bowlby helped change Durbin's mind on the separation of economics and psychology, and henceforward, Durbin became an evangelist for greater cooperation across academic disciplines. As he explained in a 1938 article in *Economic Journal*, economists who sought to understand trade cycles must have a solid grounding not only in economic but also social and political history, and the use of terms such as 'expectation' and 'confidence' demanded a knowledge of psychology alongside economic theory. He did not advocate the creation of more sub-specialities, cross-disciplinary fields such as 'war studies', or unwieldy 'cooperative research' schemes. Instead, Durbin pleaded for greater cooperation among specialists from different fields through—to use current terminology—multidisciplinary teams, discussion groups and cross-appointed researchers. He was aware that these initiatives presented challenges, especially outside large universities; however, Durbin believed collaborative work would result in much higher standards of research (see Durbin 1938: 184, 191–195).

Durbin's growing confidence in psychological insights into individual behaviour was evident in a short series of lectures he delivered in 1937 on 'The Causes of War'. Speaking to Workers' Educational Association students at Oxford, Durbin focused on the origins of human cooperation and conflict, a topic to which he devoted increasing attention amid the mounting international tensions of the period. His lectures explored and rejected the prevailing view that wars resulted from capitalism, nationalism, economic gain or class conflict. Instead, he focused on human aggression. While he acknowledged that the ubiquity of fighting had made aggression seem 'natural' for human beings, he noted that examples of 'peaceful cooperation' were in fact far more common than hostility. Accordingly, he argued that 'the problem of policy' was not to overthrow capitalism or suppress nationalism, but to find ways to support human cooperation.⁸

⁸Durbin Papers, BLPES Archives: 1/5, 'Syllabus of a Special Course of Three Lectures on The Causes of War', by E.F.M. Durbin, University Extension Lectures Committee, Oxford, 1937: 3–5.

These lectures explored the origins of conflict but offered few solutions. The following year, however, Durbin was ready to offer political and economic direction at a symposium that brought together historical, psychological and political perspectives on the causes and prevention of war. Durbin and Bowlby's contributions to this 'primitive experiment in intellectual cooperation' (Durbin and Bowlby 1938: vii) were shared first in a lengthy chapter in the conference proceedings and then as a single volume, *Personal Aggressiveness and War* (Durbin and Bowlby 1939). Informed by Durbin's earlier zoological studies as well as Isaacs' and Zukerman's research, Durbin and Bowlby traced the origins of fighting and cooperation to different methods of child-rearing. They called for a focus on 'emotional education', a concept that reflected the warmth and freedom of Durbin's own upbringing as well as the development of Bowlby's belief in love as 'a natural potentiality within children' (Durbin and Bowlby 1938: vii, 44; Mayhew 2006: 20; Nuttall 2003: 241). As Europe descended into war, Durbin continued to argue that the outcome of rational thought was peace, not war, and he urged more attention to the 'irrational causes of warfare' (Durbin to Robbins, 8 December 1939, Robbins Papers, BLPES Archives: 3/1/1). Durbin attracted criticism for his insistence that socialists must incorporate the vagaries of human nature alongside economic models; however, the potential of his interdisciplinary approach to policy making was recognised both with funding from the Rockefeller Foundation and in the work subsequently undertaken by LSE colleagues, including T.H. Marshall and Arnold Toynbee (see Thomson 2006: 222–223; Durbin Papers, BLPES Archives: 4/5).

It is important to note that Durbin's belief in reducing aggression did not make him a pacifist. Pacifism, as he and Bowlby explained, was nothing more than 'the passive acceptance of other people's aggression' and thus a 'profoundly neurotic' response in view of both the heightened international tensions of the period and the fact that it would take several generations for programmes of 'emotional education' to bear fruit (Durbin and Bowlby 1938: 44–45). Nevertheless, Durbin recognised the limits of his own willingness to make the ultimate sacrifice in wartime. Writing to his close friend Hugh Gaitskell in early 1939, Durbin noted that while Gaitskell claimed he would give his life not only for British democracy but also to defeat fascism in Italy and Germany, Durbin believed, 'I would die, or think I would, for two things and only two things—collective security and the preservation of democracy in Britain' (Durbin to Gaitskell, 3 January 1939, Durbin Papers, BLPES Archives: 3/12).

Durbin was extraordinarily active both professionally and politically at this time. By early 1939, he was serving on a total of 28 committees and

other groups. These included, by his own count, 12 committees and sub-committees at LSE (he chaired or vice-chaired four) and attendance at meetings of the Economics Department, the Economics Research Division, the Economic History Department and the Sociology Club Committee. He was also active on five NFRB committees, five Labour Party research groups and committees, the Oxford Summer Course Committee and the Chatham House Publications Committee.⁹ Durbin's income, which he recorded in detail from the early 1930s onwards, illustrates both the breadth of his activities and their significance to his family's standard of living. While the School was his main source of income until 1940, Durbin relied heavily on earnings from outside lecturing, conducting examinations and writing to support his household. From October 1938 to September 1939, for example, he was paid £590 by LSE and earned a further £415 from other activities, plus £10 in 'unearned' income. After deductions for taxes and 'expenses', he was left with £905, of which £855 was spent on 'housekeeping', holidays and 'extras' such as £100 for the arrival of a new baby.¹⁰ Durbin was remembered as a devoted family man (he married Marjorie Green in 1932 and they had three children), but other responsibilities were rarely far from his mind, as was apparent in a note he wrote to Robbins hoping the two men might find a moment to discuss 'plans for the Non Specialist classes during next session' at Durbin's young daughter's birthday party (Durbin to Robbins, 24 June [n.d.], Robbins Papers, BLPES Archives: 3/1/1).

4.1 *The Politics of Democratic Socialism*

This period of intense activity also produced Durbin's most significant book, *The Politics of Democratic Socialism* (Durbin 1940). Although it sold relatively few copies and was largely a product of the pre-war period, Durbin's main arguments were forward-looking and subsequently understood both as 'an archetypical statement of wartime socialism' and as an important influence on the 'revisionist' strain of socialist thought that emerged in the Labour Party after the war (Durbin Papers, BLPES Archives: 2/2/8; Brooke 1992: 296; Nuttall 2003: 243–244).

⁹Durbin Papers, BLPES Archives: 5/2, 'Committees' (March 1939).

¹⁰Durbin Papers, BLPES Archives: 2/1/8, 'Income and Expenditure Account Oct 1938–Sept 1939' [n.d.].

This book reflected the diversity of Durbin's interests, especially the role of psychology in politics. The first section outlined the preconditions for the development of the society he envisioned, particularly the need to preserve and strengthen democracy by reducing human aggression and fear. Durbin insisted that 'democracy is much more a result of character in a people than of law or learning. Its roots are emotional rather than intellectual. It is fundamentally a consequence of psychological health and the absence of neurosis' (Durbin 1940: 263). Accordingly, Durbin focused less on a 'cure' for the aggressor—such as build-ups of military force against Mussolini and Hitler—and more on the prevention of aggression in the first place. To that end, he deplored corporal punishment as it encouraged children to accept and normalise violence. Parents should 'spare the rod and make a free, independent, friendly, and generous human being' (ibid.: 65–66). Durbin's focus on human psychology attracted critics, including Herbert Morrison who found the first section of the book 'hard going' (Morrison quoted in Nuttall 2006: 53) and suggested it should be removed; however, Tawney concluded that his earlier 'Philistine scepticism' had been misplaced, while a young Tony Crosland was highly impressed after hearing Durbin speak about his book at the Oxford Union in 1940 (Tawney quoted in Nuttall 2006: 53; Crosland to Williams, 29 October 1940, Crosland Papers, BLPES Archives: 3/26/i).

Many of the points Durbin had made in his earlier writings were fleshed out in the more political and economic chapters of the book. At its heart was a compelling exposition of the internal inconsistencies of Marxism and a forceful argument for capitalism as the foundation for a more just and efficient democratic socialist society. This new 'middle way' would enable socialists to achieve their goals by combining ethical imperatives with Fabian-inspired planning and efficiency: 'The problem of policy can thus be defined as the search for a method whereby the virtues of capitalism—rationalism and mobility—can be combined with democratic needs—security and equality—by the extension of the State upon an ever-widening and consistent basis' (Durbin 1940: 148).

Durbin rejected Marxists' historical dialectic that excluded all but economic factors in the growing conflict between two distinct classes. As his psychological research had demonstrated, humans were affected by a multitude of forces, including nationality, government, social relationships, faith and family: 'We are more complicated than the Marxists have us believe'. Drawing on Tawney's *Acquisitive Society* (Tawney 1921), Durbin used the historical development of social class in Britain to demonstrate that the working classes had made great gains within the democratic system, advancing with the assistance of the expanded franchise, trade unions and universal

education from ‘a horde of dispossessed and ignorant peasantry’ to become a ‘lively and intelligent proletariat’. The main concern of the British worker, according to Durbin, was security, not equality, and therefore, civil war was neither inevitable nor more appealing than gradual, institutionally driven change (*ibid.*: 182–183, 199–200).

Durbin further echoed Tawney and discredited Marxism through his observation that higher levels of disposable income and the advent of limited liability had led to an increase in the holding of shares. The resulting split between owners and directors greatly increased the power of those who managed companies and created a new class of professional managers who held real power while owners became increasingly passive and parasitic (see *ibid.*: 120–128). Managerialism did not play a large part in Durbin’s analysis of the evolution of capitalism, but it strongly influenced socialist thought after the Second World War, particularly for Crosland (see Crosland 1956: Part 1).

In the face of war, Durbin concluded by urging his readers to look ahead to a society based on ‘the common happiness of mankind’. Given Britain’s past achievements—not least the work of the psychoanalysts—and the capacity of its people, he believed his vision was achievable within a single generation: ‘We have only to open our eyes and stretch out our hands to pluck this precious fruit from the tree of knowledge’ (Durbin 1940: 334). Durbin’s deep affinity for Britain comforted him during the early months of the war. The next five years tested his optimism.

5 War

Durbin had been keen to work in government since the prospect of war briefly threatened to close LSE during the Munich Crisis (Durbin to Beveridge, 28 September [1938?], Beveridge Papers, BLPES Archives: 5/21). In early 1940, he took a post in the Economic Section of the War Cabinet Secretariat, where he served in a variety of roles until October 1942 when he was appointed Assistant Secretary to the Labour leader and Deputy Prime Minister, Clement Attlee. He also continued to teach part-time after the School was evacuated to Cambridge, lecturing there on Friday evenings and Saturday mornings before heading to Oxford both to see his family and to continue with Fabian and Labour meetings and conferences for the remainder of the weekend. As his widow Marjorie later observed of Durbin and his colleagues, ‘They were never at home these men, never’.¹¹

¹¹Durbin Papers, BLPES Archives: ‘Marjorie Durban [sic]’, COLL MISC 0978: 93–94.

Durbin's wartime writing demonstrates the continued breadth of his interests and the distinctiveness of his socialism (see Nuttall 2003: 244–245). In 1942, he published *What Have We To Defend?* (Durbin 1942), a short, passionate book that made little reference to economic planning or controls but focused on the radicalising effects of the war and Durbin's conviction that the conflict presented a unique opportunity to rebuild society along the lines he had set out in his earlier work. The book sold well, assisted by Dalton's keen support: 'Your book is *bloody good!* So much so, in my view, that I have got 12 more copies and sent them out' (Dalton to Durbin, 6 September 1942, Durbin Papers, BLPES Archives: 7/7; italics in original).

Despite the wartime context, Durbin focused on threats coming from within Britain itself. He identified 'Four National Faults': economic and social inequality, 'vandalism' and 'lack of imagination'. The first two were familiar, while the third and fourth had been present but less prominent in his earlier writing. Durbin flagged 'vandalism' to draw attention not only to what the British stood to lose in the war but also how much senseless destruction of countryside and cultural monuments had already taken place well before 1939, particularly under the auspices of Conservative governments: 'Hitler's bombers have not yet wrought one-tenth of the aesthetic damage that we carefully accomplished ourselves, with full legal sanction' (Durbin 1942: 26). By 'lack of imagination', Durbin emphasised that he did not mean stupidity (although he had growing doubts about the intellectual capacity of his fellow citizens, which emerged more fully after the war). Rather, he was troubled by Britons' reluctance to 'look upwards', to think beyond their immediate troubles and believe in the possibility of a new world. Although there were advantages to the British tendency to plant their feet firmly on the ground—'France might not have fallen if her people had not possessed so sensitive an imagination'—this tendency also blocked the possibility of progress:

The man in the street must see a society that is strong and safe in the comfort of a wide association of states, a community in which no man is poor or unemployed, in which there is no servility or the pomp of wealth and of which children are the free and happy citizens. This society does not exist yet, but only because we do not see it—our eyes fixed upon the useful trifles of the world we know (ibid.: 33–34).

Durbin concluded *What Have We To Defend?* with a summary of the socialist programme he proposed to implement after victory. He underlined its moderate practicality but reminded citizens first of their duties in a country at war: 'None of us possesses any unqualified rights, not even to life itself. To every right there corresponds a duty and it may be our duty to die' (ibid.: 79, 84, 87).

Durbin's wartime writing supports Beech and Hickson's identification of him as a 'patriotic socialist' (see Beech and Hickson 2007: 88, 90); however, their claim that his patriotism was not based on racial or moral superiority is debatable. When Durbin asked, 'What, then, do we have to defend?', his answer originated in the superiority of the British, whom he described as 'the most tolerant people in the world' and 'the vanguard of the human mind'. Durbin contrasted the 'darkness in the German soul' that fostered 'a love of authoritarian discipline' with his own people's 'slowly growing faith in human liberty, equality and brotherhood'. While he supported nationalist movements in India and Africa and drew parallels between claims of white racial superiority and the Nazis' 'absurd racial doctrines', Durbin also looked forward to the 'slowly widening stream of liberty' through self-government which would ensure 'the permanence of a Greater Britain beyond the seas' (Durbin 1942: 37, 51, 54, 74, 66–67, 69–70; Brooke 1992: 274). Clearly, Durbin's prose reflects the heightened emotions of the period, but his wartime emphasis on the value of the 'British social tradition' aligns with his much longer-standing belief in the exceptionalism of British character, values and institutions. His vision of a new world was grounded not only in democracy and equality, but also in the superiority of British institutions and values.

As a civil servant, Durbin welcomed the opportunity not only to apply his expertise to the implementation of controls over the wartime economy but also to urge their continuation after the war. Eschewing any notion of political neutrality, he joined the 'tribe of experts' on Labour's Reconstruction Committee, along with XYZ Club colleagues such as Jay, William Piercy and Vaughan Berry. His contributions focused primarily on finance and international economic policy and built on Durbin's continued conviction that long-term peace necessitated both greater global prosperity and international economic cooperation.¹² Accordingly, he argued for ongoing exchange controls and a new international bank to facilitate international lending, the latter modelled on plans published by Keynes and American economic advisor Harry Dexter White in 1943. Durbin was willing to use Keynesian methods to control inflation, but otherwise he maintained his earlier scepticism, particularly about Keynes's support for a permanent low interest rate policy (see Howson 1988: 553–555; Brooke 1989: 165–166). Dalton described the Reconstruction Committee's final

¹²National Peace Council, BLPEs Archives: 13/3, E.F.M. Durbin, 'A Four Point Programme', in *The Economic Basis of Peace*, Peace Aims Pamphlet No. 16 (London: National Peace Council, n.d. [1942]): 22, 28.

report, *Full Employment and Financial Policy*, as ‘largely Keynesian’ with ‘some socialist additions’ (Dalton quoted in Howson 1988: 556). Durbin accepted that compromise but continued to insist that centralised controls were ‘the instrument naturally favoured by Democratic Socialists...to preserve a state of full employment without inflation and, therefore, without the necessity for deflationary measures’.¹³

In his less partisan capacity as Assistant Secretary to Attlee, Durbin turned his mind to directions for post-war foreign policy, including the possibility of returning to imperial isolationism, resuming a ‘great powers’ alliance along pre-1914 lines or creating a new ‘collectivity of peace-loving nations’ similar to the League of Nations. After outlining the few strengths and many weaknesses of each choice, Durbin concluded glumly that none of them avoided the necessity for Britain to commit considerable resources to armaments after the war: ‘If we are to have peace for the remainder of the twentieth century, we must pay for it—in tanks, in military aircraft and in conscription’. Above all, Durbin argued that foreign policy could only be effective if all political parties agreed on a common strategy.¹⁴ This approach was consistent with his thinking about human nature and the causes of war in the late 1930s, and it is no surprise that he used his first speech in the House of Commons (shortly after the American bombing of Hiroshima and Nagasaki) to reiterate both his opposition to pacifism and his hopes for international cooperation. It was the ‘grim paradox’ of the time, Durbin told his fellow MPs that ‘we cannot have peace unless we are prepared to fight for it’.¹⁵

Durbin’s experience as a civil servant and his commitment to a considerably enlarged role for the post-war State were reflected in several articles he wrote on government administration for *Political Quarterly*. In the first, published in 1943, he argued that economists should be more active in government on issues such as the management of employment rates and expansion of social services. More importantly, they must be the ‘paid “remembrancers” of the public conscience’, whose duty was ‘to denounce the specious pleas of monopolist and trade unionist and to summon the lazy citizen to repentance’ (Durbin 1943: 265–267). Durbin’s second article made a

¹³Durbin Papers, BLPES Archives: 3/3, ‘Economics of Democratic Socialism’, n.d. [1945–1948]. In 1940, Durbin had also identified large-scale nationalisation as an effective, if problematic, strategy to pay for the war while limiting inflation. See Fabian Society Papers, BLPES Archives: K/18/1, War Economics Committee Memorandum No.1, E.F.M. Durbin, ‘The Financing of War’, 12 March 1940.

¹⁴Dalton Papers, BLPES Archives: 2/7/10, Evan Durbin, ‘British Foreign Policy After the War’, 1 April 1943.

¹⁵Durbin Papers, BLPES Archives: 3/14, E.F.M. Durbin, MP, ‘Charter of the World Organisation’, House of Commons, 22 August 1945.

case for the inspirational leadership of economists among other civil servants who, although ‘clever and pleasant’, lacked the energy of ‘young scientists, or young socialists, or young doctors’. Durbin blamed a civil service selection process that favoured men who sought a ‘safe’ job, compounded by a lack of specialised training for new recruits. Acting on his faith in psychological profiling and commitment to adult learning, Durbin recommended a more ‘scientific’ civil service selection process, including ‘intelligence tests, practical tests and psychiatric examinations’, culminating in interviews that favoured ‘vitality’ over ‘charm’ (Durbin 1949a: 109–110). Such reforms, combined with greater efficiency and coordination in the day-to-day operations of government departments, would equip the civil service to meet the challenges of post-war reconstruction.

6 Reconstruction

Durbin predicted a Conservative victory in the 1945 election but, to his surprise, Labour swept to power and ‘The Man with a Plan’ entered Parliament at last with a decisive win in the North London constituency of Edmonton.¹⁶ Durbin hoped Attlee would reward him for his wartime work—and shared fondness for detective stories—with a ministerial post. He was ‘bitterly disappointed’, then, when the new Prime Minister instead appointed him Parliamentary Private Secretary to Dalton, the new Chancellor of the Exchequer.¹⁷ Durbin’s disappointment notwithstanding, the Treasury was central to the new government’s reconstruction programme and with Gaitskell also elected to a parliamentary seat in 1945 and Jay the following year, the New Fabian planners were firmly established in Whitehall (see Brooke 1992: 328–329).

The next three years tested Labour’s socialist credentials and highlighted many of the tensions Durbin had foreseen between social democracy, Britain’s economic weakness and the vagaries of human nature. Moreover, the 1945 victory was a very qualified one for ethical socialists such as Durbin, and in

¹⁶Durbin to Bassett, 23 June 1945, Durbin Papers, BLPES Archives: 3/1. Durbin’s campaign letterhead proclaimed him ‘The Man with a Plan’.

¹⁷To illustrate his ‘jolly’ life in wartime London, Durbin told his wife he sometimes had to wait late into the night at 11 Downing Street to get his detective stories back from Attlee, who read in his study with the blackout curtains open. See Durbin Papers, BLPES Archives: ‘Marjorie Durban [sic]’, COLL MISC 0978: 92, 96. John Bew’s recent biography of Attlee, *Citizen Clem* (Bew 2016), makes no reference to Durbin, but Durbin seems to have been quite an admirer of Attlee both during and after the war. See Durbin Papers, BLPES Archives: 3/9, Evan Durbin, ‘C.R.A.’, n.d. [1945–1946].

practice, the government had closer affinity with Labour's Fabian roots than with Tawney's 'golden moment' of socialist transformation. Before the end of the decade, tensions had increased significantly between those whom Durbin called 'consolidators', who wanted to improve on existing controls, and 'anti-consolidators', who demanded more socialisation.¹⁸ Such divisions can be understood as one of the consequences of Labour's experience in power; however, they also suggest that the success of ethical socialism is dependent upon the material circumstances that surround its presentation. Labour's ethical foundations played an essential role in its election victory but once in government and facing challenges from the party's trade union base and a resurgent Conservative Party, the limits of ethical appeals became painfully clear.

Durbin's contributions to a discussion of 'Future Policy and Problems' in the summer of 1945 demonstrated that Labour needed to be on the defensive from the outset:

Mr. Durbin said he was somewhat gloomy about the whole position with which we were faced. The major problems—food, homes, fuel—were extraordinarily difficult to hurdle at any rate in the first two years. It was, therefore, necessary that there should be first-class publicity to make it clear that those difficulties were inevitable and inherited by the Labour Government.¹⁹

Labour moved quickly to demobilise servicemen and women into peacetime jobs, build new homes and lay the foundations for cradle-to-grave security through National Insurance and the National Health Service. Durbin was initially pleased with the government's progress but he still saw a substantial gulf between these reformist measures and his ideal socialist society. Looking back on the first year of Attlee's government, Durbin lamented 'the inevitable tendency for Conservatives to move Left—and Labour to the Right'. Still, he believed Labour remained true to its democratic socialist roots, and he hoped the party could secure its uneasy coalition of trade unionists and 'educated men' for at least a decade in power.²⁰

In 1947, Durbin's cautious optimism was shattered by a severe manpower shortage in staple industries, which led to a crisis in coal production that

¹⁸Durbin Papers, 4/7, BLPES Archives: 'Labour in Power', n.d. [1946?].

¹⁹Dalton Papers, BLPES Archives: 9/1, 'Notes of an Informal Discussion on Future Policy and Problems', 30 July 1945. Participants included Durbin, Crosland, Gaitskell, Richard Crossman and Harold Wilson.

²⁰Durbin referred to the Cabinet as 'half old Etonians—and half errand boys' (Durbin Papers, BLPES Archives: 4/7, 'The Fundamental Paradoxes' and 'The Present Party Position', n.d. [1946?]).

strained Britain's already delicate balance of payments and undermined public confidence in the government. The crux of the problem was the need to attract workers to undermanned areas without infringing on the freedom of the labour market—a classic example of the need for compromise between competing interests in a planned economy. Writing in the *Evening Standard* in September 1945, Durbin had called for wartime manpower controls to be dropped as soon as possible and reminded trade unionists that the goal of planning was 'to increase liberty not destroy it'.²¹ Two years later, Durbin's priorities were unchanged. He strongly resisted government proposals to direct labour into essential industries, arguing instead for a differential wage structure to strengthen socialist planning and avoid driving British people to work 'by threats'.²² His support for a wage policy put Durbin at odds with many of his colleagues, but as his long-time friend Phelps Brown observed, Durbin never hesitated to 'Dare to be a Daniel' (Phelps Brown 1951: 92).

Ultimately, the government was unable to arrive at an agreement with the unions and Labour's 1947 conference rejected formal wage policies. This episode demonstrated the fragility of economic controls and their dependence on the subordination of the interests of organised labour to the needs of the nation. It also effectively marked the end of Labour's distinctively socialist economic policies and demonstrated the limits of pre-war socialist thinking on monetary policy (see Brooke 1992: 334; Howson 1988: 564). The government's subsequent White Paper on Personal Incomes (1948) 'walked a fine line between the disinflationary and socialist schools of thought', but it nonetheless met with Durbin's approval for including a wage stop and maintaining wage differentials in essential industries.²³

Durbin's interest in financial rewards and other methods of persuasion drew not only on his strong commitment to individual freedom in a planned economy, but also his continued interest in human psychology and emotional development. Contemporary research into IQ levels suggested to Durbin that 'quite simple work can be satisfying to large percentages of the population'. Therefore, he supported the use of material incentives while increasingly doubting the effectiveness of appeals to the greater social and

²¹Durbin Papers, BLPES Archives: 3/15, E.F.M. Durbin, 'The Right to Choose Your Job', *Evening Standard*, 24 September 1945.

²²Durbin Papers, BLPES Archives: 4/7, 'Britain's Economic Crisis', January 1948; Durbin (1948: 9–10, 23); Jackson (2007: 131–132).

²³Brooke (1991: 699); Durbin Papers, BLPES Archives: 4/7, 'White Paper on Personal Incomes', 1948.

moral good.²⁴ To clarify the situation, he urged more investigation of the underlying reasons for workers' reluctance to enter certain industries and proposed to use advertising campaigns to counter the low social status of some occupations (see Durbin 1948: 24–25). Above all, as Durbin had insisted during the war, 'there is no ground for economists to prefer restriction to adaptation' (Durbin 1943: 268).

Durbin recognised that the government's vulnerabilities extended beyond economic fragility and declining electoral support, and took him back to the question of 'emotional education' that he and Bowlby had explored in the late 1930s. Durbin and his circle believed strongly in 'the power of reason to legislate for practice', as Phelps Brown put it, and during the war, Durbin was optimistic that growing State control reflected 'the substitution of reason for instinct in the ordering of human affairs'.²⁵ However, his experiences in peacetime government increased his pessimism about human nature and intellectual capacity. Publicly, he remained positive about the government's record in the face of 'remorseless criticism and misrepresentation',²⁶ but privately he identified more fundamental barriers to the creation of the New Jerusalem:

British people tired of austerity
British people not socialists
Government not solved problem of public relations.²⁷

Increasingly, Durbin doubted that formal education could overcome the deficiencies in average intelligence that stood in the way of educating for socialism. As a result, he looked for alternative solutions (see Thomson 2006: 232; Nuttall 2006: 57–58). In September 1945, for example, Durbin presented his views on hereditary intelligence at the Fabian Society's conference on 'The Psychological and Sociological Problems of Modern Socialism'. He claimed that no more than half the population could truly benefit from school or university education and he equated communal 'wickedness' with widespread mental illness. Since nationwide psychoanalysis was impractical, he believed 'selective breeding was probably the answer'. Other participants were sceptical about the results of mass psychoanalysis and more optimistic

²⁴Durbin Papers, BLPES Archives: 4/7, 'Incentive in Industry', n.d. [1945–1948].

²⁵Phelps Brown (1951: 92); National Peace Council Papers, BLPES Archives: 13/3, E.F.M. Durbin, 'A Four Point Programme', in *The Economic Basis of Peace*, Peace Aims Pamphlet No. 16 (London: National Peace Council, n.d. [1942]): 23. See also Nuttall (2006: 54–61).

²⁶Durbin Papers, BLPES Archives: 4/7, 'A New Year Message from Evan Durbin', n.d. [1946–1947].

²⁷Durbin Papers, BLPES Archives: 4/7, 'The Next Five Years', n.d. [1947–1948]

about the potential for social institutions to improve ‘national character’, but Durbin insisted that his goal was ‘a psycho-analysed pedigree herd’.²⁸

At another Fabian conference the following year, Durbin encouraged Labour to use modern insights into voter psychology to increase the party’s chances of re-election. Cleaving to ‘mixture as before’—more socialisation and social services—would neither galvanise electoral support nor make socialists. Durbin recommended instead more ‘systematic and scientific’ studies to illuminate ‘what people really want from the state’ and strengthen democracy through improved communications such as government-issued pamphlets and films.²⁹ Durbin’s electoral canvassing also uncovered other barriers to Labour’s electoral success. On the doorstep, he met the ‘wretched housewife’ who laboured to meet ever higher standards of domestic cleanliness, nutrition and child-rearing. His constituency work led Durbin to conclude that at least 40% of individuals were either ‘caught in a pattern of rights, regulations, historical events and public policies they cannot possibly understand or master’ or suffer from inadequately treated ‘neurotic impulses’. He offered no immediate relief to overworked women but proposed that ignorance and neuroses could be reduced through the creation of a new ‘Household Visiting and Advisory Service’ and a ‘revolutionary increase’ in psychiatric care.³⁰

Many of Durbin’s observations and recommendations smack of the ineffective ‘gentlemanly expertise’ that Mike Savage has highlighted among sociologists and policy makers of this period and reduce the distinctiveness of the ‘breadth’ and ‘synthesis’ Nuttall has noted in Durbin’s thought (see Savage 2010: 107–109; Nuttall 2003: 236–237). More importantly, however, Durbin drew attention to weaknesses in Labour’s popular appeal that would haunt the party over the following two decades. In response, he urged Labour to lighten its touch—to focus less on nationalisation and economic reforms and demonstrate socialists’ commitment to human happiness through measures such as paid holidays, shorter working hours and even ‘a

²⁸Fabian Society Papers, BLPES Archives: G/49/10, Report on Weekend Conference on the Psychological and Sociological Problems of Modern Socialism, Session III (15–16 September 1945): 14–18; Nuttall (2006: 56–58).

²⁹Durbin Papers, BLPES Archives: 3/15, E.F.M. Durbin, ‘Beyond Socialism’, Fabian Society, Conference on Labour’s Second Five Years, 4 November 1946.

³⁰At the same time, he was pressing Herbert Morrison, the Deputy Prime Minister, to increase government funding for psychological research. See Durbin Papers, BLPES Archives: 3/15, E.F.M. Durbin, ‘Beyond Socialism’, Fabian Society, Conference on Labour’s Second Five Years, 4 November 1946; Durbin to Morrison, 15 April 1946 and 18 October 1946, Durbin Papers, BLPES Archives: 3/14.

slice of frivolity'.³¹ Durbin's sustained interest in psychology demonstrated that he was both alert to growing Conservative criticisms of austerity and firm in his commitment to a multidimensional democratic socialism that recognised emotional and physical comforts alongside economic and moral reforms. Very few of his colleagues were as clear-sighted.

7 Conclusion

In the autumn of 1947, Attlee appointed Durbin Parliamentary Secretary at the Ministry of Works to replace Harold Wilson, who had been made President of the Board of Trade. Privately, Durbin was angered by Wilson's elevation but he consoled himself by looking to the future. After a calming cup of tea, he told his wife, 'Hugh'll be Prime Minister. I'll be his Chancellor of the Exchequer and to hell with Harold Wilson'.³²

It was not to be. Durbin drowned off the Cornish coast on 3 September 1948 after pulling one of his daughters and another child out of dangerous surf. His friends and colleagues recalled his 'complete intellectual integrity', his 'modest and unselfish nature', a life inspired by 'noble idealism' and a promising political career cut short.³³ At his memorial service, Tawney spoke of Durbin's deep commitment to democracy and 'the substitution of reason and public spirit for the scramble for wealth and power as the determining factor in economic life'.³⁴

Notwithstanding his dim view of average intelligence, Durbin was also remembered for his inspirational teaching in both university and extra-mural classrooms.³⁵ Some BBC radio listeners may have found Durbin 'self-satisfied and patronising', even 'lazy and bored',³⁶ but to his many friends, he was anything but. In addition to his very broad intellectual interests and passion for detective stories, Durbin enjoyed walking holidays,

³¹Durbin Papers, BLPES Archives: 3/15, E.F.M. Durbin, 'Beyond Socialism', Fabian Society, Conference on Labour's Second Five Years, 4 November 1946.

³²Durbin Papers, BLPES Archives: 'Marjorie Durban [sic]', COLL MISC 0978: 101.

³³'Mr. E.F.M. Durbin', *The Times*, 8 September 1948: 6; Attlee, 'Foreword: An Appreciation of E.F.M. Durbin', in Durbin (1949a: vii).

³⁴Tawney Papers, BLPES Archives: II/90, 'The Address by Professor R.H. Tawney at the Memorial Service', 16 September 1948.

³⁵'Mr. E.F.M. Durbin', *The Times*, 8 September 1948: 6; Tawney Papers, BLPES Archives: II/90, 'The Address'.

³⁶Durbin Papers, BLPES Archives: 5/2, BBC Listener Research Report, 'Money', 9 September 1943.

racquet sports and the cinema. In the early 1930s, he wooed his future wife, Marjorie, with a ‘wonderful lunch’ he had specially prepared by the chef at New College and he later told his friend Gaitskell, ‘The three greatest pleasures in my life are food, sleep and sex’. Gaitskell likely spoke for Durbin’s many friends when he wrote to Robbins at the School in 1950, ‘It would be so much less lonely if he were here still’.³⁷

With hindsight, it is easy to be critical of some aspects of Durbin’s work. He was a product of the English public school system and Oxbridge education of his time and he remained within that world. For all the breadth of his interests, he had significant blind spots. For example, despite Durbin’s ethical vision and his interest in ‘emotional education’, he was distant from contemporary dialogue on the left about gender and sexuality (see Brooke 2011: Chapter 3). As Ben Mayhew has also pointed out, both Durbin and Bowlby were unduly optimistic about the ability of the institutions of the State to reduce aggressive tendencies among humans and they seemed oblivious to the extent to which their own value systems had been shaped by their interwar upbringing (see Mayhew 2006: 30). Although his work addressed both equality and economic planning, Durbin’s focus lay on the latter and his almost casual resort to eugenic solutions to address natural inequalities sits uneasily with the high moral bar he applied in many other areas. His confidence in reason and rational argument, although less strong towards the end of his life, is nonetheless at odds with both his low opinion of average intellectual capacity and the findings of the psychological research that so fascinated him. Also, his thinking was highly insular. His unquestioning confidence in British (chiefly English) institutions and lack of interest in international perspectives, while not entirely unusual on the left, were nonetheless conservative and notable among his colleagues (see Ellis 2012; Meade 1950: 122).

However, these observations should not detract from the originality of Durbin’s thinking and the impact of his contributions to Labour’s ideological and policy development. His distinctiveness makes him ‘difficult to pigeonhole in terms of ideological groupings in the Labour Party’ (Beech and Hickson 2007: 80), but he has attracted many labels, including ‘psychological socialist’, ‘patriotic socialist’ and ‘militant moderate’ (Nuttall 2003; Beech and Hickson 2007: 87). The most persistent area of disagreement is Durbin’s connections to the ‘revisionism’ that emerged

³⁷Gaitskell (1940 [1954]): 13; Durbin Papers, BLPES Archives: ‘Marjorie Durban [sic]’, COLL MISC 0978: 88; Gaitskell to Robbins, 28 October 1950, Robbins Papers, BLPES Archives: 3/3/9.

in the Labour Party in the late 1940s. While Kevin Jefferys warns against seeing Durbin ‘simply as a forerunner of 1950s-style revisionism’ (Jefferys 2004: 71), Beech and Hickson consider him ‘overtly a rightwing revisionist’ (Beech and Hickson 2007: 83). Durbin’s daughter, Elizabeth, referred to *The Politics of Democratic Socialism* as ‘an influential statement of the revisionist case’ (Durbin 2008), a view likely more influenced by the criticism the book attracted from the Labour left than by its central arguments. As Stephen Brooke reminds us, however, these disagreements highlight the ‘historical complexities and disjunctures’ of revisionism and the ‘radiant ambiguity’ that Tawney celebrated in the term ‘socialism’ (Brooke 1996: 51–52). Moreover, Durbin worked during a period of significant transition in the balance between political theory and technocratic action. His ethical vision bridged both, persisting through two decades in which political theorising, in general, and idealism and altruism specifically were otherwise in decline (see Harris 1996: 21–24).

The difficulty of classifying Durbin reinforces the enduring impact of his ideas. He recognised the range of responses to his ideas and thrived on them. Durbin told his friend Reginald Bassett in 1945 that his detractors considered him a ‘dangerous milk and water, pseudo-Conservative’ (Durbin to Bassett, 10 June 1945, quoted in Brooke 1991: 690). In his final book, he thanked all of his LSE colleagues, particularly Robbins and Hayek, ‘who, by criticising and disagreeing with almost everything I have ever said about this subject [economics], have kept me thinking about it’ (Durbin 1949a: x).³⁸ Whether Durbin’s blend of ‘revisionist’ and ‘fundamentalist’ ideas would have outlasted the 1940s or forestalled the divisions that dogged the party in opposition after 1951 can only be speculated, but notes for the unfinished companion volume to *The Politics of Democratic Socialism*, provisionally entitled ‘The Economics of Democratic Socialism’, do not suggest much deviation from Durbin’s commitment to a blend of planning and nationalisation (see Durbin Papers, BLPES Archives: 6/1). At the same time, Durbin’s resistance to controls, his concern for human happiness and belief that ‘there is a good deal of entertainment to be got out of living’ suggest that his views on affluence might have brought him closer to Crosland than the ‘fundamentalists’ of the Labour left.³⁹ In any case, Durbin’s rejection of Marxism and pacifism and his championing of distinctly British ethical and democratic

³⁸Durbin engaged in considerable debate with Hayek over his *The Road of Serfdom* (Hayek 1944). See Durbin (1945).

³⁹Durbin Papers, BLPES Archives: 3/15, E.F.M. Durbin, ‘Beyond Socialism’, Fabian Society, Conference on Labour’s Second Five Years, 4 November 1946; Ellis (2004b: 69–84).

socialist traditions had continued appeal among centre-left politicians, most notably Social Democratic Party leaders David Owen, Shirley Williams and Bill Rodgers (see Jones 1998: 5). Above all, as Tawney observed, despite Durbin's tremendous intelligence, 'the secret of his power was less intellectual than moral' (Tawney Papers, BLPES Archives: II/90, 'The Address').

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20

R.G.D. Allen (1906–1983)

Jim Thomas

1 Introduction¹

Roy Allen was born on 3 June 1906, in Stoke-on-Trent, but the family moved and he grew up in Worcester. He was the elder son of George Henry Allen and his mother was Jessie Callcott Hill. His father was a steelworks manager, but was also an All-England angling champion and later opened a fishing tackle shop above which the family lived. Allen attended a local primary school in Worcester before he won a scholarship to the Royal Grammar School in Worcester. In 1924, he went to Sidney Sussex College, Cambridge, as a mathematics scholar. He obtained a First in Part One of his degree in 1925 and became a Wrangler in 1927. He was awarded a research scholarship and remained in Cambridge for an extra year, which was devoted mainly to reading economics and philosophy.

Allen came to LSE in August 1928 on a one-year contract as an Assistant in Statistics. He became an Assistant Lecturer in Statistics in 1934, was promoted to Lecturer in 1938, and in 1939, he became Reader in Economic Statistics

¹For personal information on Roy Allen, I have consulted Cairncross (1984), LSE (1984), Grebenik (1984), Pratten (2004), Stone (1998) and Thomas (2004). I have concentrated here on Allen's contributions to economics; for an evaluation of his statistical work, the reader should consult these sources.

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(with special reference to mathematical economics). He was appointed Professor of Statistics at the University of London in October 1944. With the exception of the time when he visited the USA (a Fulbright Scholarship in 1937, a Visiting Professorship at Harvard University in 1950 and a sabbatical year at the University of California in Berkeley in 1957) and his absence on war service from 1939 to 1945, Allen's entire career was spent at LSE.²

Allen was involved in war service from September 1939 to November 1945. From 1939 to 1941, he served as a statistician in the Treasury, where he worked organising data on exchange requirements and the balance of payments. Then, he became Director of Records and Statistics for the British Supply Council in Washington, D.C. from 1941 to 1942. He then became British Director of Research and Statistics for the combined Production and Resources Board in Washington from 1942 to 1945, a post that involved coordinating British and American statistics of war production.

His commitment to public service continued after he completed his war service. In 1946, he spent some time at the Economic and Social Council of the United Nations and he returned to the Treasury in 1947 during the crisis that followed the suspension of convertibility to provide improved statistics on the outflow of dollars. In 1949–1950 and again in 1952, he was at the United Nations as a consultant to the Statistical Office.

In the area of academia and professional institutions, Allen was President of the Econometric Society in 1951 and in 1955 he was invited by Sir Sidney Caine to advise on the development of law teaching at the University of Malaya. He was elected a Fellow of the British Academy in 1952 and served as the Treasurer from 1954 to 1973. He served as Treasurer of the International Institute of Statistics and was an active member of the Council of the Royal Statistical Society, serving as its President in 1969–1970. He was awarded the Society's Guy Medal in Gold in 1979. He was a member of the Institute of Statisticians and an honorary member of the Institute of Actuaries from 1965. For many years, he was a consultant to the British Medical Association.

Allen contributed to the *Report of the Committee on the Taxicab Service* (HMSO 1953) and was a member of the Research Council of the Department of Social and Industrial Research (DSIR) in 1964–1965,

²Cairncross (1984: 379) reports that Allen rejected 'all offers from elsewhere however tempting, including a Chair at Oxford ("far too parochial to suit me") and the Mastership of a Cambridge college. When presented to the Queen at a British Academy reception, he was asked by Her Majesty why he had stayed so long in one place and replied with his usual quiet modesty: "If one happens to be in the best place, why should one move?"

of the University Grants Commission in 1966 and on the Council of the Social Science Research Council in 1967–1970. He was also involved in the regulation of civil air transport for many years, first as a member of the Air Transport Licensing Board from 1960 to 1972 and then on the Civil Aviation Authority from 1972 to 1973.

Allen was Chairman of the Committee on the Impact of Rates in 1963–1965 and a member of the Commission on Civil Liability in 1974–1978. He also sat on the Committee of Inquiry on Decimal Currency (HMSO 1963). The recommendations of this Committee had a somewhat controversial outcome. While it might seem relatively simple to devise a decimal currency system, the process was subject to considerable political problems. The simplest solution would have been to keep the pound sterling intact and introduce a new subdivision into 100 cents. However, each cent would be worth 2.40 pence, and it was argued that 2.40 pence was too much for the basic monetary unit. The obvious alternative solution that was suggested was to introduce a new pound sterling equal in value to ten shillings. Now, one cent would be equal to 1.20 old pence, but there were strong objections to this proposal by those who felt that the old pound must be preserved at all cost, as setting the new pound equal to ten shillings might be seen as a massive devaluation. The solution that emerged was a typical English compromise, known as the (£-cent-½) option: the old pound was preserved and divided into 100 cents, with one cent equal to 2.40 old pence, but a ½ cent coin was introduced to deal with the argument that 2.40 pence was too large a basic unit.³

Allen was one of four members of the Committee that signed the Majority Report, with two members signing a Memorandum of Dissent. There was some surprise among Allen's academic colleagues that he, as the only mathematician on the Committee, had agreed to a decimal system with a halfpenny tacked on. He was rather sensitive on this issue but seemed to have some private reservations about the outcome.⁴

³The ½p coin remained in circulation until 1984, by which time inflation had sufficiently reduced its value for it to be taken out of circulation.

⁴Professor William Baxter, a colleague and friend of Roy Allen, delivered one of the tributes at the Memorial Meeting. In his address, he said: 'Roy liked the Senior Common Room, and often joined in its discussions. I remember something he said that may still have interest. In the 'sixties, he was on the committee that planned the switch to decimal currency. One day he came in with—unusual for him—a look of some disappointment. He said that the government had misled the committee into adopting as the base unit of the new scheme the pound. The committee would have preferred the half-pound (and, as experience in South Africa and elsewhere has shown, 10 shillings was indeed a simpler unit)' (LSE 1984: 1).

Allen's considerable contribution to public life was rewarded with an OBE in 1964, a CBE in 1954 and a knighthood in 1966. In 1971, he was made an Honorary Fellow of Sidney Sussex College, Cambridge, and of LSE. Despite his knighthood and other public honours, Roy Allen was a modest man. He was a regular visitor to the Senior Common Room and was very approachable. He was interested in the fortunes of his junior colleagues and was always ready to listen to them and offer advice. He was quietly spoken and had a good sense of humour.

In 1936, Allen married Kathleen Lily, who was a former student of his at LSE. They had two sons and a daughter. He had a great love of music and the theatre. He was able to achieve so much through hard work and long hours: he would often return from a concert or the theatre and then work for several hours before retiring to bed.

He died of a heart attack on 29 September 1983, a few days before he was to attend a retirement party at LSE to celebrate his fifty-five years teaching there.

2 Academic Career

2.1 Teaching

Mathematics: Initially, Allen's teaching was concentrated in mathematics and he offered both basic and advanced courses. This continued until the late 1930s and culminated in the publication of his book *Mathematical Analysis for Economists* in 1938. In 1961, when it was decided to offer an introductory mathematics course as a first-year option in the BSc(Econ), Allen developed 'Basic Mathematics': a fundamental course that progressed from sets, groups and fields, via exponential and logarithmic functions to complex numbers, vectors and matrices, all in 25 lectures. This resulted in a corresponding textbook (Allen 1962).

Statistics: In 1936, Allen began teaching courses in statistics that reflected his growing interest in economic statistics. Initially, the courses combined the development of basic statistical techniques (such as different means, measures of dispersion, time series trends and fluctuations and simple methods of measuring correlations) with applications to economic statistics. After his return to LSE after the Second World War, this became the main focus of his teaching and he taught courses on index numbers, national income statistics, and international trade and the balance of payments. However,

while he positively glowed with enthusiasm for those subjects, he failed to ignite a corresponding glow in the hearts of the majority of students taking the courses.⁵

*Economics and Econometrics*⁶: In the 1938–1939 session, Allen taught a new course of fifteen lectures as an ‘Introduction to Mathematical Economics’. In addition to providing teaching in mathematics, the reading list referred students to Chamberlin and Joan Robinson on ‘Monopolistic’ and ‘Imperfect’ competition respectively, Knight on ‘Risk and Uncertainty’, plus the standard works of Jevons, Walras, Marshall, Edgeworth, Pareto, Wickstead, Wicksell and Pigou. This session also saw Allen teaching a ten-lecture course that was recommended for postgraduates on ‘Some Problems in Econometrics’. This was a pre-Cowles Commission course, and the econometric topics considered were ‘the deduction of elasticities of demand and supply from market data, the analysis of family budgets and the measurement of the cost of living’ (LSE 1938: 118). All of these courses were repeated in the 1939–1940 session, and there was a new course of nine lectures recommended for postgraduates on ‘The Economic Approach to Business Cycle Problems’, with Tinbergen’s econometrics studies and Frisch’s ‘Propagation Problems and Impulse Problems in Dynamic Economics’ (Frisch 1933) among the suggested readings.

Allen returned to LSE after his Second World War service, and in the 1946–1947 session, he taught a ten-lecture course on ‘Problems of Econometrics’ that was essentially his 1939–1940 business cycle course with the addition of Leontief’s input–output analysis. The course was repeated in the 1947–1948 session, but then discontinued and a course on econometrics disappeared from LSE for a number of years.⁷

Summary: In the period before the Second World War, Allen’s teaching was quite varied, involving mathematics, mathematical economics and econometrics as well as statistics. Upon his return to LSE after the war, his teaching was largely concentrated in courses on the sources of economic statistics and their interpretation. However, as we shall see below, his research and other publications covered a much wider range.

⁵I can vouch for this from the comments I received from my tutees and from my own experience as a student.

⁶To avoid duplication, Allen’s econometric involvement with the Cowles Commission and the Econometric Society is presented in Chapter 1 of this volume.

⁷The development of courses on econometrics at LSE is considered in more detail in Chapter 1 of this volume on ‘LSE and Econometrics’.

2.2 Research and Other Publications

Research: In evaluating Allen's research and output of other publications, it is convenient to divide them into his publications before the Second World War and those that appeared after his return to LSE after the war.

Before the Second World War: Between arriving at LSE in 1928 at the age of 22 and the outbreak of the Second World War, Allen published ten solo articles, four joint articles and two books, one of which was joint-authored. This is an impressive record. The majority of his articles were in economic theory and were published in respectable journals, with Allen using his mathematical skills to extend and generalise a number of areas of theory.⁸ The topics covered were 'The Foundations of a Mathematical Theory of Exchange' (1932a), 'Decreasing Costs: A Mathematical Note' (1932b), 'The Nature of Indifference Curves' (1934a), 'A Comparison Between Different Definitions of Complementary and Competitive Goods' (1934b), 'A Critical Examination of Professor Pigou's Method of Deriving Demand Elasticity' (1934c), 'A Note on the Determinateness of the Utility Function' (1935a), 'Some Observations on the Theory and Practice of Price Index Numbers' (1935b) and 'Professor Slutsky's Theory of Consumers' Choice' (1936).

There was a degree of proselytising in Allen's approach and, for example, in Allen (1932b), his second publication in economics, he examined the consistency of competitive equilibrium with decreasing costs that had been discussed in articles by Sraffa (1926) and Harrod (1930, 1931). Allen generalised the discussion and stressed the advantage of the more general approach:

Mr. Harrod, considering only a single supply schedule and a demand schedule, comes to the conclusion that marketing expenses must be included in the supply function, which, therefore, becomes dependent on the state of demand. This representation of the problem is an over-simplified one, and Mr. Harrod, I think, attempts to extract too much from it. Two-dimensional representations are easily over-strained (Allen 1932b: 325–326).

Allen's most important research in economics was that he carried out with John Hicks (Hicks and Allen 1934a, b) on value theory. While the

⁸Although Allen was officially a statistician, he had a strong interest in economics and attended the famous Robbins seminar in the 1930s (see Howson 2011: 250–252). This close connection continued, and in discussing the strengths of the Economics Department in the 1950s, Robbins included among the assets 'R.G.D. Allen, now Professor of Statistics, virtually a member of the department in his capacity of mathematical economist' (Robbins 1971: 218). The one exception to the run of articles on economic theory was 'The Assumptions of Linear Regression' (Allen 1939), which contained an algebraic analysis of an 'errors-in-variables' model with two variables.

publications divide their work into two separate components, with Hicks credited with the article that appeared as Part I in February 1934 and Allen with Part II in May 1934, Hicks made clear in his later writings that there was considerably more collaboration between them than the division suggests. In the first volume reprinting his writings on *Wealth and Welfare*, as a background to ‘A Reconsideration of the Theory of Value’, Hicks explains that:

I was led to the Fundamental Formula:

Price elasticity = k (income-elasticity) + $(1-k)$ (elasticity of substitution), where k is the proportion of income (or total expenditure) that is spent on that commodity. I think I got that far, before I began close collaboration with Allen; but the use of the income-elasticity, along an income-expenditure curve, or Engel curve, linked up with the work he had been doing on family budgets with Bowley—and so, very naturally, aroused his interest.

The Fundamental Formula is clear of any reference to measurable utility, so the proof of it should surely be capable of being set out in a form which has no such reference. This is done (with, I now think, some sacrifice of mathematical elegance) in the opening section of Allen’s part of the paper ... He went on from that to extend the analysis to the case of more than two goods, thus finding room for complementarity, along an indifference surface, or hypersurface. This was in origin his contribution (I think of it as his chief contribution) but it was of course absorbed into my “literary” version (Hicks 1981: 4)⁹.

‘A Reconsideration of the Theory of Value’ provided one of the inputs into the study of family expenditure in Allen and Bowley (1935). This was novel, as was noted by one reviewer:

This investigation differs from all previous family budget studies in that it is based definitely on some of the preconceptions of modern mathematical theory of utility and exchange. That theory enables us to explain the pattern of consumption of an individual in terms of his tastes, his income, and market prices. Its application to family budget data should yield useful and interesting results (Schultz 1936: 613).¹⁰

⁹In referring to this article in the Introduction to the volume, Hicks wrote: ‘This appeared in two parts, one of them being signed by me the other by my collaborator; but, as was explained, this did not properly represent the division of the work between us. He [Allen] has agreed, this being so, that both parts should appear here’ (Hicks 1981: xii). The importance of these two articles will not be explored further here to avoid duplicating material in the chapter on Hicks in this volume.

¹⁰There were other reviews by Williams (1936) and Marschak (1936).

In contrast to this theoretical work, Allen's two joint articles with Brinley Thomas were empirical studies using employment exchange data to examine the speed of adjustment in the labour market in the London building industry (Allen and Thomas 1937) and 'the supply of engineering labour under boom conditions' (Allen and Thomas 1939). They found in the second study that the boom produced a shortage of skilled workers, due in large part to a shortage of apprenticeships, a finding that has a modern ring to it, though solving the problem through migration was not a policy in the 1930s.¹¹

Other Publications: In 1938, Macmillan published Allen's *Mathematical Analysis for Economists* (Allen 1938), a book that provided a systematic treatment of the mathematical techniques thought to be useful for economists and based on lectures he had given at LSE since 1931.¹² It was a great success and, being without competitors for many years, was reprinted seven times, the last being in 1956.

After the Second World War: Research

On his return to LSE after the war and with his increasing involvement in public service (see below), Allen's research output changed markedly. While there was still some work involving economic theory, the focus was more on the measurement and collection of economic data and its analysis in the context of public policy. His publications in the second category may be grouped under a number of broad headings: economic policy: (1945), (1946a, b, c), (1956a), (1957a); index numbers: (1952), (1953), (1963); prices: (1948a, b), (1949a), (1950a), (1954a), (1958a), (1972); national income and expenditure: (1954b), (1956b), (1957b), (1958b), (1964), (1970); and balance of payments: (1951).

¹¹Brinley Thomas was one of the young economists at LSE who sided with Keynes in the Keynes versus Hayek controversy and claimed to have suffered as a result: 'The ruling powers were passionate believers in freedom, and this included freedom to adjust the constraints within which freedom was exercised by the non-favorites. The main type of adjustment was the postponement of tenure. In my own case I did not receive tenure until, on the advice of Sir Alexander Carr-Saunders [then Director of LSE], I moved from monetary theory to migration and economic growth' (Thomas 1991: 390).

¹²One reviewer compared Allen's book with Bowley's *Mathematical Groundwork of Economics* (Bowley 1924) and was 'struck immediately with the greatly extended use of mathematics that has taken place in the fourteen years between the two publications. For Bowley a short account of the elements of the differential calculus with applications to extreme values and the development of functions sufficed. With Allen we find, in addition to a great deal of quite elementary work, a fairly full account of the calculus, including a treatment of differentials, and also some account of differential equations, determinants and quadratic forms, and the calculus of variations' (Belz 1938: 269). There were also reviews by Davis (1939), Hicks (1939), Hotelling (1939), Schneider (1938), Tintner (1938), Tintner (1939), in which he states that 'Mr. Allen is one of the most prominent young English economists and statisticians, and he has himself made very important contributions to economic theory which he has partly incorporated in his treatise' (ibid.: 272), and Yntema (1940).

Interspersed with the publications listed above were a number of articles relating to economic theory. Allen (1949b) was concerned with ‘The Economic Theory of Index Numbers’ and discussed the work of Staehle, Hicks and Samuelson. ‘The Mathematical Foundations of Economic Theory’ (Allen 1949c) raised the question of whether mathematics should be the scaffolding or the steel frame of economic theory and argued strongly for the second possibility. Allen (1950b) was concerned with reconciling the Slutsky-Hicks theory of value with work by Samuelson. In ‘The Engineer’s Approach to Economic Models’ (Allen 1955), Allen reviewed Arnold Tustin’s *The Mechanism of Economic Systems* (Tustin 1953).¹³ Allen enthused about the potential use of the engineer’s closed-loop diagrams and presented and compared a number of economic models (Phillips, Goodwin, Hicks and Kalecki) in this format. He returned to this theme in later publications (see Allen 1956c, 1967).

Allen’s final contributions to theoretical economics were two short notes, jointly authored with E.J. Mishan on the substitution term (Allen and Mishan 1965, 1967).

Other Publications: Allen’s interest in economic statistics and index numbers was reflected in three of the books he published in the post-war period. *Statistics for Economists* (Allen 1949d) combined a simple introduction to statistical methods with illustrations of their use on economic data. In *International Trade Statistics* (Allen and Ely 1953), the two editors, with 23 other contributors, brought together information on the world’s presentation of statistical data on trade.¹⁴ Finally, *Index Numbers in Theory and Practice* (Allen 1975) presented a thorough exposition of index number theory with illustrations using mainly UK data.¹⁵

In the 1950s, Allen returned to writing books for economists with his *Mathematical Economics* (Allen 1956c). In the Preface to the first edition, he explains that there were three reasons why he decided not to extend his 1938 *Mathematical Analysis for Economists*, but to write a new book.

¹³Arnold Tustin was an electrical engineer with wide interests, and in the book under review, he showed how a wide range of economic models could be represented by closed-loop systems. At the time he was writing the book, he was Professor of Electrical Engineering at Birmingham University and he acknowledged getting feedback on the text from his colleagues, Frank Hahn and Terence Gorman.

¹⁴Chapter 16 on ‘British Colonies and Dependencies’ contains data on many of the world’s tax havens, but as information on the capital account is not considered in the book, it has little of interest to offer the modern reader. For reviews, see Adler (1954), Kindleberger (1954), Makower (1955) and Morgenstern (1955).

¹⁵The absence of references to US data and exercises was criticism raised by two reviewers (see Fisk 1976 and Folkerts-Landau 1976).

The first was the expansion of the range of mathematics developed in parts of mathematical economics. In addition:

A second development of the last twenty years is the growth of econometrics. This has been so rapid that I think there is some risk that the necessary development of economic theory, formulated in a way which makes econometric sense to a statistician, will lag behind rather seriously. Such formulations of economic theory must be in mathematical terms but simplified as far as possible.

Finally, the change in the direction of economic thought over the past twenty years has involved a considerable upheaval in the structure of economic theory. This is partly, though by no means entirely, the result of the work of Keynes. I believe that there is now a real need for some synthesis of the “new” economics, for some calm survey of the form and scope of economic theory (Allen 1956c: v).

In contrast to his *Mathematical Analysis for Economists*, which was essentially a mathematical text with economic examples, the new book was a text covering both economic theory and mathematical techniques. In response to the two points quoted above, Allen did not follow the standard economic textbook approach of using timeless comparative statics but began the text with three chapters on dynamic models, in which the variables have time subscripts, so that, in principle, an econometrician with suitable economic data might estimate the parameters in the models.

A novel feature of the book is Chapter 9, ‘Economic Regulation: Closed-Loop Control Systems’, in which Allen extended his discussion of the work of Tustin from his review (Allen 1955) and also covered the work of his colleague A.W. (Bill) Phillips.¹⁶ The dynamic discussion is followed by a mathematical section, in which game theory and linear programming are developed before being applied to the discussion of the allocation of resources and the theory of the firm.¹⁷

Allen’s next textbook contribution to economics was his *Macro-Economic Theory: A Mathematical Treatment* (Allen 1967). The structure of the book

¹⁶See Phillips (1954a, 1956, 1957). Phillips reviewed Tustin’s book (see Phillips 1954b). Both Tustin and Phillips agreed that analogue models would help to deal with nonlinearities in economic systems, but whereas Tustin’s analogue models were electrical circuits, Phillips developed the Phillips Machine, in which coloured water flowing through tubes was used to show the workings of an economy (see Phillips 1950), which had more visual appeal than electrical circuits to economists. For a full discussion of Phillips’s work, see Chapter 23 in this volume.

¹⁷The book was reviewed by Baumol (1957), Carter (1958), Christ (1958), Fabian (1958), Morgenstern (1958), Roy (1957) and Theil (1958).

involves a dynamic approach to macroeconomic modelling, and while it ranges far outside the conventional treatment of macroeconomic theory, the Preface offers a warning about concentrating too much on fashionable topics, such as the turnpike theorem¹⁸:

Much has been written about the Keynesian short-period analysis and on models of long-period growth. More recently, the attention of mathematical economists and econometricians has turned to such optimisation problems as those based on the Turnpike Theorem. The present text aims at a uniform treatment of equilibrium in the short-period and in the long-period, and it goes on to develop models of disequilibrium. In doing so, it stops a good deal short of the Turnpike. There is, in my view, a large area to be explored, lying between growth theories and optimisation, and having to do with disequilibrium in the medium-run as analysed in the last main section here [four chapters on Medium-Period Disequilibrium Cycle Models]. This area is worth intensive cultivation, not only by theorists but also in econometric terms. I hope that not all econometricians are committed to pushing along the Turnpike (Allen 1967: xi–xii).

The material in this book covered ground already traversed in Allen's *Mathematical Economics*, but with a different emphasis and with more discussion of growth models.¹⁹

3 Conclusion

As this summary of Allen's teaching, research and other publications makes clear, he was a fine mathematician who used this ability with much skill in his contributions to economics in both fundamental research and his approach to teaching economics. His interest in econometrics was an early response to the growing possibility of bringing economics and statistical testing together. Overall, this was a significant contribution to economics from a statistician. Allen also made major contributions to political decision-making through his involvement with public inquiries and other activities, which were recognised through the award of a CBE, OBE and a knighthood.

¹⁸See McKenzie (1998) for a discussion of the turnpike theorem.

¹⁹It received somewhat mixed reviews from Ball (1969), De Menil (1969), Dernberg (1968) and Parry Lewis (1968).

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21

Richard Sidney Sayers (1908–1989)

Alec Cairncross and Charles Goodhart

1 Introduction¹

The Radcliffe Report (1959), otherwise known as the Committee on the Working of the Monetary System (HMSO Command 827, August), was, and remains, the most important examination of monetary affairs in the UK in the decades since the Second World War. The Committee was appointed (by Treasury Minute) on 3 May 1957; it began taking evidence on 11 July 1957, and the final date on which evidence was taken was 30 April 1959; and the Report was issued in August. The key members of the Report were Lord Radcliffe and the two economists on the Committee, Alec Cairncross and Richard Sayers, though the Secretary, Robert Armstrong, also played an important role behind the scenes.

Alec Cairncross died in October 1998.

¹This chapter is derived from Cairncross (1991) and adapted by Charles Goodhart with the permission of the British Academy. In preparing this memoir, I [Cairncross] have drawn heavily on the recollections of Leslie Pressnell and Stuart Wilson and have been greatly assisted by Sayers's daughter, Jennifer, his son, Michael, his sister, Mrs. Whitaker, and his younger brother Walter. Others who have provided useful information include Theo Barker, Alan Day, Henry Phelps Brown and the Revd Stella Taylor.

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Cairncross, later knighted (1967), was a leading macroeconomist of the day, who had published a widely used textbook on macroeconomics (*Introduction to Economics*, 1944, sixth edition 1982), but was not a monetary specialist. At this time, Sayers was the pre-eminent money and banking specialist economist in the UK. His best-known work was his textbook, *Modern Banking*, whose first edition was in 1938, and whose final edition (seventh) came out in 1967. So, it was always going to be likely that Sayers would be chosen as the monetary specialist to be on the Radcliffe Committee. Indeed, these three main protagonists, Radcliffe himself, Cairncross and Sayers, dominated the questioning and both the analysis and substance in the subsequent Report. In this exercise, Cairncross worked for slightly over two years in continuous close conjunction with Sayers.

So, when Sayers died in 1989, Cairncross was the natural choice to write the article-length memoir on his life for the British Academy. Cairncross wrote this memoir shortly after Sayers's death, published by the Academy in 1991 and had the advantages, not only of their prior close cooperation, but of immediacy and access to colleagues, while memories of Sayers remained fresh. This means that Cairncross's memoir, republished below, with hardly any alterations (thanking the British Academy for their agreement to allow this), contains much more detail than could be put together at this much later date when many of those close to Sayers at the time have gone.

Perhaps the only advantage that one has, compared with Cairncross's excellent memoir, is the lapse of time which can allow for a slightly greater perspective and also may relax some of the constraints about referring to his disposition and personal life.

The conclusions of Radcliffe were contentious. In contrast to the relatively simple proposals and arguments of the growing band of monetarists, under the developing influence of Milton Friedman in the USA, the Radcliffe Report was circumspect and cautious in its arguments about what monetary policy should, or could, do and sought to replace attention on measurable quantities of monetary aggregates with a focus on a largely unmeasurable, indeed hard to define, concept of liquidity. Thus, in the concluding chapter of the Report (Chapter XII, paragraph 981), the Report states:

Secondly, the factor which monetary policy should seek to influence or control is something that reaches beyond what is known as the "supply of money". It is nothing less than the state of liquidity of the whole economy. The behaviour of our economy—in particular, the moderation or pressure of demand from time to time—is influenced by the relative liquidity of potential spenders at any one time, and thus, at one remove, by the liquidity of those who might act as lenders to them or subscribers to their funds.

While the downgrading of the monetary aggregates, and the quantity theory of money, infuriated the monetarists, the fuzziness and lack of clarity of the proposed alternative concentration on ‘liquidity’ left even the likely supporters of such an approach more than a little bemused. Roger Alford, Sayers’s colleague at LSE, commented in his subsequent book, *Life and LSE* (Alford 2009: 223), as follows:

The Report was not well received; it was widely felt to lack a clear and workable framework for monetary policy and this criticism was a great disappointment to Richard. I can remember Lionel Robbins’s seminars in the Graham Wallas Room at which the Report was discussed; Richard sat at the back and at every effort to clarify what the Report was really saying, he pursed his lips and shook his head gloomily, implying that yet another speaker had missed the point.

In some respects, with the collapsing reliability of demand for money functions in the 1980s, the subsequent decline of interest in the monetary aggregates and the growth of ‘shadow banking’, the emphasis of Radcliffe on a wider but fuzzier concept of liquidity might be better received now than it was in the 1960s and 1970s, when monetarism was the fashionable new idea. But in other respects, the methodology and approach that Sayers espoused have been on a steady downhill path since then. His great strength was always as a historian, and his approach to monetary, financial and banking issues was descriptive, institutional and historical, which allowed considerable reliance on quantitative data, but eschewed econometrics, and included virtually no maths or equations whatsoever. This approach had a long pedigree at LSE with Barrett Whale and Sir Theodore Gregory, but this institutional/historical approach became increasingly under threat, not only at universities elsewhere, but even at LSE over the course of subsequent decades. One of the results of this has been that Sayers’s main textbook, *Modern Banking*, which had been read by almost every economics undergraduate in the UK from 1938 to the mid-1970s, is now rarely read or cited.

Sayers’s reputation as a money and banking *historian* remains, however, undiminished, and the many historical works that he produced, which are described in Cairncross’s memoir—notably *Bank of England Operations, 1890–1914*, *Financial Policy, 1939–1945*, *Central Banking after Bagehot*, *Lloyds Bank in the History of English Banking*, *Gilletts in the London Money Market* and *The Bank of England, 1891–1944*—remain the most important accounts of such historical developments. Also, the many current studies which he did then, often in conjunction with his student, J.S.G. Wilson, e.g. on *Banking in the British Commonwealth*, although somewhat stodgy, remain valuable documents about how the institutional set-up of central

banks and banks in a wide variety of countries operated at the time. So Sayers remains a foremost banking historian.

For the rest, he was even perceived at the time as becoming somewhat old-fashioned, and the historical/institutional approach has been largely put on one side to be replaced by much more rigorous mathematical models, though ones which tend to assume a given and constant institutional background and whose underlying assumptions often hardly bear detailed scrutiny.

In addition to being technically old fashioned, Sayers was not a man to engage in easy collegial activities. Face to face he was not an easy person, but he was a great help to many during their careers in numerous ways. On paper, he was able to express warmth that so often eluded him in personal meetings. He was intensely private, withdrawn, proud at what he had done and introverted. I was myself a member of Sayers's Money Group for a couple of years, 1966–1968, at LSE, and I had a good relationship with Richard and was able to talk with him on several occasions; but, in general, people approached Richard with trepidation, and he did not participate in standard academic socialising, as this paragraph from Roger Alford's book records:

In 1968, Richard Sayers retired and there was the customary retirement dinner for him; it was felt that, in his withdrawn way, he might decline to attend, so his wife guaranteed to get him there on time without him knowing what was coming. It was not a very cheerful occasion. Many of us felt that he was retiring as a disappointed man; the Radcliffe Report, of which he was a leading figure, had not been well received and had failed to make an impact on monetary thinking and monetary policy (Alford 2009: 324).

Things then went from bad to worse. After retirement, he left his wife for a woman who had been his research assistant for his historical book on Gillett Brothers, thereby alienating his family. He ended up in a down-at-heel house at Clacton-on-Sea, increasingly concerned that he was losing command of his intellectual faculties and upset that the tradition of historical and monetary scholarship that he had maintained at LSE was under threat. It was an unhappy ending for a man who had been a true scholar in his chosen field.

2 Early Life

Richard Sidney Sayers was born in Bury St Edmonds in Suffolk on 11 July 1908. He prided himself in afterlife on being a Suffolk man and looked forward to retiring to Suffolk. He did in fact move to Long Melford a year or

two after his retirement, although reluctantly he moved on again later, first to Essex and then to Sussex.

Of the seven children in the family, Richard was the fifth. The oldest, a boy, died in infancy. Then came a girl, four boys and another girl. Richard's oldest brother, Frank, worked in Lloyds Bank, whose history Richard subsequently wrote. Another brother became a squadron leader in the RAF. Richard was the only one to attend university. His older sister, Margaret, who studied economics at night school and may have kindled his interest in the subject, might also have gone to university but was unable to take up a place at LSE which she won, along with a gold medal, in a Royal Society of Arts examination.

The family belonged to the lower-ranking part of the professional class, Sayers's father, Sidney James Sayers, describing himself on the birth certificate as County Council Finance Clerk. He had attended night school like his daughter and trained as an accountant, working for 40 years as West Suffolk County Accountant in Shire Hall at Bury. Something of Sayers's father's concern with accounting shows up in his son's precise analysis, in balance sheet terms, of the successive effects of a change in a bank's cash reserves on its lending operations.

Sayers went to a succession of schools in Bury from 1912 to 1926, the last nine of those years at West Suffolk County School. He was inclined to speak disparagingly of the school despite being Head Prefect for two years (he retained many of the characteristics of a Head Prefect all his life). He showed no great interest in sport, although his older brothers were in the soccer First XI. But when taunted again and again at the age of 14 or 15 with his inability to play football and match their achievements, he rounded on the pack with a tremendous outburst of temper, pledging his selection on merit by a fixed date when he would play and play well. Although he was listened to with scorn and disbelief, he abandoned his books, trained regularly and hard, took all the knocks without complaint and was duly selected to play by the date he had set without any favouritism on the part of the selectors. He played in one game with grit and determination, and then abandoned football for good, telling the headmaster the full story and asking that he should never again be selected. 'My studies' he declared 'are more important'.

When it was decided to enter him for Cambridge, the 'local university', he found it necessary to go to a crammer for 'Little-Go' as he had no Latin. He had, however, been able to begin the study of economics in his final year and in the summer of 1926, before he went up to Cambridge, he was already reading Marshall and Taussig in preparation for Part I of

the Economics Tripos. In December of the previous year, he had sat the Scholarship Examination for Gonville and Caius College and been placed in the Exhibition Class in history. Neither Caius nor St. John's (to which he had also applied) could take him before 1927 but he was offered and accepted a place at St. Catherine's.

Throughout his life, Sayers hesitated between economics and history and the hesitation was evident in his proposed course of study at Cambridge. He had intended to take Part I of the Tripos in Economics and Part II in history but ended up taking both Parts of the Economics Tripos, graduating in 1929 with a First in Part II. Nevertheless, he retained his interest in history and his closest associations when on the staff of LSE were with scholars who showed the same liking for a combination of history and economics: Barrett Whale and Theodore Gregory in pre-war days and T.S. Ashton after the war.

At Cambridge, he was tutored by Gerald Shove, now a rather obscure figure but in his day one of the most powerful and original minds in economics. Sayers attended Keynes's lectures at a time when he was lecturing from the proofs of *A Treatise on Money* and was duly awed by the great man who made him a member of his Monday night 'Keynes Club'. In later life, however, he was a follower of Dennis Robertson rather than of Keynes and it was to Robertson that he habitually sent drafts of his work before publication.

During his studies at Cambridge, Sayers became attached to an old friend and form-mate from Bury, Millicent Hodson (the school was co-educational), and in September 1930, a year after graduation, they were married in Bury Cathedral. He then embarked on a year of graduate study at Cambridge, while his wife taught in a school in Dry Drayton. Later, she devoted herself to helping her husband with his writing and bringing up two children, a boy and girl. Sayers did not involve her much in academic contacts and, in post-war years at least, his colleagues hardly ever saw her.

3 Starting as an Academic

In 1931, Sayers was appointed as an Assistant Lecturer at LSE where his main duty was to assist Barrett Whale. It was an association to which he looked back with pleasure for 'the rich and generous source of ideas, inspiration and encouragement' (Sayers quoted in Robertson 1951: 440) that Barrett Whale provided and the debt was acknowledged wholeheartedly in successive editions of *Modern Banking*. It was at LSE that Sayers began to learn about banking, working with Barrett Whale and Gregory and taking

part in seminars with a team from the City who later cooperated to produce a very early study of comparative banking in a variety of countries entitled *Commercial Banking Legislation and Control* (Allen et al. 1938).

Sayers remained at LSE for four years before moving to Oxford as a college lecturer, holding appointments at Corpus Christi, Exeter and Pembroke and becoming a Fellow of Pembroke in 1939. He had used his time at LSE to conduct extensive research on monetary policy before the First World War, making thorough use of money market reports in *The Economist* and *The Statist* but without the access to Bank of England records subsequently accorded to Clapham when preparing his official history of the Bank up to 1914. He had applied, with Gregory's help, to be allowed to consult the Bank's records, and although Montagu Norman refused, it seems to have been this that put into his head the idea of having an academic like Clapham prepare a history of the Bank for its 250th anniversary in 1944. If so, Sayers played a key part in opening up the archives of the Bank.

Bank of England Operations, 1890–1914, which was published in 1936 when Sayers was already in Oxford, is not perhaps one of his major works but it established his reputation as a monetary historian. Unfortunately, the type of the book was destroyed in wartime bombing and this—and the war itself—limited the attention it received. In the originality and meticulousness of the research, the organisation of the material and the clarity and elegance of the exposition, it is a model of its kind. The evidence is carefully marshalled from scattered sources and woven into a clear line of argument with no attempt to claim more than can be demonstrated.

Sayers provided convincing evidence of the wide difference between the conventional view of the working of the pre-war gold standard and the actual course of events. Instead of a smooth and more or less automatic mechanism of adjustment to balance of payments pressures through variations in Bank Rate, Sayers showed that even in its heyday the international gold standard 'worked under disadvantages of the same kind, though not to the same degree, as [regimes] of post-war years'. With tariffs, war debts, reparations and political insecurity, it was 'remarkable that the gold standard ever worked at all' (ibid.: xx–xxi). 'Even if there was a much smaller supply of internationally mobile funds...our fathers and grandfathers must have been less sensitive, less ready to take fright at the succession of shocks which occurred' (ibid.: xxii).

Sayers went on to show what difficulties the Bank of England had in maintaining the convertibility of the currency. It could neither afford large reserves nor was its power over market rates of interest assured. On the contrary, it had to resort to all kinds of shifts to bring rates closer to Bank Rate

and find other ingenious devices to protect its gold reserves by placing obstacles in the way of withdrawals.

Once at Oxford, Sayers embarked on the more ambitious project of writing a textbook on *Modern Banking*. First published in 1938, this was and remained the outstanding textbook on banking operations and on the practical and institutional aspects of banking. A strong motive in writing it, he once remarked, was the approaching birth of his first child and the usefulness of additional income; this was by no means the only occasion on which he responded to the prospect of a flow of royalties.

It was written at a time of upheavals both in theory and in economic activity. There was an intense interest in monetary problems and never-ending controversy over the revolutionary new ideas that kept appearing. The Macmillan Committee had issued its report at the beginning of the decade. Book after book on money and banking by Keynes, Robertson, Hawtrey and other leading economists were published in the next few years. It was no mean task in these circumstances to synthesise and summarise 'current theory' and produce what Sayers described as a 'restatement of ideas which are the subject of agreement among most economists'; the 'restatement' required the same kind of intellectual effort as the works synthesised. Sayers's aim was to write a textbook on banking that would help honour students 'to understand how this important part of the economic system really works nowadays'. These comments were included in the Preface to the first edition of *Modern Banking* in 1938 but were restated in the Preface to the first few editions written after the war, notably on page six of the Preface to the third edition published in 1951; this indicates that Sayers himself attached particular significance to these two passages. In the volume, Sayers confined himself largely to English institutions with occasional references to American practice but little on other countries except a chapter on banking in the developing countries. In the first edition, he also dealt with the international monetary system, the stock market and the nationalisation of the banks but these chapters had largely disappeared by the third edition in 1951.

With *Modern Banking*, Sayers became an internationally recognised economist. The book was for many years the standard work in Britain and indeed in many other countries, ran to seven editions, each largely rewritten. The last, in 1967, was said a few years ago to be still in use in India.

Although Sayers made little claim to originality, no good textbook can be a mere summary of other people's ideas and *Modern Banking*, which was decidedly a good textbook, gave expression to many original thoughts that are prominent in Sayers's later writings. By the second page, he was

already presaging that emphasis on liquidity which some readers of the Radcliffe Report were later to find so baffling: ‘The economic significance of a change in the supply of money’, he maintained, ‘is based on the disturbance of the liquidity-distribution of the public’s assets’ (ibid.: 2). By page 3, he was explaining how the supply of money can respond to a change in the demand for it, as when an individual is moved to borrow from a bank in order to make an advantageous purchase. Another example occurs in his discussion of the interaction of short- and long-term rates of interest. Well before Mr. Dalton, he urged that ‘to maximize the effect in pushing up gilt-edged prices...the banking policy of low short-term rates should be accompanied by propaganda such as we had in 1932’ (ibid.: 155). Finally, there is an indication of his later scepticism about monetary policy in his statement that ‘I know of no case in monetary history of a dear money policy alone producing a general deflation of money incomes’, followed within a couple of sentences by the apparently contradictory judgement on Bank Rate that ‘for producing a general revision of money incomes it is a halting, clumsy, indeed a brutal, instrument’ (ibid.: 164).

In the years immediately before the war, Sayers became involved in the work of the Oxford Economists’ Research Group which sought to establish empirically whether businessmen followed the practices which seemed self-evidently rational to economic theorists. Sayers’s part was to prepare a summary of the replies from a large number of businessmen to enquiries as to their reactions to changes in interest rates when deciding on capital expenditure (Sayers 1940). In 1951, when this and other papers were reprinted in *Oxford Studies in the Price Mechanism*, Sayers contributed a new paper, ‘Business Men and the Terms of Borrowing’ (Sayers 1951), which threw even more doubt on the influence of changes in interest rates in controlling investment.

4 Wartime and Then LSE

In wartime, Sayers worked in the Ministry of Supply where his duties carried him into the hush-hush area of the atomic bomb. He was concerned with the development of uranium supplies and was one of the negotiating team sent to Washington in 1944. His part in the preparation of a joint report is highly praised by the official historian who noted Sayers’s intellectual powers, literary ability, tact and good temper, all of which contributed to an excellent draft report.

When the war was over, he was persuaded by James Meade to join the Economic Section of the Cabinet Office and remained there for two years as a joint Deputy Director alongside Marcus Fleming. While Fleming concentrated largely on external economic policy, Sayers dealt with domestic issues, including the price and investment policy of the nationalised industries (he took a particular interest in transport charges), wage policy (here, he laid great stress on education and persuasion much as he had done in relation to long-term interest rates) and industrial policy generally. In the summer of 1946, he was brought in on external economic policy and briefed the Lord President in favour of maximum blocking of sterling balances and full use of IMF facilities. Later in the year, he was sent to Paris to help Sir David Waley (not that Waley wanted any help) in discussions on financing arrangements for intra-European trade.

When Sayers joined the Economic Section as Deputy Director, he seems to have contemplated a long spell in public service but in 1947 he opted to resume his academic career, accepting appointment as Sir Ernest Cassel Professor of Economics at the LSE. There he remained until he took early retirement—as he had long intended—in 1968.

He busied himself with a sketch of the American banking system published in 1948 and the preparation of a new edition (the third) of *Modern Banking*. Thereafter, he turned to the second of his major works—some would say his best—*Financial Policy, 1939–1945*. This was a volume in the series of official histories edited by Sir Keith Hancock and perhaps the most difficult and challenging assignment of the entire series. It took over five years to complete, appearing finally in 1956. Sir Wilfred Eady, who had been Second Secretary in the Treasury in wartime, reviewed it for the *Economic Journal* and had nothing but praise, calling it ‘an exciting book’ (Eady 1957: 494). Brian Tew, in a more circumstantial analysis, was even more enthusiastic: ‘The story as it unfolds’, he wrote, ‘is so fascinating and so well told that it deserves to be something of a bestseller’ (Tew 1957: 504). But the market for histories of financial policy, however exciting, is a limited one, especially if they cover the ground in detail and are works of scholarship, not vulgarisation. In comparison with *Modern Banking*, *Financial Policy* remained virtually unknown to the professional economist.

It was a study that drew on Sayers’s twin gifts as economist and historian and was written with his customary elegance and lucidity. The story revealed how it was left to a small group of administrators and professional

economists in the Treasury to wrestle with momentous issues of the most diverse kind. In telling it, Sayers had two important advantages. He had unlimited access to the official files and could consult those members of the Treasury team who were still alive. Exceptionally, he was also allowed to let the more prominent members of the group who were wartime recruits and not established civil servants—Keynes, Henderson, Robertson and Catto, for example—emerge from the shadows of anonymity and be named as the authors of contributions to the policy debate. The story could thus be told in full detail and with the added colour and life that personalities lent to it.

It was a story in which it was necessary to be quite clear about the role of finance in a war economy and Sayers made this his starting point. But it was also a story of political pressure: of tensions and their handling; of deciding what policies were feasible and sustainable, not just attractive in economic logic; and of finding persuasive arguments and forming sound and convincing judgements about an uncertain future. Sayers dealt with both economic and political issues with great skill and recreated the atmosphere of the wartime Treasury, conveying to the reader the sophistication of the arguments employed, the relief and satisfaction when negotiations were successful and the frustration and disappointment when they failed or were prolonged indefinitely. While the first half of the book on the development of domestic policy gave the impression of successful outcomes, the latter half on external economic policy was in the words of Brian Tew, ‘a depressing chronicle of frustrations’ (Tew 1957: 504).

The fifties were a highly productive period in Sayers’s life. In addition to *Financial Policy*, he produced two other books: a collection of essays, most of them written in 1955–1956, *Central Banking after Bagehot*, and a full-length study, *Lloyds Bank in the History of English Banking* (both published in 1957). The Lloyds study was his favourite book (though not his best) perhaps because it gave full scope to the historian in him to dwell on the personalities of bankers rather than the propositions of economists. He also edited *Banking in the British Commonwealth* (1952) and (with T.S. Ashton) a collection of reprints of *Papers in English Monetary History*. A fourth edition of *Modern Banking* was in the press and appeared in 1958. The spate of books ceased after he was appointed a member of the Radcliffe Committee on the Working of the Monetary System in the spring of 1957. It was the most important assignment in his career and occupied him for over two years.

5 Radcliffe Committee

Sayers played a dominant part in the Committee's affairs. Since he was the only expert on money and banking on the Committee, it was inevitable that he should take the lead in the more technical parts of its work. The Chairman, Lord Radcliffe, regarded him as 'our fast bowler' and usually brought him on early in the interrogation of witnesses. Some complained afterwards that hardly anybody else put questions to them.

What was less clear was what part he would play in the preparation of the Committee's Report. Radcliffe offered no guidance as to who should prepare a draft and to Sayers's embarrassment the Secretary, Robert Armstrong, set about the job and was well on the way to completing the draft before anybody knew. Sayers and I [Cairncross] saw no possibility of arriving at a satisfactory text by amending the draft and decided that it must be quietly set aside. This could not be done, however, without agreement on alternative arrangements for the preparation of a report. The Chairman wanted to draft the final chapter on the constitutional issues but no other member of the Committee aspired to draft any of the other chapters. Sayers and I, therefore, took on the job and nobody dissented. He concentrated on the central issues of domestic monetary policy and wrote about two-thirds of the whole while I tackled the trimmings: the introduction, the capital market, statistics and international finance.

In the autumn of 1958, in the middle of the Committee's work, Sayers had an attack of a virus akin to polio (probably Coxsackievirus) in the tongue at the end of a visit to the USA.² This affected his speech and made it doubtful whether he would be able to continue on the Committee. Fortunately, he recovered sufficiently to rejoin it and to resume drafting. When the Report appeared in the summer of 1959, it was obvious to those in the know that the main ideas in it were his.

This is not the place to re-examine these ideas and the criticisms they encountered. Those who reacted most strongly against the Report were the economists who were later christened 'monetarists'. They insisted that there was a fundamental difference between money and other financial assets and were highly critical of the Report's emphasis on liquidity as the appropriate focus of policy. The money supply seemed to them more measurable and less vague than the state of liquidity. These criticisms made little impression

²Infection with Coxsackievirus can produce weakness and paralysis by damaging the muscles (not the nerves as in the case of polio) but the muscles generally recover completely.

on Sayers. The fact that payments are made in money, not in other financial assets that can be readily cashed for money, seemed to him beside the point; he would have rejected the notion that policy must always bear on measurable quantities like the money supply rather than on confidence, incentives, liquidity and attitudes that are incapable of precise measurement.

What excited most controversy was the apparent belittling of monetary policy. The Report seemed to some people to be saying that money did not matter. In fact, it said no such thing but warned the authorities not to put too much faith in the power of monetary policy. It argued that in dealing with a boom or a slump ‘monetary policy can help, but that is all’ (Radcliffe Report 1959: paragraph 514). Of course, if pushed to the extreme of a liquidity crisis, the effect would be dramatic and inescapable. In all other circumstances, it was the limitations of monetary policy that Sayers wishes to emphasise.

The views he expressed in the Report of the Committee were not a brand new set of ideas flowing from the hearings. On the contrary, many of them—particularly those relating to liquidity—can be found in the papers in *Central Banking after Bagehot*. The limitations of Bank Rate, the desirability of steady long-term rates and occasional ‘changes of gear’ and the need to operate deliberately on the terms of lending across the whole financial spectrum, are all expounded in that earlier volume.

Sayers was bitterly disappointed by the reception of the Report—‘two years of my life—two years, wasted!’ he once exclaimed. His teaching at LSE and seminars he conducted there on Monday evenings, at which leading City figures were often invited to speak, were deeply affected by his reactions to widespread criticism of Radcliffe doctrine.

6 Later Work

His disappointment did not prevent a considerable volume of new work in the 1960s and, after his retirement in 1968, in the 1970s. New editions of *Modern Banking* appeared in 1960, 1964 and 1967. He contributed a study of *The Return to Gold* in 1925 to the volume of *Studies in the Industrial Revolution* edited in 1960 by Leslie Pressnell, edited a volume on *Banking in Western Europe* in 1962 and the *Economic Writings of James Pennington* in 1963 and delivered the R.C. Mills Lecture in Sydney in 1965 on ‘The Vicissitudes of an Export Economy: Britain Since 1880’. Other work followed in 1967–1968: *A History of Economic Change in England, 1880–1939* (1967); a contribution to the OECD volume on *Fiscal Policy*

for a *Balanced Economy* (1968); and a centenary history of Gilletts Discount House, *Gilletts in the London Money Market* (1968). The last of these followed an earlier work, *Gilletts, Bankers at Banbury and Oxford*, written under his supervision by Miss Audrey Taylor (who had held a temporary Fellowship at an Oxford college). It had not been Sayers's intention to write the history of the firm but he decided to do so in honour of a much-respected banker when Ronald Gillett, the head of the firm, died prematurely in 1965, shortly after the publication of Miss Taylor's book.

Apart from the histories of Lloyds and Gilletts, Sayers was much in demand as a historian of banking institutions. I can remember being asked about 1970 what the chances were of his accepting an invitation to write a history of the Federal Reserve System but whether he was ever formally approached I cannot say. What he did undertake was the equally demanding task of producing a sequel to Clapham's history of the Bank of England to 1914. This was completed in 1976 in two volumes covering the years from 1891 to 1944. To end in 1944 rather than in 1946 with the nationalisation of the Bank seems rather odd but was presumably intended to allow the narrative to end with the 250th anniversary of the foundation of the Bank and with the retirement of Montagu Norman from the Governorship after 20 years.

Sayers took great pains over the work. He made full use of the Bank's archives and interviewed many of the leading figures, or their relatives, in the UK, the USA, France, Germany and other countries. In digging into the Bank's accounts, he was able to use his mastery of balance sheet changes, as he had done in the early chapters of *Modern Banking*, in order to show how monetary policy worked. The result was a study on at least the same level of erudition as Clapham's and superior in the ease and elegance of its style, although some have found it less readable.

In covering the period between 1890 and 1914, Sayers was returning in his last book to the subject of his first with the advantage of an insider's view of events. This produced no surprises but he was able to avoid a mere repetition of earlier work and to produce a more authoritative account.

The treatment of the later period after 1914 adds more to what was previously known in several respects. It gives a fuller picture of the Bank's relationship with government, including full details of the conflict with Cunliffe; of Norman's part in European reconstruction and other international dealings of the Bank; and of the Bank's efforts to assist in industrial reconstruction in the 1930s. It is true, as Sidney Pollard maintains in one of the few reviews to appear, that political issues count for more in this period, not only domestically but internationally. The struggles in a

depressed interwar economy between the groups asked to share the burdens of depression are echoed in the struggles between rival financial groups in Britain, France and Germany over the investment opportunities associated with reconstruction in countries like Austria and Hungary. But if Sayers gives limited space to those struggles and to the political background to them, preferring to concentrate on clarifying technical detail and the aims and operations of the Bank, this is surely understandable. In a history that already occupies three volumes, it is surely right to give priority to the outlook and influence of the Bank rather than stray too far into all the conflicting forces affecting its operations.

The book was highly praised—it was ‘a work that will take its place among the classics’ (Pollard 1978: 673)—but left Sayers himself dissatisfied. He told his sister that he would never write another book. Nor did he. After 1976, he wrote very little but did deliver in 1979 the Keynes Lecture to the British Academy, returning to his favourite topic of Bank Rate in a survey of ‘Bank Rate in the Twentieth Century’.

7 Other Activities

Sayers was in great demand as an editor and as a consultant on publications. He was, for example, the ‘chief architect’ of the *Three Banks Review*, acting as editorial adviser from its launch in 1948 until his retirement 20 years later.³ He set the pattern of the review, which always carried a historical article, as well as a general economic article and one on a more specific industrial or commercial subject. He brought in many of the contributors and contributed himself but showed a distinct preference for articles in the areas he prescribed over articles in his own special field of banking and finance, which seldom appeared. As a member of the editorial committee, he is said to have been good company, modest about his own work, but firm over any questioning of his proposals on policy or of his editorial standards.

He was also for a time closely associated with the editorial side of *Economica* and was Chairman of the British Academy’s publications committee from its inception in 1969 until 1974. This was at a time when the finance of academic publications was becoming increasingly costly and difficult and it was necessary to consider subsidies and alternative methods of

³His role in that capacity is the subject of a tribute by Donald Fair in the September 1989 issue of *The Royal Bank of Scotland Review*, the successor to the *Three Banks Review*.

publication. Sayers's own publishers, like the Academy's, were the Oxford University Press to which he acted for many years as a consultant.

Sayers was a superb lecturer, taking immense pains over his lectures and shutting himself up in his room beforehand, virtually to memorise his lecture before delivering it. Both in lecture and in conversation, he expressed himself slowly and with deliberation. He was always clear, concise and well within the allotted time. Latterly, however, he lectured less and less and from about 1963 until his retirement in 1968 gave no more than a few lectures each year. His weekly seminars were shared with Roger Alford, Leslie Pressnell, Alan Day and, for a time, Charles Goodhart.

Students stood in awe of him for his eminence and erudition but felt little of the affection some teachers inspire. They were said to scramble at seminars for seats well away from him in case they were easily spotted and obliged to answer awkward questions. He saw each of his students briefly at the beginning and end of term and gave particular attention to any student whom he knew to be in personal difficulties. In the case of an Indian student under stress, for example, he invited him to his home in Sevenoaks for a weekend. But he could also be brutal in his criticism if, for example, he suspected a student of borrowing other people's ideas without acknowledgement. There were occasions on which Sayers's colleagues feared that the victim might commit suicide and hurried to offer reassurance that the violence of the criticism reflected Sayers's ill health.

Before the war, Sayers had been happy to work with Barrett Whale and to mix with others at LSE. When he rejoined the staff in 1947, he struck up a close friendship with T.S. Ashton who, like Sayers, combined economics with economic history. Ashton had begun as an economic historian and had been tempted back to Manchester by George Unwin, only to find himself required to teach currency and banking, never economic history. He had been in the Economic History Chair at LSE for three years when Sayers arrived and the two soon discovered their common interests. Sayers became something of a protégé of Ashton who protected him and when necessary would come to his defence.

8 Personal Relationships

With other colleagues at LSE, Sayers's relations were less close. He had shown at Oxford a capacity to collaborate in research into business behaviour. But temperamentally, he was a loner who preferred to get on with his work without much social activity.

This tendency was accentuated in later life by ill health: after a mishap on a platform at Leamington Spa station during the war, he suffered from a bad back which obliged him to rest for long spells. The trouble continued and he had a bed installed in his room on which he rested. Apparently, the ‘cushions’ between the discs in his spine were disintegrating and medical science had not yet found how to control this complaint. Later, however, Stuart Wilson remembers the emotional occasion when Sayers came back from seeing his doctor who had just told him that he could look forward to a normal lifespan. Less hopeful expectations may help to account for the pace at which he worked in the early 1950s, turning out a book a year.

When he returned after a summer vacation, he once complained to a colleague that he had been unable to get any work done because of back trouble. Another problem was trouble with his throat: he used to say that he could barely last an hour’s lecturing because of this. Shortly before he retired, he described a visit to a throat specialist who was unable to find the cause of the trouble but called him back as he was leaving and said: ‘Let me look at your shirt collar ... Yes, it’s much too tight’.

It was a measure of the distance he preserved from his immediate colleagues that only one of them ever visited him at his home and then only in order to drive him to a conference at Nottingham. This was in 1970 when he had already retired and was trying to sell his house. Similarly, he never brought Mrs. Sayers to LSE or spent much time in the Senior Common Room or organised any of the tea or sherry parties for students and staff which LSE encouraged. In the 1950s, all the entertainment of money and banking students—especially those from abroad—and most of the visiting academics were undertaken by Stuart Wilson and his wife. There were periods when weeks would go by, nearly a whole term, without his colleagues seeing him. One of them, himself a distinguished monetary historian, cannot recall that Sayers ever dropped into his room about anything more than once or twice in eight years. It was a cause of general astonishment and excitement when he and his wife accepted an invitation to the wedding of his Secretary, Miss Adamson, in 1959.

Due to his bad back and because also of his immersion in writing and research, Sayers’s immediate colleagues bore with the infrequency of contact with him and his light commitment to teaching in later years. But there were times when lack of contact and consultation could land colleagues in embarrassing situations out of ignorance and it never made for a healthy relationship. In other respects, he was extremely conscientious: for example, in the keeping of records and preparing annual reports on his undergraduates. He took great trouble over his graduate students—most of them

foreigners—and wrote personal letters of congratulation to all of them on graduation.

Sayers was a man of great determination and persistence. From an early age, he pursued the truth with question after question, maintaining that no one could establish it without personally examining all the evidence and rejecting hearsay. At the same time, he was shy, diffident and withdrawn. There was, too, a prickly side to his nature that grew more apparent with age and made him at times somewhat autocratic (one colleague called him ‘The Emperor’). He could be crushing to students, colleagues and even visiting speakers, defending himself on the grounds that robust argument is justifiable in academic debate—although the victims were not all academic. On the other hand, those of his colleagues who visited the USA and Australia were told of a Sayers they had never known—a charming man, good-humoured and good company, and one who left behind warm memories and deep admiration. His younger brother insists that he had a hidden personality as a loving and caring person of whom many who knew him were unaware and cites as an example his firm promise in May 1940 to his brother, then a Territorial in a front-line position on the South Coast, to look after his wife and children should he be killed in a German invasion.

He loved children and was sad that he had no grandchildren. He would brighten when any of his colleagues mentioned an expected addition to the family and welcome an opportunity to meet children brought to the School.

He was himself extremely sensitive to criticism and felt that his work did not receive all the attention it deserved. The post-Radcliffe years put a great strain on him and, in consequence, on his colleagues. He took the attacks on the Report very badly and was unable to restrain himself at lunches and seminars in encounters even with friendly City critics like Wilfred King of *The Banker* and W.F. Crick, Midland Bank’s Economic Adviser, while with others like Victor Morgan, who had severely criticised the Radcliffe Report in print, he would sit sullen and silent, as once occurred at a seminar organised by the Midland Bank.

Seminars at LSE suffered in the same way. They were oriented too exclusively to Radcliffe doctrine and contrary approaches were given short shrift. Questions would be put to students; their answers taken but not necessarily debated and there would follow an oracular pronouncement from Sayers. He became unhelpful, too, in offering comment on the work of his colleagues if it diverged from his own views.

Within LSE, Sayers did little to resist the decline of monetary history as a subject of study. This was strange given his dedication to the subject and his plea in 1960 for ‘the systematic study of recent monetary history’

and his lament for its neglect (Sayers 1960: 327). A history paper had been compulsory in Finals at LSE but when it was made optional in the 1960s Sayers offered no support for its retention. When taught MSc degrees were introduced in the 1960s, he let the old-style written MSc in monetary history go. Thus, the place of monetary history in the curriculum was gradually whittled down with the acquiescence of its principal exponent. Few good students went on to postgraduate research and those who wanted to do so might be advised to choose another university for the purpose. This was all very disheartening to Sayers's immediate colleagues.

In the rise of model-building and econometric analysis, which marked the 'modernisation' of economics at LSE (and elsewhere) in the 1960s, Sayers took no part. His interests were in the functioning of institutions, the framing of policy and the process of change. As a result, there were those who came to regard him in the 1960s as one of yesterday's men and could hardly wait to see him make way for a successor from the econometric generation.

Sayers was a great traveller in and out of term and had a wide range of contacts, particularly with bankers, all over the Continent as well as in North America and Australia. This network was of great value to him and to LSE during his tenure there. He had some knowledge of European languages, enough at least for reading purposes, but less so for speaking. Frank Paish alleged that after his [Sayers's] efforts to lecture in Spanish, his Spanish hosts decided that future speakers should be asked to lecture in their native tongues. Whatever the truth of that story, when he declared his intention to retire early at 60, it was with the intention of teaching foreign languages. As retirement neared, however, he began to have second thoughts, hoping apparently that he might be asked to stay on after all and regretting that nobody had done so. No one for that matter consulted him in any way about his eventual successor or even informed him.

Even when his academic career had ended, he was tempted to take the Cambridge Chair in Economic History. David Joslin had succeeded Michael Postan in 1965 but died suddenly of heart attacks in 1970 at the early age of 45. Sayers, then aged 62, seems to have been approached but decided not to take the appointment, writing subsequently, 'I can see now what a ghastly mistake it would have been' (private correspondence between Sayers and Cairncross).

Sayers was elected to the British Academy in 1957 and served as Vice-President in 1966–1967. He was especially proud of his FBA and liked the letters to be used after his name as evidence of his pride in that honour. In 1960, he served as President of Section F of the British Association. For a time in 1968, he was a part-time member of the Monopolies Commission.

He became closely associated with the Economic History Society and was President from 1972 to 1974. A few years earlier, he had tried to revive an editorial committee for the *Economic History Review* but without success, one of the editors indicating his unwillingness to edit under supervision.

Sayers was awarded many academic honours. The Universities of Warwick and Kent conferred honorary degrees on him and the University of Cambridge would have liked to do so too. He at first agreed but was unable, because of illness, to attend the ceremony and when approached a second time in the following year he for some reason declined the honour. He was, however, an Honorary Fellow of his old Cambridge college, St. Catherine's, and an Honorary Fellow also of LSE and of the Institute of Bankers. After the publication of *The Bank of England*, he was offered but refused a knighthood (on the curious grounds that he could not possibly give an answer within 24 hours).

Music, art and walking were his main non-academic interests. He was an avid concert-goer and after he retired he was able to indulge more fully his interest in listening to music. His letters were full of comments on the London musical scene.

For most of his life, in spite of his back trouble, Sayers was basically a healthy and vigorous man. He enjoyed long walks and even after his hips started bothering him—he suffered latterly from arthritis—he would walk for hours over rough country. According to his daughter, he would climb anything in the Alps that did not require mountaineering equipment. Towards the end of his life, however, various complaints added up and in his last few years he was almost completely immobile.

9 Conclusion

Cairncross's final conclusions were that with Sayers's death the UK had lost an authoritative guide to British banking, the author of a textbook read for a generation in successive editions all over the world and an outstanding contributor to banking history. More than anyone, he was the source of the doctrines associated with the Radcliffe Committee. His former pupils came to occupy positions of eminence in many countries and included, it is said, 19 ministers of finance (Cairncross 1991: 560–561).

Cairncross's original memoir was published in 1991; with the further passage of time, over 25 years since then, Sayers's standing and reputation as an economic historian remain undimmed. Anyone seeking to study and research UK monetary and financial developments between about 1890 and

1945 is bound to profit by reading Sayers's accounts of the British banking and financial system during this period. But his approach to current monetary policy analysis has fallen into disuse, perhaps too much so. His textbook, *Modern Banking*, once read by every undergraduate in this field, is no longer cited. Sayers's focus on liquidity rather than any particular monetary aggregate, which formed the centrepiece of the Radcliffe Report, was widely scorned during the heyday of monetarism. However, perhaps it may be regarded with somewhat greater appreciation nowadays with the development of 'shadow banking', money market mutual funds and the abandonment by many central banks, especially the US Federal Reserve, of any focus on, or major attention to, monetary quantities.

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22

Ronald H. Coase (1910–2013)

Alain Marciano

1 Introduction

Ronald H. Coase was one of the most important and also one of the most famous economists of all time. His work transformed economics by giving birth to new subfields in economics, namely transaction cost economics¹ and law and economics.² It also transformed economists' way of thinking with regard to firms and transaction costs, institutions and property rights in economic activities. It is no surprise that Coase's work is heavily cited. His 1960 article, 'The Problem of Social Cost', is reputed

¹Williamson (2006) claimed that Coase's work can be regarded as being among the very earliest on the subject of transaction cost economics.

²The Problem of Social Cost' is usually viewed as the foundation of 'new' (see Posner 1975) or 'modern' law and economics (see Hovenkamp (1990: 994); see also Manne (1993) and Stigler (1992)). Coase has been described by the American Law and Economics Association as one of the founders of the law and economics movement (the others are Guido Calabresi, Henry Manne and Richard Posner (see <http://www.amlecon.org>)).

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for being one of the most cited economics articles of all time, and thousands of pages have been devoted to his work. This has contributed to a clear but narrow image of Coase and his ideas. Despite important recent scholarship that has tried to refine this image (among other works, see Bertrand 2006, 2009, 2010; McCloskey 1998; Medema 1994, 1995a; Medema and Samuels 1997; Klaes 2000; Boettke and Candela 2014), this image has not really changed. In particular, Coase remains closely associated with the (new) Chicago School of Economics and is viewed as one of the most important defenders or promoters of free markets. This is not recent: in 1962, while Coase was *not* at Chicago, Lawrence Miller cited ‘The Federal Communications Commission’ (Coase 1959) as an ‘example’ (Miller 1962: 66, fn. 10) of pro-market Chicago economics. Old habits die hard.

However, Coase did not feel at ease with the way of envisaging economics advocated by the (new) Chicago School of Economics of Milton Friedman, George Stigler or Gary Becker and Richard Posner (Posner 1975; Medema 1994, 1995b). He explicitly referred to this unease in the late 1990s and early 2000s when he complained that Stigler’s naming of the ‘Coase theorem’ was too ‘abstract’ (Coase in Ferrarini et al. 1997) and misrepresented his [Coase’s] work. Coase had also expressed his disagreement with Chicago in the 1970s. While the claim that economics should be viewed as an ‘approach’ (Becker 1971: 4, 5) without subject matter was gaining importance and economics became increasingly applied to areas traditionally not studied by economists—in particular, to the law by Posner³—Coase wrote articles to clarify his methodological position and distinguish it from those of Becker and Posner and also of Lionel Robbins. The latter, one of the most important and influential economists of the London School of Economics (LSE), had a conception of economics that was used and pushed further by Chicagoans but one that also became dominant more widely in economics. Actually, Coase established another connection between Chicago and London. When Coase was at LSE, besides Robbins and a group of

³Coase had been a contributor to the area of law and economics since the late 1950s/early 1960s (see Coase 1959, 1960) and became the editor of the *Journal of Law and Economics* in 1964. He disagreed with Posner’s idea that economics could be used as a method to analyse the workings of the legal system. Indicative of the gap that existed between Coase and Posner, in 1972, Posner launched the *Journal of Legal Studies*, following a suggestion by Coase, this at a time when Coase was still editor of the *Journal of Law and Economics*. Both journals were hosted at the Law School of the University of Chicago and published by the University of Chicago Press. On the differences between Posner and Coase, see, among others, Harnay and Marciano (2009).

economists around him, there was also Arnold Plant and his so-called industry group. Coase belonged to the latter. It was in this group that his views on economics and on markets were born. This is precisely the connection between a certain tradition at LSE and Coase's ideas that we would like to discuss in this chapter.

After a biographical summary, we discuss Coase's conception of economics and then show how it impacts his conception of institutions and, in particular, markets.

2 Biographical Summary

Coase was born on 29 December 1910 at Willesden (a suburb in north-west London) and died on 2 September 2013 in Chicago. He had lived a long 102 years, half of which was spent in the USA—first at Buffalo and then Chicago—and the other—first—half was spent in the UK, in particular at LSE. It is there that he studied economics. More precisely, he enrolled in an undergraduate commerce degree in 1929. In his second year, Coase attended the lectures of Arnold Plant. We will say more below about how Plant's ideas influenced Coase. For now, let us say that Plant then supervised the work Coase did during his third year (1931–1932) which he spent travelling in the USA on a Sir Ernest Cassel Scholarship. Coase visited factories and carried out interviews to improve his understanding of the structure of American firms and industries. When he came back, he secured teaching positions first at the Dundee School of Economics and Commerce from 1932 to 1934, at the University of Liverpool from 1934 to 1935, and then at LSE from 1935 until 1951, with the exception of the Second World War during which Coase worked in the Central Statistical Office. In 1951, Coase earned his PhD from the University of London and was offered by Robbins the Tooke Chair at LSE. At the same time, Coase received a proposal from Ralph C. Epstein at the University of Buffalo (now the State University of New York at Buffalo). To Robbins's disappointment, Coase accepted Epstein's proposal and went back to the USA where he settled permanently.

Coase taught for a few years at Buffalo. In 1957, he gave a series of lectures examining the economics of radio, television and the press. One of the attendees was G. Warren Nutter, then Professor at the University of Virginia and Co-Director of the Thomas Jefferson Center with James Buchanan, who recounted later that: 'Nutter returned to Charlottesville mightily impressed with Coase, and he immediately commenced to examine the prospects of

prying Coase away from the University of Buffalo ... After lengthy, and sometimes tortuous negotiations, the deal was made, and after a full year's delay, Coase shifted to Charlottesville' (Buchanan 2006: 36).

Actually, before moving to Virginia, Coase went to California where he spent time during 1958–1959 at the Center for Advanced Study in Palo Alto. It is where he began writing 'The Federal Communications Commission' (Coase 1959; see also Coase in Kitch (1983: 222)). This was the paper that attracted the attention of economists at Chicago. Disagreeing with the anti-Pigouvian message of the paper, they invited him to present his views at a Chicago seminar for economists, a number of whom, Coase explained, thought '[p]art of the argument...to be erroneous' (Coase 1991a)⁴ because it was 'contravening Pigou's analysis' (Coase 1996: 810). After the seminar, Coase was invited to Aaron's Director house to explain himself and was successful in convincing 'all the big shots of Chicago' (Coase in *Reason* 1997) that his view was not 'heresy': 'In the course of two hours of argument, the vote went from 21 against and one for Coase to 21 for Coase' (Stigler 1988: 76). To Stigler, it was such an 'exhilarating event' that he 'lamented afterward that we had not had the clairvoyance to tape it' (ibid.: 76). The evening was also stimulating for Coase. Indeed, it was a turning point in his career. For one thing, 'After that seminar, Aaron [Director] said, "Would you write this up for the *Journal*," and I wrote it up in the summer of 1960' (Coase in Kitch 1983: 221). The first outcome was Coase's famous article on 'The Problem of Social Cost', and the second was a position at the Law School of the University of Chicago—where Stigler managed to hire him in 1964—and the editorship of the *Journal of Law and Economics*, a proposal made by Director. Coase spent the rest of his academic career at Chicago, retiring in 1982.

In 1991, he was awarded the Nobel Prize in Economics. But he did not stop working. Among the many things that he did one must mention the founding, under the auspices of the Coase Society, of a journal called *Man and the Economy*. Coase was over 100-years-old when this took place. The goal of the journal was to contribute to the emergence of a new paradigm in economics because the discipline had degenerated into 'blackboard economics' and moved too far away from 'the ordinary business of life'.

⁴The 'part' was the fact that the allocation of resources is independent from legal rules.

3 Coase's Methodology and His Definition of Economics

Remembering his years in London, Coase once wrote that 'What set me going was the approach I learned from Plant, and this no doubt came via Plant from his teacher, Edwin Cannan' (Coase 1988a: 20).⁵ The views of these two economists are crucial in understanding Coase's conception of economics.

Cannan, 'the chief teacher of economic theory at the LSE...before World War I' (Koot 1982: 14), was instrumental in the creation and establishment of an LSE tradition in economics and, more specifically, contributed to a revival of classical liberalism in Britain.⁶ Cannan favoured, as Coase remembered it, a 'common sense' approach in economics (Coase 1982: 33). To Cannan, economics should be 'expressed in plain language understood by the people' (Cannan 1933: 370), which was not, at least to him, the case in those years. Moreover, a common sense political economy should be theoretical and 'relevant to a particular time and place' (Koot 1982: 15). It is no surprise that the title of Cannan's Presidential Address to the British Association in 1902 was entitled 'The Practical Utility of Economic Science'. He was not against theory per se but rather against a sort of theory that would be completely 'abstract'⁷ and devoid of any connection to history and to the practical problems of society.⁸ Thus, in the Presidential Address he delivered at the 1933 annual meeting of the Royal Economic Society, the year after the publication of Robbins's *An Essay on the Nature and Significance of Economic Science* (Robbins 1932), Cannan regretted

⁵Medema and Zerbe (1997) also cite Hewins (see, in particular, his 1911 article in *Encyclopedia Britannica*) as someone whose work was close to Coase's. One of the founders of LSE in 1895, Hewins, was described by Hayek (1946: 4) as 'in revolt against "orthodox" economics'.

⁶According to Hayek, Cannan 'created the tradition which, more than anything else, determined the intellectual climate in the central department of the School' (Hayek 1946: 6). It was thanks to Cannan's influence that LSE became 'one of the very few centres of teaching in which the tradition of classical liberalism was carried on' (Hayek 1963 [1995]: 52–53). Let us note, however, that Cannan had already retired when Hayek arrived at the School.

⁷For instance, he opposed Marshall, criticising him for having 'constantly supposed that "abstract theory" must be defended at almost any cost against the attacks of the "historical school"' (Cannan in Koot 1982: 15).

⁸Among the many examples Cannan used to illustrate his claims, one finds the theory of marginal utility which, 'simple as it is...has never made much way among the general public...because...it has been treated as a classroom plaything to be illustrated by lines and curves on a blackboard, which, like the stone and wooden idols of the more degraded religions, come to be revered for themselves rather than for the things they were originally intended only to represent' (Cannan 1933: 370).

the tendency of economists to 'find peace and contentment in neat equations and elegant equilibria' (Cannan 1933: 370) and deplored 'the almost complete absorption of the younger teachers in making what they rightly or wrongly believe to be important advances in the higher branches of theory' (ibid.: 367), hoping that 'out of the large accessions to the ranks of professional economists which have taken place in recent years, a substantial force might be spared to assist common sense to grasp the bare elements of economic science' (ibid.).⁹

Among Cannan's students were both Robbins and Plant. However, as Coase noted, 'it seems to me that the influence of Cannan's common-sense approach to economic policy is more strongly felt in Plant's work' (Coase 1977: 87) compared to that of Robbins. Actually, Robbins disagreed with Cannan and Plant regarding the definition of economics (see Robbins 1929, 1932; Coats 1982). Plant, in contrast to Robbins, 'retained in his teaching Cannan's interest in institutions and his common sense approach' (Coase 1982: 33). This translated into a conviction that economics should aim at explaining the real world: Plant was, as Cannan had been, 'an applied economist' (ibid.). Indeed, what Plant taught to the economists who were around him was to treat economic theory 'seriously' (Coase 1970: 115) and as a means of applying 'ideas...[to] the real world' (ibid.). Plant taught his students to pay attention to theory but also to the real world, to the applied dimension of the problems they studied: '[A]lthough deeply interested in the theoretical advances being made, [the Plant group was] more concerned with the practical problems of their application to business and public administration' (Coase 1977: 87). Without facts, economic theorising was empty and vain. Gathering facts and data was the first task that economists should perform. It was only after having gathered facts that theory could be used to make sense of the empirical evidence with which the analysis started.

This is what Coase retained, saying that economics should be 'practical', its goal being to improve our understanding of the real world. To him, economics is 'the study of man as he is and the economic system as it actually exists' (Coase 2012). This methodological claim has two dimensions that

⁹This echoes what Robbins had said in his Inaugural Lecture, delivered in 1930: 'Ladies and gentlemen, the science which will emerge from the developments I have been indicating will not be a body of knowledge accessible to everyone. The days are gone when Political Economy was a fit subject for a gentleman to study in his moments of relaxation. It is sometimes said that one of the main duties of economists at the present day is to make plain to everyone the main doctrines of their science. This is not a view which I find possible to accept ... [I]n fact, I believe, that the hope that Economics will ever become something which the layman can comprehend without training is doomed for ever to frustration' (Robbins 1930: 23). See Coats (1982) for a more complete comparison.

are important in understanding Coase's methodology: the first relates to the domain of economics and what economists should study, and the second relates to the methods that economists should use.

Regarding the first dimension, Coase was convinced that a science has a, and should be defined by its, subject matter; this is 'the dominant factor producing the cohesive force that makes a group of scholars a recognizable profession' (Coase 1978a: 204), 'the normal binding force of a scholarly profession' (ibid.: 206), what 'distinguishes' (ibid.: 207) it from other professions. Economists are no exception. As with any other scientists, they 'do have a subject matter' (Coase 1998: 73) or, in other words, they 'study certain kinds of activities' (Coase 1978a: 206). The implication is that economists should limit their analyses and use the tools of their discipline within the limits of their subject matter. This is what Coase meant when he insisted that economists should study 'the working of the economic system, a system in which we earn and spend our incomes' (Coase 1998: 73). To be more precise, for Coase this meant that economists should study '[t]he working of the social institutions which bind together the economic system: firms, markets for goods and services, labour markets, capital markets, the banking system, international trade, and so on' (Coase 1978a: 206–207). He added that we are talking about a 'system' that is 'a complicated set of interrelationships' (Coase 1998: 73) made up of interactions and transactions and not the sum of choices made by isolated individuals. He therefore did not distinguish between studying the economic system and its institutions. It can thus be said that Coase did not only define economics by its subject matter and, complementary to this, as a science of exchange (see Boettke and Candela 2014, 2017).

By contrast, he opposed the conception of economics as a science of choice *and* as a method (without subject matter), two characteristics that Coase also viewed as indissociable (see Coase 1998: 72–73). His reasoning was that, once economics is defined as the science of choice, there is no reason to limit its scope to economic activities. Economics can then easily become 'the study of *all* purposeful human behaviour and its scope is, therefore, coterminous with all of the social sciences' (Coase 1978a: 207; italics added). Once restricted to the study of human choice, economics can be defined as a method or as an approach that can be applied to any kind of social phenomenon. Hence, the so-called economic imperialism that developed at Chicago, which Coase could easily observe but with which he disagreed. Indeed, the economists who envisage their discipline as a set of tools without subject matter—as Becker and Posner did but also John Maynard Keynes and Joan Robinson (see Coase 1998: 73)—behave as if they were studying 'the circulation of the blood without a body' (ibid.).

After having specified what, according to Coase, economists should study, the next question is *how* they should proceed to understand the *system as it actually works*. This is the second step that is necessary to characterise Coase's methodology. After all, one may well conceive economics as limited to a subject matter and as an abstract science. This was, for instance, the view held by Stigler. As he wrote to Thomas Kuhn, he was convinced that 'it is part of a theory's formulation that it have a domain' (Stigler quoted in Schliesser 2012: 163). To Stigler, the 'insularity' or 'autonomy'—he used the terms synonymously—is not only 'surely essential to [the] existence [of a science]' (Stigler 1960: 45) but also to its success and a discipline open to other disciplines 'would simply not be a discipline'. But Stigler also believed that economics—and any science—should not depend 'upon the current output of events' (*ibid.*) but should be based on abstract and general statements.

Coase disagreed with these two aspects. He criticised those approaches to economics that he called, in an echo of Cannan, 'blackboard economics' (see Coase 1988b: 19, 28, 179; 1991b) for being abstract. To him, abstraction implied 'little concern', 'disregard' or 'disdain' 'for what happens concretely in the real world' (Coase 1998: 72). Abstract theories could not but 'misrepresent the character of man and the nature of the economy' (Coase and Wang 2013).

Coase identified the misrepresentation of man in the 'theories of utility' which are abstract because they represent individuals as atomised rational utility maximisers (Coase 1978b: 244; see also Coase 2012; Coase and Wang 2013). To assume that individuals maximise a utility function allows economist to predict—with some accuracy—what *abstract* individuals will do. The prices and quantities that are computed in economic models are valid only in the abstract world that models represent. These are estimates made by an external observer that do not, and cannot, exist in the real world. This is the case because prices and quantities do not exist for the individual independently from the choice itself or, as Buchanan argued, cost and choice are interrelated.¹⁰ It is only when the choice becomes effective that individuals know the price that they are ready to pay and the quantities that they want to demand or supply. This was also Coase's view as seen in particular in a series of articles on business organisation and accounting

¹⁰Such a subjectivist approach to costs was also perfectly summarised by George Thirlby in 1946 when he wrote that, 'Cost is inevitably related to the behaviour of a person' (Thirlby 1946: 33). Thirlby was also an LSE graduate and then faculty member. He co-edited with Buchanan a book on the so-called LSE tradition in cost theory (Buchanan and Thirlby 1973). That Coase remained a 'subjectivist' after the 1930s is far from clear (see Bertrand 2015b).

(see, for example, Coase 1938; see also Bertrand 2015a), and the reason why Buchanan saw Coase as one of the representatives of the LSE's tradition on cost (Buchanan 1969; Buchanan and Thirlby 1973). As a consequence, the economic theories that assume that individuals are rational utility maximisers do 'not tell us why people choose as they do' (Coase 1988b: 5), what are 'the purposes which impel people to action' (Coase 1978b: 244) and 'for which they engage in economic activity' (Coase 1978a: 208). These theories are therefore 'sterile' (*ibid.*) because they do not tell us anything about how men actually behave and cannot teach us anything about how the system works in practice.

Similarly, firms, one of the most important institutions of economic systems, are misrepresented by standard economic analyses because they are viewed in abstract terms. Economists do not analyse them. They presuppose the existence of firms and focus on the process of production only (see Coase 1988b: 5).¹¹ Firms are then reduced to a technology of production aimed at turning inputs into outputs. They are a 'shadowy figure', Coase wrote (*ibid.*), echoing Frank Hahn (1981: 131). Coase identified similar problems with the modern treatment of the market in economics: '[I]n modern economic theory the market...has an even more shadowy role than the firm' (Coase 1988b: 7); 'discussion of the market itself has entirely disappeared' (*ibid.*) from modern textbooks because the focus is put only on the determination of price. The consequence is that these theories cannot be said to adequately describe what is happening in the real world¹² and provide any valuable insights as to why there are firms, how they function and are organised, how entrepreneurs or managers make choices, and what is the role of markets.

These theories are, as with the theories of utility, useless and can even be dangerous. They lead to erroneous normative conclusions. For instance, the

¹¹In 1978, Coase considered that economists 'have a primitive analytical system to handle the firm, the market, the process of contracting and property rights' (Coase 1978b: 244). The comparison between economics and biology which Coase made in this paper, an approach that was particularly trendy in those years among economists, was not favourable to economics. Twenty years later, the judgement was the same: 'Biologists now have a detailed understanding of the complicated structures that govern the functioning of living organisms. I believe that one day we will have similar triumphs in economics' (Coase 1998: 73).

¹²Coase was struck by one problem: economic theories viewed coordination through markets, via prices, as distinct and separate from coordination through firms. Realistic explanations of market coordination and coordination through firms, the existence of firms and their size were lacking. There was a 'gap in economic theory between the assumption (made for some purposes) that resources are allocated by means of the price mechanism and the assumption (made for other purposes) that this allocation is dependent on the entrepreneur-co-ordinator' (Coase 1937: 389). Coase found it necessary to 'bridge' (*ibid.*) that gap in order to gain realism.

economists who analysed the economic function of lighthouses started with an objective definition of a public good, in turn noting that markets can fail in the presence of public goods and what the State should do to correct these failures, all of this without considering the behaviour of individuals. In this case, we are not talking about ‘misinterpretation’ but simply of ‘ignorance’: ‘Despite the extensive use of the lighthouse example in the literature, no economist, to my knowledge, has ever made a comprehensive study of lighthouse finance and administration. The lighthouse is simply plucked out of the air to serve as an illustration ... This seems to me to be the wrong approach’ (Coase 1974: 375).

The ‘right’ approach, that would allow for the production of useful theories, consists in observing what actually happens in the economic system. This is why Coase pleaded for, and took care to anchor his work in, empirical studies and, more precisely, case studies:

I think we should try to develop generalisations which would give us guidance as to how various activities should be best organised and financed. But such generalisations are not likely to be helpful unless they are derived from studies of how such activities are *actually* carried out within different institutional frameworks (ibid.; italics added).

Coase did not do much empirical work in the form of econometric studies (exceptions are his works with his LSE colleague, R.F. Fowler, on the pig cycle (Coase and Fowler 1935a, b, 1937)). Significantly, his famous article, ‘The Nature of the Firm’ (Coase 1937), grew out of interviews with managers he made in the USA in 1931. That was for him the only possible way to improve his understanding of how firms work and how industries are structured. After those interviews, Coase could say that he had clearer ideas about markets, firms and coordination. The same kind of method can be found in his analyses of public utilities which did not consist of abstract models about public firms but were grounded in observations of how the postal service or the BBC (Coase 1947a, 1948, 1950) actually work. Interestingly, Coase’s second famous article, ‘The Problem of Social Cost’ (Coase 1960), was entirely based on case studies. It was, as Mishan put it, ‘a learned paper, replete with case law’ (Mishan 1965: 29). Of course, this is also the approach Coase followed in his analysis of lighthouses (Coase 1974), in which he started from empirical evidence rather than with abstract and a priori categories.

This does not, however, mean that Coase defended an approach based on a spineless, atheoretical, collection of facts and data in the form of a ‘naïve’ empiricism that has frequently been used by institutionalists (see Boettke and Coyne 2005). On the contrary, he insisted that facts without a theory

would be useless. Indeed, this was his main criticism against institutionalists: ‘John R. Commons, Wesley Mitchell, and those associated with them were men of great intellectual stature, but they were anti-theoretical, and without a theory to bind together their collection of facts, they had very little that they were able to pass on’ (Coase 1998: 72; on Coase and institutionalism, see Medema 1996). Thus, from this perspective, one may note that ‘The Nature of the Firm’, although based on empirical evidence, is not an empirical paper. In ‘The Problem of Social Cost’, Coase used what we have called a ‘toy model’—that was his theoretical framework—‘in complement with the legal case and, reciprocally and complementarily, the legal cases were means to illustrate and clarify the result reached through the numerical example’ (Frischmann and Marciano 2015: 330). Such a back-and-forth movement between facts and theory is typical of Coase’s method, albeit a method necessary ‘to improve our analysis of the working of the economic system’ (Coase 1993: 250). As Bertrand (2008) has noted, Coase used facts to establish theories. Let us see now how this affected his analysis of markets and firms.

4 Institutions: Markets, Firms and the State

Coase was not the pro-market economist that a close association between his name and work and a theorem—named for him by Stigler (1966: 113)—seem to imply. He was balanced with regard to the respective roles of the market and the State and, more broadly, to the roles of all possible institutions. Indeed, he ‘was essentially studying and trying to explain the boundary between “the market” and “the non-market”’ (Veljanovski 2015: 5). Or, as Coase put it himself: ‘I am arguing for sensible government action. I am arguing for a properly functioning market. These aims are not inconsistent’ (Coase 1965: 167). It nevertheless remains that the case that Coase believed that the market should be used, and favoured over any intervention by the State, whenever it was possible, even in cases which economists usually view as incompatible with the market, such as increasing returns, public goods or harmful effects. This thesis characterises *all* of his work. It was not only present in Coase’s later writings, such as the ‘The Federal Communications Commission’ (1959), ‘The Problem of Social Cost’ (1960), ‘The British Post Office and the Messenger Companies’ (1961), ‘The Interdepartment Radio Advisory Committee’ (1962), ‘Evaluation of Public Policy Relating to Radio and Television Broadcasting: Social and Economic Issues’ (1965), ‘The Lighthouse in Economics’ (1974) and many other works; one also finds evidence of a defence of markets and the pricing system in his earlier

articles, such as ‘The Nature of the Firm’ (1937), ‘The Marginal Cost’ (1946) and of course his work on the BBC and the British postal service. Coase thus did not become a liberal when he moved to the University of Virginia and met Buchanan, Nutter and Tullock. His confidence in the market and the price system predates this. Indeed, it can be traced back to the years he spent at LSE.

For sure, at LSE, Coase made the acquaintance of Abba Lerner, Nicholas Kaldor, Brinley Thomas and Evan Durbin, all economists who, as Coase put it, held ‘very different views’ (Coase 1982: 34) than Robbins, Hayek and Plant about private enterprise (ibid.). They were indeed favourable to the regulation of the economy by the State and played, according to Coase, an important role in the ‘transfer of allegiance from Hayek to Keynes’ (Coase 1982: 29) at LSE.¹³ But even if ‘[t]he intellectual atmosphere [at LSE] was extremely agreeable’ (Coase 1982: 34) and ‘differing political views did not impede economic discussion’, it was rather the teaching of Robbins, Hayek and Plant who influenced Coase, the effect being ‘to make students look to private enterprise for solutions to economic problems’ (ibid.). Plant played the major role in convincing Coase of the virtues of the market. Attending Plant’s seminar was to Coase a ‘revelation’ (Coase 1991b: 715), radically changing his views on the working of the economic system: ‘Before being exposed to Plant’s teaching, my notions on how the economy worked were extremely woolly. After Plant’s seminar, I had a coherent view of the economic system’. In particular, Coase found coherence around the notions of the invisible hand and competition. Plant had ‘introduced me to Adam Smith’s “invisible hand”...[and]...explained how a competitive economic system coordinated by prices would lead to the production of goods and services which *consumers* valued most highly’ (ibid.; italics added), which can be interpreted as meaning that Plant taught him why a *competitive system coordinated by prices* should be preferred to other forms of the organisation of production.¹⁴

¹³Bob Coase cited Lerner and Kaldor but added also Tibor Scitovsky, Harold Barger and Michał Kalecki whom Coase met when he was at LSE.

¹⁴Thus, from this perspective, consumers’ sovereignty is the normative criterion to use to judge a *competitive system coordinated by prices*. Plant insisted on the importance of consumers in the economy: ‘[T]he demand of ultimate consumers...is...the controlling power which ultimately determines the nature and volume of all production’ (Plant 1932: 46), and ‘[t]he controlling employer in the productive system is the community of consumers’ (ibid.: 52). This echoed William H. Hutt’s defence of consumers’ sovereignty—a concept that he invented (see Hutt 1936, 1940). Hutt graduated from LSE, where he was influenced by Cannan and Plant. In 1928, he became Lecturer at the University of Cape Town where Plant also held a position, moving back to LSE in 1930. For his part, Plant did not speak of consumers’ sovereignty but of the ‘freedom of consumers’ choice, as the condition of individual liberty in its *full sense*’ (Plant ibid.: 53; italics added).

Coase was more explicit in his research. In particular, in his early work, perhaps when Plant's influence was still significant, he included references to the important role of consumers, explaining that the pricing system is 'one in which individual consumers have command over various sums of money which they use to obtain goods and services by spending this money in accordance with a system of prices' (Coase 1946: 171). Then, he justified the system by arguing that it is the most efficient way to know what individuals, consumers and producers want to pay for obtaining a good. Thus, he wrote that a pricing system is 'a most useful guide to what consumers' preferences really are' (ibid.: 172). More broadly, as he wrote in the case of radio frequencies: '[I]t is one of the advantages of the pricing system that, for its efficient working, the only person who needs to know about how any given user would use radio frequencies is the user himself' (Coase 1962: 43). This therefore guarantees 'that resources are obtained by those who will pay the most for them' (ibid.: 40). By contrast, information about an individual's willingness to pay is very difficult to obtain in a centralised way that is by the State:

[A]n administrative agency...cannot, by the nature of things, be in possession of all relevant information possessed by the managers of every business which uses or might use radio frequencies, to say nothing of the preferences of consumers for the various goods and services in the production of which radio frequencies could be used (Coase 1959: 18).

Indeed, the arguments for the adoption of the pricing system 'derive their main force from the view that such estimates of individual demand by a Government would be very inaccurate' (Coase 1946: 175).

Coase defended the pricing system, even though he admitted that such a system is costly. Actually, Coase had realised quite early on that 'there are costs of using the pricing mechanism' (Coase 1991b). According to his own recollections, this realisation took place in the summer of 1932 that is when he was working on 'The Nature of the Firm'. Moreover, throughout the years, Coase never failed to insist that all the operations that are necessary 'to carry out a market transaction...are often extremely costly' (Coase 1960: 15). More precisely, all transactions in an economy are plagued by so-called *transaction costs*. This concept became a sort of motto for Coase. For him, 'it is impossible to understand the working of the economic system...[w]ithout the concept of transaction costs' (Coase 1988b: 6). Thus, he insisted that if we want to study and understand 'the world that exists', there is no better alternative than 'to introduce positive transaction costs explicitly into economic analysis' (ibid.: 15).

This might seem surprising, or indeed an ambiguity, since Coase is frequently cited or remembered and criticised for having argued that markets are always efficient when there are no transaction costs. For example, he began ‘The Problem of Social Cost’ by examining the ‘case...when...the pricing system works smoothly (strictly this means that the operation of a pricing system is without cost)’ (Coase 1960: 2). However, to reduce Coase’s analysis to just this case—and to the Coase theorem—would be doubly misleading. It gives the wrong impression, first, that Coase was interested only in explaining how markets work under the specific assumption of zero transaction costs and, second, that, to him, the economy could either be based on a pricing system (when there are no transaction costs) or on other forms of institutions (when there are transaction costs). But that does not correspond to Coase’s view. Indeed, even Coase agreed with the assumption of zero transaction costs as being somewhat ‘heroic’ (McKean 1970: 31) or, at least, he dismissed it as unrealistic: ‘[T]he assumption...that there [are] no costs involved in carrying out market transactions is, of course, a very unrealistic [one]’ (Coase *ibid.*: 15). As such, although the insights we get from the study ‘of what would happen in a world of zero transaction costs’ (Coase 1981: 187) may be ‘valuable’, they are ‘without value except as steps on the way to the analysis of the real world of positive transaction costs’. As a consequence, ‘[w]e do not do well to devote ourselves to a detailed study of the world of zero transaction costs’ (*ibid.*) or ‘[i]t would not seem worthwhile to spend much time investigating the properties of such a world’ (Coase 1988b: 15).¹⁵ It could be interesting but only as ‘a preliminary’ that would precede the ‘development of an analytical system capable of tackling the problems posed by the real world of positive transaction costs’ (*ibid.*), which was the actual objective pursued by Coase. To be more precise, Coase was primarily interested in understanding how various institutions—including the market—perform when there are positive transaction costs, which happens all the time; he wanted to know whether or not one can rely on the pricing system in cases where there are transaction costs and, accordingly, if other institutional arrangements are required to complement or replace the market.

To understand Coase’s solution to this problem, one must not forget that he believed that *all* institutional arrangements—the pricing system

¹⁵This claim would be trivial, a truism indeed, for when there are no transaction costs; the pricing system works smoothly; and, one could add, all exploitable gains from trade are exploited. Coase even wrote that, ‘In an economic theory which assumes that transaction costs are nonexistent, markets have no function to perform’ (Coase 1988b: 7). More generally, ‘the institutions which make up the economic system have neither substance nor purpose’ (*ibid.*: 14).

but also firms and the government—are costly. Certainly, ‘a pricing system puts additional marketing costs on to consumers and firms’ (Coase 1946: 172), but one should not therefore condemn the market because there are always transaction costs. Or, to put it in other words, one should not justify the intervention of governments on the basis of the failure of markets. We should not forget, claimed Coase, that the ‘governmental administrative machine is not itself costless. It can, in fact, on occasion be extremely costly’ (Coase 1960: 18). Therefore, the costs of the pricing system ‘may in fact be less than the organising costs which would otherwise have to be incurred by the Government’ (Coase 1946: 172). If this were the case, one should prefer the market over the government. In other words, since all institutions are costly, the goal cannot be to adopt a system that would be costless. We should only choose the institutions for which the benefits are greater than the costs. This idea is long-lived in Coase’s work. It was already present in his discussion of marginal cost pricing in 1946, where he argued that it was only by comparing ‘the results of adopting the Hotelling-Lerner solution with those of using multi-part pricing’ (ibid.: 174) that it could be decided which solution should be adopted (see Bertrand and Marciano 2015; Frischmann and Hogendorn 2015). In 1960, he wrote again: ‘When an economist is comparing alternative social arrangements, the proper procedure is to compare the total social product yielded by these different arrangements’ (Coase 1960: 34). Coase was indeed one of the first economists, along with James Buchanan and Roland McKean, to stress the need for a ‘comparative institutional analysis’ to determine which institutions should be used to organise the economic system.¹⁶

Of course, as explained in the preceding section, such a comparison between the respective costs and benefits of using different institutions should not be made theoretically, that is by using pure or abstract theories. It is only by using case studies and by making comparisons that one can decide which institutions recommend themselves:

It is my belief that economists, and policy-makers generally, have tended to over-estimate the advantages which come from governmental regulation. But this belief, even if justified, does not do more than suggest that government regulation should be curtailed. It does not tell us where the boundary line

¹⁶It is accepted that political economy should be comparative. No institution can be said to be better than another in absolute terms. Each institution has benefits and costs, and it is only by comparing these benefits and costs that one may decide which one should be chosen (see Boettke et al. 2013; Ramello 2015).

should be drawn. This, it seems to me, has to come from a detailed investigation of the *actual* results of handling the problem in different ways (ibid.: 18–19; italics added).

Also, one may add, Coase used facts because they provide evidence of what individuals do.

From this perspective, the main lesson that facts tell us is that individuals do not behave as economists predict they would. In particular, they do not behave opportunistically—or, at least, as opportunistically—as economists claim. Individuals do cooperate where they are supposed—if one follows the assumption used in mainstream economics—to free ride. Or, in other words, facts teach us that individuals find private solutions to solve their problems and deal with transaction costs and, as a consequence, ‘the economic system “works itself”’ (Coase 1937: 387; see also Coase 1988c: 17 and 1991b).¹⁷ Thus, public goods are privately produced where they should not have been produced because individuals free ride. Coase studied the lighthouses that were actually provided privately in England in the nineteenth century and explained that economists had missed this occurrence because they were trapped by their abstract assumptions and models.¹⁸ Similarly, Coase admitted that individuals could deal with ‘harmful effects’ by rearranging legal rights through the market, even when there are transaction costs. Moreover, if there are transaction costs, the next solution he envisaged consists in creating firms: in certain conditions, ‘it would be hardly surprising if the emergence of a firm or the extension of the activities of an existing firm was not the solution adopted on many occasions to deal with the problem of harmful effects’ (Coase 1960: 17).¹⁹ Even if firms are not decentralised mechanisms and involve planning, they nonetheless represent a private mechanism of coordination. As such, even when the market is too costly and if there are harmful effects which limit its efficiency, the intervention of the State is far from being a necessity. It is a solution to deal with harmful effects or with public goods which should be considered only after all private solutions have failed.

¹⁷This quotation comes from Sir Arthur Salter whom, as Coase recounted, Plant quoted during his seminar (see Coase 1991b) and who he also quoted in ‘Trends in Business Administration’, Plant’s Inaugural Lecture at LSE (Plant 1932: 51). Coase also noted that Dennis Robertson used the same quotation in *The Control of Industry* (see Coase 1937: 387, fn. 3).

¹⁸Coase may have been a little optimistic regarding the benefits provided by privately maintained lighthouses (see Bertrand 2008, 2016).

¹⁹This example is slightly different. As with Coase’s work on radio broadcasting and the BBC, it does not really rest on an empirical demonstration that firms do find solutions to deal with externalities.

More broadly, the existence of firms provides further evidence that individuals do cooperate with each other. They are able to find alternative forms of economic organisation when markets are too costly; that was the message of Coase's first path-breaking article on 'The Nature of the Firm'. Individuals are also able to cooperate across firms. This is what Coase demonstrated when he discussed the acquisition of Fisher Body by General Motors (GM) and criticised Benjamin Klein's account of what happened in this particular case (Coase 1988c, 2000). In a nutshell, Klein claimed that GM, which was being 'held up' by Fisher Body, wanted to end a commercially difficult situation by acquiring its supplier of automobile bodies. This thesis was justified by a theoretical analysis, essentially based on the assumption that managers adopt opportunistic behaviours. To put it in other words, vertical integration—and the existence of firms—was a consequence of a desire to avoid such opportunism. In fact, Coase explained (Coase 1937, 2000) that the facts did not corroborate this thesis. Or, put differently, Klein 'misinterpret[ed] the evidence' (Coase 2006: 275). The long-term contract that existed between Fisher Body and GM was respected by both parties. It is only when they realised that it was too costly to carry on as they were that the acquisition took place. In other words, once again, opportunism—like free riding—should not necessarily be postulated as being present and normative conclusions should not be derived from an abstract and unreal assumption about human nature. On the contrary, observing what individuals do in the real world would have revealed that they do in fact cooperate. Or, a non-prejudiced approach of the interactions between these two firms would have provided evidence that there was no opportunistic behaviour.

5 Conclusion

Coase's conviction that the market pricing system is the best way to organise economic activities, a view which he learned from his classes with Plant and from discussions with Robbins and Hayek, was not only grounded in economic theory. To a certain extent, it could be said that economic theory—abstract as it can be—does not make as much room for markets and private solutions as individuals themselves do. Economic theory predicts that individuals should not cooperate or should free ride and behave opportunistically. Facts tell us otherwise. We should learn from them and use theory to generalise what facts tell us rather than trying to interpret facts by using a priori and abstract categories. This is the main lesson that Ronald Coase taught us, a lesson that is rooted in the LSE of Cannan and Plant.

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23

A.W.H. Phillips (1914–1975)

James Forder

1 Introduction

Just in terms of name recognition, A.W.H. ‘Bill’ Phillips must be one of the best-known economists of any time. But as Laidler (2002: 223) also noted, his fame comes from entirely erroneous ideas about his role and that of ‘the Phillips curve’ in the promotion of inflationist policy in the 1960s and 1970s. Indeed, there is no merit at all in those commonly told stories. However, there is more to it than that since in the 1990s and after, Phillips became the subject of other stories, just as erroneous, although this time in one way or another to his credit, and the disparagement previously heaped on him was pursued by an equally untoward adulation.

So, myths lie upon myths, and the problem of sorting fact from error, either of those from speculation, speculation from the fruits of wishful thinking and all those from what may be simple invention has become a considerable task. In writing about Phillips, then, there is an obstacle course to be traversed and close attention to the basis of others’ claims is essential. That is an inconvenience, but to some extent an interesting picture unfolds of Phillips’s life, and afterlife in fact, and in the imaginings of his detractors and certain of his admirers.

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2 A.W.H. Phillips, 1914–1975

The first account of Phillips's life of any substance at all was Blyth (1975), who recorded in about 2000 words what Phillips told him, intending it for a *Festschrift*, but when Phillips died, adding a few lines about the end of his life and publishing it as an obituary. It was then reprinted in Bergstrom et al. (1978)—the *Festschrift* transformed into a memorial volume. Blyth's account seems to be based just on Phillips's memory and is certainly thin on detail, but supplementing it with information from Phillips's 'Student' and 'Staff' Files from the BLPES Archives, and other sources, as indicated, an outline of Phillips's life can be presented.

He was born in rural New Zealand in November 1914, left school at 15, worked until 1935 as an apprentice electrician and then travelled widely in Australia, Asia and Europe, while taking various jobs and continuing his education by correspondence. He arrived in London in 1937, passed examinations for the Institute of Electrical Engineers and the Institute of Linguists, and enrolled in courses at the Regent Street Polytechnic and in 1939 at LSE, in the latter case, to study for a BSc in economics,¹ before volunteering for the Royal Air Force in 1940. He was appointed MBE for his work in Singapore and his bravery during its evacuation but was then captured after the surrender of Java in March 1942 and spent the rest of the war as a Japanese prisoner. He opted to be demobilised in the UK and continued his studies at LSE where he earned a Pass Degree in 1949.² During his undergraduate studies, Phillips had the idea of building a hydromechanical machine to illustrate principles of macroeconomics. The result, and the account of it that became Phillips (1950), so impressed James Meade in particular, and also Lionel Robbins that, as Barr (1988) described, it earned him his first appointment at the School. He was then also supervised by Meade and James Durbin for his PhD, which was completed in 1953 and awarded the Hutchinson Medal.

In a very rapid ascent, he became a Lecturer in 1951, Reader in 1954 and Tooke Professor in 1958. He was a member of Maurice Kendall's 'Research

¹Consequently, the speculations in Leeson (1994: 611) about Phillips's war experience being the source of his interest in social science can be discarded.

²Bollard (2016) refers to it variously as a 'C-pass' (ibid.: vi), a 'bare pass in sociology' (ibid.: 105 and 112) 'a third class pass in sociology' (ibid.: 139 and 155), a 'third class examination pass' (ibid.: 102). A Third Class Degree is an Honours degree, whereas a Pass is not. The idea that Phillips's degree was in sociology is also found in Lancaster (1979) and Leeson (1994). As his Student File shows, Phillips took a Pass Degree in economics. Only his 'Special subject' was sociology.

Techniques Division’, took a sabbatical in Australia in 1959–1960, discussed at length by Cornish and Millmow (2016) and another at MIT in 1961–1962, recalled by Solow (1979), and was elected a Fellow of the Econometric Society in 1964, becoming their Walras-Bowley Lecturer for 1966 (see *The Econometric Society* 1965, 1967: 90). At LSE Phillips played an important role in the development of the teaching of econometrics and in the appointment of Denis Sargan, just as the School became a pre-eminent centre of that activity.³ He left LSE in 1967 and moved to the Australian National University with the intention of working on the Chinese economy, but suffered a stroke in 1969, and retiring, moved to Auckland where he taught a little at the University and died from a second stroke in March 1975.

In his lifetime, Phillips published eleven papers and a handful of book reviews. Blyth listed ten of the papers and Phillips (1959a)—a study of the relationship of wage change and unemployment in Australia, which had appeared only as a ‘monograph’ of the Economic Society of Australia and New Zealand (something like a working paper). The surviving part of the Walras-Bowley Lecture was published for the first time as Phillips (1978) in the memorial volume. These papers, Phillips and Quenouille (1960), and two of the book reviews (omitting Phillips 1952a, b) were then republished in Leeson (2000) along with the operator’s manual for the Phillips Machine (as an appendix to Vines (2000)), some work Phillips left unpublished, much of it incomplete, and some commentaries on his life and work. Then, there was another posthumous publication, Phillips (2011), described by the volume editors as an early draft of Phillips (1950) but actually quite different from it and, in the BLPES Archives, a few other bits and pieces which remain unpublished.

3 The Story of the Radio in the Prison Camp

As an example of the kind of embellishment of Phillips’s life which has taken hold, tales about his time as a prisoner of war serve well. Leeson (1994) presented the finding that Phillips operated a secret radio as a discovery following his identification of him with a ‘New Zealand officer’ whose radio operation was depicted by van der Post (1970). Making a great deal of van

³This is described by Gilbert (1989) who also said that, ‘The rapid expansion of econometrics at the LSE in the early sixties, and its introduction into the teaching of economics degrees, was due to Phillips more than to any other individual’ (ibid.: 127–128).

der Post's story of the prisoners' radio malfunctioning and being repaired with parts burgled from the Japanese camp commander's office (ibid.: 101–107), Leeson quoted van der Post's account of the first broadcast the New Zealand officer heard, saying he had,

some trouble making contact but after a great deal of fiddling had picked up a news broadcast from Delhi. Unfortunately, he had not come in right at the beginning, but near enough it to realize that something tremendous had happened. He wasn't quite certain what precisely it was, but in the course of the morning of the day which was now ended, something more like an act of God than of man had been inflicted on Japan at a place called Hiroshima (van der Post 1970: 119; quoted in Leeson 1994: 607).

This story was plainly treated as a factual account by Leeson, as it was by Sleeman (2010), discussing Phillips's wartime experiences, and by Bollard (2016), modified to say the announcer 'was talking excitedly about an act of God that had fallen upon a city in Japan called Hiroshima' (ibid.: 73).

Much more caution is appropriate. Reviewing van der Post's book, Hare (1971: 11) observed that it was written in 'a kind of prose-poetry' which 'invites scepticism' as to the reported facts. Indeed, although this passage is relatively restrained, the intention to create dramatic effect is plain. Apart from that, scepticism arises from the repair being completed just in time for the first transmission received to be about the bomb, and since, according to Bollard (ibid.: 38) Phillips had actually been arrested in Hiroshima during his travels, and Jones (2002: Chapter 11) said van der Post had visited Japan as a journalist, neither would naturally have created the allusion to mystery suggested by the last four quoted words.

More than that, though, it is clear that the reader is intended to visualise a bewildered broadcaster in Delhi describing a confused report from Hiroshima. It is inconceivable that there could have been such a report within hours of the event—there were, it is safe to assume, no Indian journalists observing the explosion. On the other hand, there was a Presidential Statement by Truman explaining what had been done. That was reported by *The New York Times* of 6 August and would have been the source of news in Delhi too.

Then, there is the point that in the Prologue to the book, van der Post said it began as unscripted remarks on a television programme when he was present to talk about something else. He made them in response to another participant's comments about the use of the bomb and said that the book was his 'orchestration' of those remarks and, 'it reflects much more accurately the spirit of our brief confrontation, in that strange subliminal light of a

transitory television set-up, that it is in a living sense far more truthful than a literal transcript of that occasion could be' (van der Post 1970: 32). Do biographers really need more warning that the details of what are said about the activities of a 'New Zealand officer', supposedly on the very night of the Hiroshima bomb, are not to be taken as factual reports of anyone's activities?

In any case, van der Post was a habitual liar, a charlatan and an egomaniac. Although doubts about the honesty of his accounts of his own wartime heroism were raised by World War II Investigator (1988), the position became fully apparent with the publication of Jones (2001). It is a remarkable work not only for being destructive of van der Post's reputation in the kind of way Aldington (1955) sought to be of T.E. Lawrence's, but also for being authorised by his family. This, however, seems to have made rather little difference to those treating van der Post as a reliable source for information about Phillips. Sleeman (2010: 416, fn. 5) described Jones's book as 'most revealing' about its subject, but although he quietly corrected various pieces of misinformation in other discussions of Phillips, he then cited Jones only for supplementary information or to offer confirmation of certain details about what van der Post wrote—never to question it. Bollard (2016: 239, fn. 7) said that he relied considerably on van der Post 'but note that' he was 'an unreliable witness', and he also cited Jones for the view that 'all van der Post's stories need to be treated with caution' (ibid.: 240, fn. 15). That is not an overstatement since Jones's own way of putting it was 'Laurens told stories—inventions—lies—*constantly* throughout his long life' (Jones 2001: 445; italics in original), also noting that his 'institutive predilection for the truth of the imagination over literal facts and events would never be better seen than in his account of his experiences during the Second World War' (ibid.: 5). Bollard himself hardly showed the caution he recommended since the only approach to a critical assessment of van der Post's book is the suggestion that his egotism explains why he did not name the New Zealand officer. That only emphasises the importance Bollard attached to the story, as a factual one, about Phillips. Bollard did, though, say that Phillips regarded van der Post as 'something of a pseud' (Bollard 2016: 69), and that on reading Jones, Phillips came to the conclusion that van der Post was 'a habitual liar on a grand scale' (ibid.: 69). That is obviously a mistake, and a very peculiar one, since Phillips was long dead before Jones's book was published. But even coming that close to realising that van der Post is not a proper source did not lead Bollard to doubt that his book gives a worthwhile account of Phillips's exploits.

Leeson (1994), of course, was written before Jones's book. He might have noted the implausibility of van der Post's story for himself, but instead, he claimed that van der Post's story 'is oriented around a secretly built and

operated radio' (ibid.: 606). It is not. It is orientated around van der Post's person, his insight and his leadership—it is thus with all van der Post's writing. In so far as it has a theme beyond that, it is the question of the morality of the use of the bomb. Concerning the burglary, Leeson said 'Phillips and van der Post had obtained the parts for the radio by breaking into the camp commander's office' (ibid.: 608). In van der Post's account, the burglar was someone called 'Donaldson'—unmentioned by Leeson—and the New Zealand officer merely kept watch (ibid.: 104).

Beyond all this, however, if the enquiry were really into Phillips's biography, these accounts would all contain mention of Blyth (1975: 305), on whom the authors generally rely without question. He said the prisoners first heard of Hiroshima 'from the Japanese'. Evidently, though, that sort of thing does not add to the drama of the narrative, so van der Post's obviously fictional story is the one to be used.

All that might seem to raise a question as to whether Phillips operated a radio at all. Surely he did. Radios in prisoner of war camps seem to have been more common than might be guessed,⁴ but certainly building them is not technically difficult. It is just the sort of hobby mechanical- or technical-minded boys of the 1920s and 1930s pursued and numerous manuals were produced. *The Scout* magazine of 17 June 1922, for example, included such instructions. In Phillips's case, there is other evidence since his operating of a radio was reported in his obituary in *The New Zealand Herald* 'Weekend Magazine' of 22 March 1975. One does not, therefore, need to rely on serendipitous readings of van der Post for that information. But even supposing, as seems entirely possible, that Phillips was the model for the New Zealand officer, there is no basis at all for supposing everything van der Post wrote about that character is true of Phillips. It is as if, finding a box of bones under a grave marked 'Arthur', one would be entitled to infer that there once was a boy who won a kingdom by drawing a sword from a stone.

4 The Phillips Machine

The 'Phillips Machine' was a water-filled, hydromechanical device, seven-feet tall, five wide and three deep, first designed and built by Phillips and his friend the economist Walter Newlyn. The flow of coloured water around it,

⁴The operation of such radios has become well known since the film version of Lomax (1995) was released in 2013; other such accounts are easily located.

channelled in various directions and into various tanks, illustrated macroeconomic relationships, such as the determination of national income as the sum of consumption, investment and government expenditure. By adjusting various valves, the operator could, for example, ‘increase taxes’ or ‘reduce the propensity to consume’, and the changed flows of water would indicate the effects. Connected to chart-plotting apparatus, it could give quantitative results and so was an analogue computer capable, in effect, of approximately solving certain differential equations that would have been prodigiously difficult for a human.

Although the use of mechanical models to illustrate economic ideas was certainly not new, it does have enough standing, as explained by Swade (2000), to earn its place in the Science Museum in London. In economics, it has been treated by Morgan (2012), as an exemplar of one way of thinking in terms of ‘models’, and by Vines (2000), as an inspiration for further discovery; according to Lipsey (1972: 29, fn. 36), it frequently featured in textbooks. Perhaps it did in others, though it is notably not in his own—Lipsey (1963). It also featured fairly prominently in popular culture, notably being the inspiration of a humorous piece in the British weekly magazine, *Punch*, on 15 April 1953.

There is no doubt that all this speaks to Phillips’s cleverness and ingenuity. As a teaching device, the Machine also clearly had appeal in illustrating the working through of Keynesian national income accounting. It also offered a way of showing the simultaneous achievement of stock and flow equilibrium. The interest rate, for example, was directly determined by the level of water in a particular tank. That water represented ‘idle money balances’, and the size of the tank, and hence the depth of a given volume of water, could be varied to reflect changes in liquidity preference, or the ‘money supply’ could be changed by allowing more water into the system as a whole. The interest rate, so determined, set valves determining both inflow to that tank (‘saving’) and outflow from it (‘investment’). Thus, in this arrangement, at any moment ‘the interest rate’ was determined by the level of water in the tank, but that was also one of the determinants of ‘saving’ and ‘investment’ so that equilibrium was only achieved when they were equal.

Beyond these things, though, and despite much more being claimed, it is really not clear there is much to be said. It sometimes seems to be implied that the Machine was a device for conducting research, but it really was not. Clever as it was, it was much less than a full representation of the economy, so the results, although quantifiable, were not of great importance. In any case, Newlyn (1950), publishing the first photograph of the Machine in an academic journal (*contra* Leeson 1994: 605), then Phillips (1950), made it perfectly clear that it was merely a teaching device.

Even as a teaching device, appealing as it was, it does not seem to have been very successful. As well as its appeal, it had plenty of publicity too—not just in *Punch*, but in various newspaper reports noted by Morgan (2012) and also across two pages in the March 1952 issue of the American monthly, *Fortune* (pp. 100–101), complete with a colour picture and a price tag of \$4300. Following an initial appearance at the Robbins Seminar at LSE, Howson (2011: 716) says it was also demonstrated to the Council of the Royal Economic Society and the LSE Governors, then Morgan (2012: 176) had it going to the Liverpool meeting of the Association of University Teachers of Economics. Leeson (1995: 236) said it was demonstrated at the 1950 American Economic Association (AEA) meeting at Roosevelt College, Chicago, and at the AUTE and Chicago events, '[t]he brilliance and enchanting quality of the Phillips Machine lent a special aura to any economic relationship prefixed by the name Phillips'. That seems to have nothing to support it. Of the demonstration at Chicago, Leeson (1994: 613) claimed that the Machine 'caused a sensation'. In fact, that demonstration, by Abba Lerner, was not at the AEA, and Dorrance (2000: 117) said that it had 'generally sceptical audiences', with only Andrew Court (of General Motors) and Jacques Polak (of the IMF) being enthusiastic about it. Neither of those organisations seems to have bought one.⁵

As to where it was sold, authors tend to follow Blyth (1975) who listed the Universities of Oxford, Cambridge, Birmingham, Manchester, Melbourne, Roosevelt College, Chicago, the Central Bank of Guatemala and the Ford Motor Company. Barr (1988) added Harvard without explanation. Leeds—the home of Newlyn—also had one, although Phillips apparently forgot to list it. Stern (1992), however, investigated the matter, suggesting about 14 were sold, but did not list Oxford.⁶ He did list Harvard, but only to say that he 'believed' one was sold there. On this, though, Bollard (2016: 122) suggested otherwise, saying that Hayek enquired of Machlup whether they were interested. He was told they were not, even though the price quoted, if Bollard's report is correct, was just \$400. It seems likely, then, that both Oxford and Harvard have been erroneously added to the list. Moreover, if, as Bollard (*ibid.*: 122) said, Phillips had hoped to sell 1000, the outcome was clearly disappointing.

⁵The programme of the meeting—American Economic Association (1950)—lists no such event, and none of Lerner, Court and Polak is listed as attending the Conference. Probably, it was demonstrated at Roosevelt College *during* the Conference.

⁶My own attempts to locate any sign of either such a machine, or anyone who knew of one, failed, and none is mentioned in Young and Lee (1993).

Beyond its hoped-for role as a teaching device, and its alleged use as a tool for research, it has also been claimed that the Machine resolved the ‘loanable funds’ debate. That concerned the question of whether, as Keynes (1936 [1973]) maintained, the interest rate was determined by the quantity of money and liquidity preference or whether, along the lines of pre-Keynesian theory, as Robertson (1940) argued, it equilibrated saving and investment.

Barr (1988: 329) and Morgan (2012: 209), supposing that the Machine resolved this question, quoted Robbins (1972) saying that ‘Keynes and Robertson need never have quarrelled if they had had the Phillips machine before them’. Bollard (2016: 114–115) also said that ‘Denis [sic] Robertson...had made the most penetrating critique of Keynes’ General Theory’ and though the Machine could not prove one of the theories right and the other wrong, it showed Keynes’s and Robertson’s ‘interpretations were special cases of a more generalized framework’. Bollard gave no indication of his reason for these claims, though a few pages earlier, without giving a source,⁷ he had quoted Robbins as saying that with the Machine, ‘problems of the relationship between savings, investment, the supply of money, and the speculative demand for liquidity which had been the subject of extensive debate for the preceding decade, resolved themselves almost automatically’.

It is difficult to see how that could be correct. Keynes (1936 [1973]: 170) defined the speculative demand for money as that arising from ‘the object of securing profit from knowing better than the market what the future will bring forth’. The Machine surely does not show the relationship between that and, say, the supply of money.

These authors might have considered the discussion of the question in Phillips (1950: 299–301). He explained how the Machine demonstrated the simultaneous achievement of stock and flow equilibrium and implied that that was all there was to the loanable funds debate. Even he, though—like these later authors—gave no worthwhile account of the substance of the debate, so it seems the matter must rest entirely on Robbins’s authority. That authority is not to be disparaged, but there are substantial reasons to doubt the Machine solved this problem. For one, there is a question of whether it even offers a representation of both views. As Phillips (*ibid.*: 292) described it, the Machine incorporates a lag between the occurrence of ‘expenditure’

⁷The source is in fact a reference letter by Robbins for Phillips at the time of his promotion to Reader, dated 1 April 1954, and is in Phillips’s Staff File. Bollard also had Robbins (1972) in his bibliography, although he never actually cited it.

and the accrual of ‘income’ (or some other lag on different interpretations of the water flows). The incorporation of such lags by Dennis Robertson is precisely what led to many of the criticisms of his approach. Since Phillips offered no response to those criticisms, it is hard to see how he resolved the matter. Then, there is the question of whether treating ‘investment’ as a diminution of idle money balances, as Phillips did, offers a correct interpretation of *The General Theory*. Equally, there is the matter of Phillips (*ibid.*) treating ‘consumption’ as being the residual from ‘income’ when ‘saving’ was determined whereas the usual interpretation of the Keynesian system has changes in consumption determining income. Whatever the merits of either side of these issues, it does not seem that they arise merely from a confusion of stocks and flows. Then, there is a further question as to what the water in the Machine is taken to represent. It obviously suggests the circular flow of income. But Phillips (*ibid.*) said that it represented ‘money’, which is not the same thing at all. It is difficult to see that a device leaving that many doubts could have resolved a serious theoretical argument.

Whatever the ultimate resolution of those ideas may be, it is easy to see that the Machine did not in fact resolve this debate simply because it continued, apparently undiminished. Hahn (1955: 52) said that although there had been much discussion, the area of disagreement was increasing, and then further contributions appeared from Tsiang (1956), Smith (1956), Ackley (1957), and Rose (1957). None of them even mentioned the Machine. There is something of an exception in Newlyn (1962: Chapter VIII) and its later editions. He did present a version of the argument apparently drawn from the Machine. Newlyn, though, was exceptionally close to the matter and furthermore his exposition, by showing how such an argument appears, reveals how far other textbooks were from presenting it. Lipsey (1963: Chapter 31), for example, made no such argument and clearly treated the loanable funds and Keynesian theories strictly as alternatives.

The absence of any attention to Phillips’s approach is also a characteristic of assessments of the loanable funds debate by non-participants—when the focus is actually on the debate, rather than on Phillips. So, for example, none of Bibow (2000), Hayes (2010) or Jakab and Kumhof (2015) mentioned the Machine. But perhaps most telling of all is the case of Johnson (1961). He summed up the debate saying that it had been about stocks and flows, and there had been ‘violent and prolonged controversy which had turned out in retrospect to be sterile’ (*ibid.*: 6). Since, as is apparent in many of Johnson’s writings he was much given to snappy dismissals of problems he thought unworthy of attention, he could be expected to relish the thought of a violent and prolonged, yet sterile debate that had been solved

by a water-filled machine invented by an undergraduate. Although Johnson sometimes understood much less than is often supposed, including, as discussed in Forder (2017), in relation to the Phillips curve, in the case of the Machine, he must have known its capabilities since Moggridge (2008: 102–103) noted that he was the person put in charge of the one at Cambridge University in 1951, and that he demonstrated it to students. Yet, he too said nothing about it. It seems, then, that not only did the Machine not resolve the dispute but made no impact at all on its content.

There remains the possibility that the Machine made some wider impact but, if so, it is undetectable. In computing, it must have been apparent as early as Smith and Erdley (1952) that electronic computers were more useful. In economics, there is a scattering of references to the Machine in the research literature, but no substantial discussions suggesting it to have made a difference to the study of economics. Even when Coddington (1976) coined the term ‘hydraulic Keynesian’ to describe the outlook of those like Meade and Phillips to the effect that there are stable and discoverable aggregate relationships that could be used to control the economy, he eschewed the opportunity for an apposite reference to the Machine—or perhaps he just did not think of it.

So the Machine changed neither economics nor computing. There seems to be no analytical basis at all for the claim that it resolved the loanable funds debate—except perhaps that those making that claim suppose the debate to be fully summarised as a confusion over stocks and flows. That, though, finds no support in later analyses of the debate. Also, the Machine was no commercial success.

All this being the case, though, and since so much that has been said about it has been overblown, one might reflect that apart from launching Phillips’s career, it was intricate and clever, its conception was remarkable, and its construction showed great skill and embodied an attractive idea about effective teaching. Moreover, for its time it was a technologically sophisticated analogue computer, and surely there are not many inventions of undergraduate students that find a place in the Science Museum.

5 The Phillips Curve and Policy Making

Although, as explored in Forder (2014a), something called a ‘Phillips curve’ takes one of various roles in very many macroeconomic models, the extent to which any of those ideas are properly traceable to the work of Phillips, and to his much-cited, though probably rather less-studied Phillips (1958a),

tends to be enormously exaggerated. That exaggerated standing is an aspect or consequence of a widely told story of Phillips's work being quickly accepted and becoming the basis for the adoption of inflationist policy intended to maintain high employment with continuous excess demand, and this view being abandoned only after much dispute following from Friedman's (1968) supposedly innovative idea that the trade-off would disappear as expectations adjusted to reality.

That whole story—so I have argued in Forder (2014b) and Forder (2018)—is fictitious and indeed only started to be told in about 1975, just about the time Phillips died. In fact, inflationary policy was never adopted on the basis of any understanding of a Phillips curve, and barely ever advocated; Friedman brought no revolutionary insight by raising the matter of inflation expectations. Moreover, contrary to innumerable unsubstantiated assertions to the contrary, it is all but impossible to find any influence of the Phillips curve on policy making except in so far as it indicated a way to control inflation (as it was frequently taken to do after 1970). That was the use to which it was put, for example, by the Council of Economic Advisers (1969) in the USA.

However, it is only the beginning of the story that really concerns Phillips. That beginning is the remarkable idea that it was not until 22 years after *The General Theory* that anyone had the thought that wages rise more quickly when unemployment is low, and this 'missing equation' as Friedman (1970) called it, was discovered. The truth, of course, is that earlier statements of the idea are readily found. When they have been noticed, though, they have tended to be dismissed, one way or another, as having no historical significance. That approach was taken furthest by Leeson (1997a: 118–119) who, identifying many such instances, simply stated: 'Most of these precursory discussions disappeared almost instantaneously from the mainstream literature' (ibid.: 119). He said nothing to establish that, and it seems just to be axiomatic that Phillips was the crucial innovator.

If we simply dispense with that axiom, the explanation of why these authors' contributions went unremarked—'disappeared'—is evidently that there was no reason for them to attract attention. There was never a surprise in the idea of a negative relation of wage change and unemployment, and indeed, it was stated many more times than even Leeson suggested and lay in the background of numerous other discussions—the question of whether price stability and full employment could be simultaneously achieved was a universal preoccupation of the post-Second World War period. So, there is 'not the remotest possibility that such an idea would ever have surprised anyone', as I concluded from attention to the literature, in Forder (2014b: 12).

Bollard (2016: 160), though, was another who rather than consider that kind of thinking, preferred to give Phillips as much prominence as possible. He listed just a small number who had previously considered such a relationship and said that economists had ‘toyed with’ the idea before Phillips. But when he produced his work, ‘There was something in the Phillips curve relationship that fascinated LSE economists’ (ibid.: 168).

There is, of course, a question as to what point Phillips’s paper was making if it was not this one. That takes some extra colouring from the point that Phillips himself said it was a rushed paper, a ‘quick and dirty job’ as Schwier (2000: 24) recalled him saying; or perhaps that it was done on a ‘wet weekend’ as Bollard (2011: 7) claimed. Not only was it quick, though, but also poor. Lipsey (1960), noted various deficiencies, as did a number of others until Holt (2000: 309)—one of Phillips’s colleagues at the Research Techniques Division—retrospectively summed it all up by calling it ‘conspicuously sloppy’.

Although it contained a few other scraps of novelty—the nonlinearity of the suggested relationship perhaps being the most important—the principal point of the paper, clear enough if one is not presuming something else, was that it suggested that for the whole period from 1861 to 1957 the mathematical relationship between unemployment and wage change in the UK had remained the same. That invariance, plainly, is a striking claim since one would certainly have expected the social, political and economic changes over that period to have affected such a relationship. So the suggestion—the remarkable suggestion—in Phillips’s paper was that wage determination was invariant to all these things. The finding of any such enduring regularity would be a matter of great significance.

In the particular case in question, there was also a possible conclusion very relevant to policy, though not one highlighted by Phillips. That is that, contrary to many views, inflation must ordinarily be due to ‘demand-pull’. In the thinking of the time, employment was determined by demand. If wage change, and hence inflation, was determined by unemployment, it was ultimately determined by the level of demand as well. The alternative, ‘cost-push’ theory supposed such things as the aggressiveness of trade unions—more or less independently of the level of demand—were key. But if the variation in trade unionism that had occurred over 100 years had not affected wage bargaining, that could not be correct.

The process by which Phillips reached his conclusions began with the ideas that the price of labour would adjust to differences in supply and demand and that when unemployment was falling, wages would rise faster than when it was rising. Price change was disregarded except when the basic

relationship would have led to a fall in real wages, in which case he supposed wages would rise faster than they otherwise would. In this way, an element of 'wage-push', not determined by unemployment, was admitted to the picture as an addendum to the relation indicated by the curve. Then, Phillips took the data up to 1913, sorted it into the years in which unemployment had been in various ranges—0–2%, 2–3%, etc.—and found the average rate of wage increase for each. That gave him six data points corresponding to his six ranges of unemployment, and he simply hypothesised that they were related by the equation $\log(y+a) = \log b + c \log x$, where y was the rate of change of wages and x was the rate of unemployment. Then, he estimated b and c by least squares using just four of the data points and fixed a to fit the other points. The resultant curve passed very close to the six points, though there was no sniff of any formal estimation or statistical testing. Only one period seemed to diverge from the pattern, and Phillips dealt with that by substituting a different wage index for those years, thereby bringing about close conformity of that period to the others. The idea of the rate of change of unemployment being relevant found its representation in the fact that taking the annual data points in chronological order they traced anti-clockwise 'loops', as Phillips called them, round the curve. When unemployment was falling—the economy was moving leftward, year-to-year on the graph—wage change was above the curve; when it was moving rightward, wage change was below it.

Phillips then considered the post-1913 data and, performing no further estimations, simply observed that all or nearly all the data points lay close to the curve, except where his subsidiary hypothesis about rapid price increase explained a deviation from it. In the post-1945 period, there was only one business cycle and its 'loop' ran clockwise, but Phillips introduced an argument about there being newly relevant lags in the process, and this removed the anomaly. He drew the obvious conclusion that inflation would be stopped (or, in a then popular variant proposal, wage increases would be stopped, allowing prices to fall) if an appropriate level of unemployment were achieved.

The approach of constructing the six points was a textbook way of making rough calculations before electronic calculators eased the process, rather than the key to understanding a brilliant, obscure analysis, though Desai (1975) tried to argue that case. The fitting of the curve, one might say, was an engineer's way of finding something that would work more than an econometrician's approach to statistical analysis, much less a social scientist's to the discovery of underlying forces.

The manner of handling of the post-1913 data clearly shows the point being made was simply that nothing much had changed from the earlier period. That, first of all, confirms that it was the constancy of the relationship over the whole period that was being emphasised, but secondly, since further calculations could have been performed, and there was no scientific reason for not performing them, it also confirms that the work was indeed ‘quick and dirty’—rather rushed, in other words. But then the superficiality of the work is also apparent from the shortcut calculating technique, lack of statistical testing, off-hand treatment of price change, substitution of data series and ad hoc introduction of lags when required.

It is apparent, then, that Phillips’s paper did not transform economics. It was not a high-quality piece of analysis; the idea that it is so frequently said to have introduced was simply nothing new; and the actual point it was making has hardly been taken seriously, let alone been widely accepted. Moreover, when further tested, as it was by Lipsey (1960), it was easily rejected.

Then, there is the question of how the paper was related to Phillips’s other work. In appearance, it is quite different since most of what Phillips wrote is deeply technical. Nevertheless, Lipsey (1978), taking the view that the idea of a negative relationship was strikingly new at least at the time of the entirely theoretical Phillips (1954a), said that it was in the earlier paper that the ‘now famous curve’ made its ‘debut’ (Lipsey *ibid.*: 49). In the same vein, Leeson (1997b: 158) described the curve of 1958 as ‘uncannily like’ the earlier one, which it is not,⁸ and clearly felt it should be seen as an integral part of Phillips’s life’s work. The specific case of Lipsey’s argument is considered more fully in Forder (2013), but these sorts of ideas lose their purchase when it is recognised how ordinary the idea of the negative relationship was. There is simply no reason to see another curve deeply buried in the theoretical analysis of stabilisation policy in an earlier paper as intimately related to an empirical relationship in a paper making an entirely different sort of argument. Moreover, the idea that a paper so widely, and justifiably, seen to be as rough and ready as the 1958 one was an integral part of the life’s work of a man who only published 11 papers in his career is comically improbable.

⁸The earlier one plotted the change in all factor prices and became strictly vertical at high employment; the later one treated only wages and did not. Phillips (1954a: 308) postulated ‘a fairly sharp bend where it passes through zero rate of change of [factor] prices’. The sharp bend in the 1958 curve is at about a 2% wage increase, a much higher level by the standards of the time. (The representation of the 1954 curve in Leeson (1997b: 159) has much more resemblance to the 1958 curve than did the original.)

A further issue is whether Phillips himself thought the curve made a case for inflationary policy. It is often implied, for example, in some of the textbooks considered in Forder (2015), but only rarely, as by Chapple (1996, 1998, 1999), has it actually been argued, that he did. No such case can be made from the 1958 paper since he only discussed aiming at stable or falling prices. Most of what else he said about actual policy was in Phillips (1962), his Inaugural Lecture. There, he summarised his theoretical work on stabilisation policy and moved on to discuss policy possibilities. He mentioned Phillips (1958a), though he indicated no particular link with the theoretical work, and expressed doubt about whether price stability would be achieved, but the implication was clear that it would be desirable. He discussed ways of improving the relationship between inflation and unemployment, such as by promoting worker mobility or changing the role of trade unions. Those were very ordinary views, with nothing inflationist in them, although they do reveal that even he placed no weight on the idea that the inflation–unemployment relationship was invariant to institutional change.

Once it is recognised that it is not true that conventional opinion saw the curve as suggesting inflationary policy, there is no reason to attempt, as Leeson (1999) did, to construct elaborate stories about how Phillips's special insights led him to a different view. Certainly, he was no inflationist, but there was nothing special in that. Leeson's suggestions cannot stand the weight put on them, but one deserves attention for its suggestion of wider importance. It is that, as he put it, Friedman 'used "Phillips' Adaptive Inflationary Expectations Formula" to undermine the high inflation Phillips curve' (ibid.: 97). Since no other basis for the claim was indicated, that presumably rests on the evidence adduced in Leeson (1997b: 166) where he had argued that the expectations argument should be called the 'Phillips-Friedman-Phelps critique' because its 'intellectual origins' lay in a conversation between Phillips and Friedman in 1952.

The apparent intent to put Phillips at the heart of the supposed revolution associated with the 'Friedman-Phelps' expectations argument is doomed by the fact that, as argued in Forder (2010), the idea of expectations adjusting to ongoing inflation long predates the 1960s. But in any case, all Leeson said was that in private correspondence Friedman told him he had discussed with Phillips the modelling of expectations in conditions of hyperinflation, and that Phillips had suggested a formula later used by Cagan (1956). So Phillips helped with a mathematical representation of an idea that Friedman already had. That is a long way from making Phillips's contribution crucial since in his famous discussions of the expectations argument—Friedman (1968, 1977)—he did not use *any* formula.

A question with more substance is that of why Phillips produced such a rushed piece of work. Sleeman (2011) speculated extensively as to various facts to make a case that it was to enhance Phillips's chance of being appointed to the Tooke Chair. However, the Chair, which had been vacant, was revived specifically for Phillips,⁹ and it is hard to see his appointment was ever in doubt. A conspicuously sloppy paper could anyway hardly have helped his chances. But perhaps the most clear-cut piece of evidence against Sleeman's view is that the CV which Phillips submitted for the appointment did not list the 1958 paper,¹⁰ although another one 'in draft' was listed. If he had been rushing to complete it for the application, it would have been there (and being 'in draft' its sloppiness would have been invisible).

Another idea is that he had to finish it before taking an upcoming sabbatical. This takes some apparent life from the point that Blyth (1975: 306) reported Phillips, presumably referring to Brown (1955), saying of his own paper that 'It was a rush job', and that he wanted it finished before his sabbatical, and 'A J Brown had almost got these results earlier, but failed to allow for the time lags'. That appears to suggest Phillips was concerned about his priority in some discovery. Any impression that Phillips thought he was the discoverer of a negative relationship between wage change and unemployment is immediately dispelled by the point that the construction of the curve did not involve any lags. They related only to 'loops'. Perhaps Phillips thought his loops were particularly important; perhaps, by the time he was talking to Blyth he was either confused himself or needed something to say to explain why he had published such a poor paper. In any case, Phillips's explanation is not quite convincing since Cornish and Millmow (2016: 4) said he only left the UK in March 1959, so there would have been plenty of time to improve it, and of course he could have worked on it while travelling or on arrival.

A better explanation is probably that it was a result not so much of haste as of impatience. It is explained by the fact that for Phillips, as of 1958 at least, it was an *unimportant* paper. Once the idea that it presented one of the great insights of post-war economics is shaken off, there seems no objection to this view. It suggested an intriguing and striking possibility—that of the long-enduring relationship—but it had no claim to being a thorough working out of the analysis and was not in the area of Phillips's other work. So,

⁹This is apparent from a letter from the Director of the School to the Chairman of the Court. See Phillips Staff File, BLPES Archives: 2 April 1958.

¹⁰Phillips Staff File, BLPES Archives: Phillips CV February 1958.

having presented his idea, he stopped working on it. The paper was, in other words, no more than the result of playing around with some data on a wet weekend. That being done, it was as ready for publication as Phillips cared to make it.

All this leaves one curiosity, or apparent curiosity, which is the question of how it is that the ‘Phillips curve’ features so prominently in the lexicon of economics when so little thinking about it is attributable to Phillips. The answer to that, though, is purely lexical. It is often said that Samuelson and Solow’s (1960) use of the expression is its practical origin. More likely, that is to be found in the fifth edition of Samuelson’s textbook (Samuelson 1961: 383), where he described what he called a ‘Phillips curve’ (as he did in later editions). Those books had wide readership and probably influenced the content of other textbooks too. As noted in Forder (2014b: 133–135, 284, fn. 20), use of the expression only became very common at the end of the 1960s. That would be just when those who learned from Samuelson started to teach and publish. But more than that, as Samuelson used the expression, it described cost-push inflation and so his thinking was quite different from Phillips’s. Moreover, it was not only different, but also earlier, since much the same discussion, without the label, appeared in Samuelson (1955: 358–359). What that means is that when Samuelson put the matter in terms of the ‘Phillips curve’ that was *merely* a label—a new label for an old idea. So, in such textbook treatments and the work later produced by their readers, the idea of the ‘Phillips curve’ owed nothing to Phillips, either in content or in genesis.

6 Stabilisation Issues

Great though the retrospective attention on those works has been, the bulk of Phillips’s research was of a quite different character. It concerned much more formal analysis of matters related to the optimisation of stabilisation policy. Some of this work was purely theoretical, and the rest concerned matters of estimation arising, broadly speaking, from problems in putting the theory to work.

The theoretical portion was clearly derived from engineers’ thinking about optimal control and feedback problems. At the time Phillips wrote, the idea that these ideas had clear applications to economic problems, and particularly macroeconomic stabilisation, was beginning to be clearly recognised, with Morehouse et al. (1950) and Simon (1952) being early contributions before Tustin (1953), a professor of engineering, followed up on his

brief remarks in Tustin (1951–1952) by expounding and proselytising the approach. His book attracted a review from Phillips (1954b), who gave the publication year as 1954, a comment and elucidation from Allen (1955) and a number of other reviews, of which the most positive was Goodwin (1954) and the most thoughtful Tizard (1957). Although unnoticed in economists' accounts of Phillips's work, Richardson (1991) gives an account of the movement and Phillips's notable, though not exceptional, role in it.

Phillips's own principal contributions were Phillips (1954a, 1957). He noted that conventional macroeconomic analysis tended to suppose that automatic stabilising forces would bring equilibrium about, but the process might be so slow as to warrant additional policy measures. However, he pointed out that it was well known in engineering that automatic forces, or 'negative feedbacks', could well induce oscillations or even be destabilising. Consequently, the presumption that the basic problem in Keynesian policy making was to find ways to augment automatic forces was extremely dangerous. Equally, policy actions which would deliver the desired equilibrium in comparative static analysis might also generate surprisingly large fluctuations on the way. In either case, stabilisation policy would be much more difficult to design than it seemed from conventional analysis.

Phillips (1954a) showed that in the face of a shock, policy would need to respond to the size of the shock, the accumulated error in outcome from target, and the speed of movement of the target variable—these being the 'proportional', 'integral' and 'derivative' aspects of stabilisation. Even in the simple case where prices were fixed and there was a target for the level of production and a single instrument, Phillips showed that unless policy was framed in terms of all of these, it would either be ineffective in meeting the target, or else introduce possibly violent oscillations. He then considered the case of price flexibility and the consequences, for example, of a change in prices affecting the demand for money, hence the interest rate, and in turn investment. Since the change in prices accumulates while the error lasts, that was of the integral type. On the other hand, if demand were affected by the rate of change of prices that would introduce a derivative element, although the direction of the effect would depend on how changes in prices affected expectations of future prices. He showed that, where changes in price led to changes in demand in the same direction (on the basis that expectations were that the price trend would continue), there could well be parameter values whereby the system would be unstable—the more so, the greater the response of prices to errors in output. But if expectations were that price changes would be reversed, the system was much more stable.

One aspect of the problem generating complexity in the solution was that Phillips, realistically, allowed for lagged effects of a change in one variable on another. In Phillips (1957), he showed that further complexity arises from a consideration of non-exponential lag forms and that small changes in lag structures could produce very substantial changes in the stability properties of the model. This meant that detailed econometric knowledge of lag structures would be required for policy to be effective. Phillips (1958b) was then an algebraically intensive specific application of the ideas, and Phillips (1961) developed them in a model incorporating growth.

The most striking single point emerging from these works is the extreme complexity of dynamic responses of even rather simple-seeming systems. Something of this line of thinking had emerged, for example, in Samuelson (1939), Goodwin (1951), and Friedman (1953), but an obvious point about Phillips's analysis is that it took the point much further and gave it much more general expression. A second point, though, is that particularly in contrast to Friedman, Phillips seems to have taken his results as setting an agenda for further research. Whereas Friedman took such findings as warnings against discretionary policy, Phillips set about econometric investigation that might improve it.

Even though concrete conclusions from such abstract work were sparse, Phillips seems always anxious to identify those that are available. So, from the analysis of Phillips (1954a: 314–315) he took the point that even small policy adjustments, if applied continuously, could be effective. That suggested that monetary policy, even if not powerful, might be more effective than the slow-moving fiscal policy. In Phillips (1957: 276), he drew attention to the benefit of reducing any delay in implementing corrective policy—contrary to Friedman, that is a proposal for ‘fine tuning’—but said that even if delayed, policy should seek to operate gradually. Secondly, he found it was usually necessary that there be some derivative control, and the longer the delay in implementing policy, the more important derivative control became. These ideas may not have given much immediately useful guidance to policy makers, but the intent of the author actually to advance the understanding of stabilisation policy is very clear.

The other branch of Phillips's research, then, was in econometric theory and seems very much to have been addressed to the problems raised by his theoretical work. Phillips (1978) is incomplete, so it is impossible to see what theoretical (as distinct from econometric) motivation it had, or what conclusions Phillips drew. Otherwise, his principal papers were Phillips (1956, 1959b). The first considered the matter of lag structures, and the estimation of what must be continuous processes with discrete data

which do not reveal the form of effects between observations. The second noted that systems of lagged dependencies could be approximated by systems of linear stochastic differential equations and sought to develop ways of estimating such systems. In both cases, he was clearly following up the econometric aspects of the issues raised in his theoretical work—seeking to discover, in other words, how empirical work could be done that would lead to the knowledge required for effective stabilisation. Again, the practical objectives are plain.

The theoretical work, and in particular the 1954 paper, was widely noted—with the exception of the Phillips curve paper, it is much his most cited work. As well as that, the ideas also appear without actual citation in Allen (1956: 69–74, 262–280 and Chapter 9). The relation of proportional, integral and derivative policy was explained by Day and Beza (1960: 408–416) and Meade (1971). Lipsey (1963: 529–533), citing Phillips (1962), gave a simplified account of the line of thinking. Kendall (1960) gave an enthusiastic account of the ideas, seeing great promise in them and also reproducing an otherwise unnoticed circuit diagram describing the determination of the balance of payments, which he said came from an unpublished paper of Phillips. Allen, Day, Meade, Lipsey and Kendall were all LSE figures, so in all cases the ideas must have come from Phillips and it is interesting that they are all textbooks or, in the case of Meade's and Kendall's, works clearly intended to disseminate understanding as much as to develop it. Various later works involving Meade, such as Vines et al. (1982), continued to describe his work as important sometime later, and there may well be other accounts of his ideas, so here, at least, Phillips's work had a clear impact.

Phillips's econometric work presents something of a conundrum. Most systematic assessments of it seem to be contributions to Leeson (2000), and even allowing for the fact that the book was intended to raise Phillips's reputation, the message that his econometric work was exceptionally insightful is clear, and in this case, all that admiration comes from expert authors who are clearly in touch with the work itself and so in a position to assess it.

Admiration, though, is one thing and detecting Phillips's influence through this work is not at all easy. Of Phillips (1959b), Hansen and Sargent (2000) began by saying it shared the fate of Muth (1960, 1961) in that it took a long time for others to see how much could be done with the ideas and cited 13 works as following where Phillips led. Since those were chosen to show Phillips's influence, it is interesting that of the 12 that are published, only six actually cite him.

Making the position clearer himself, Peter Phillips (2000a) said that Phillips's work 'opened up a new field of research on continuous time econometric modelling and statistical inference' (ibid.: 342) and he also placed much emphasis on the difficulty of the problems, and the sophistication of Phillips's appreciation. But concerning Phillips (1956), Phillips (2000b: 349) observed that points from the paper were made 15 years later by Sims who did not refer to Phillips's work and 'must have been unaware of it'. Then, he said that Phillips's proposed method to estimate linear stochastic differential equations was 'never used' (ibid.: 351). Similarly, Phillips (2000a), discussing Phillips (1956, 1958b) noted that, 'The historical importance and relevance of his work to the subject of error-correction modelling has still to be widely recognized in the literature and is, unfortunately, not cited in any of the recent textbooks, handbooks or overviews of the subject' (Phillips (2000a: 344). Citing himself and Loretan (1991), he said they recognised Phillips's significance and priority. They did, but only to assert it in the first lines of their paper before referring to Bergstrom (1988) for a history of the matter. That source, though, mentioned neither Phillips (1956) nor Phillips (1958b), but referred only to Phillips (1959b) saying it 'developed an algorithm for estimating the parameters of a complete system of stochastic differential equations from discrete data', and 'The algorithm developed by Phillips was never used' (Bergstrom ibid.: 369).

The most upfront about the matter is Hendry (in Hendry and Mizon 2000). After writing a page describing one idea, he concluded it was 'not very relevant for empirical modelling' (ibid.: 358). Hendry is, however, particularly interesting on the matter of error-correction mechanisms (ECMs), saying that 'once one knows where to look' (ibid.: 359) in Phillips's work, they appear readily in transformations of optimal control problems. But concerning the work of Davidson et al. (1978), which presented ECMs as a general class of models, he said he 'did not even think' of using Phillips's work, 'despite both knowing his work well, and having studied Sargan (1964) many times' (ibid.: 359). In that paper, Sargan too made no mention of Phillips's work on control theory, despite being an LSE colleague of Phillips at the time, and his subject matter being the relation of wage change to unemployment. It seems that no one saw Phillips's work as suggesting ECMs until after they were otherwise developed, though, then, there are plenty of remarks on their relationship—Salmon (1982) and Qin (2013) being two works of historical bent making this point.

Then, there is Phillips (1978), the most substantial of the work he left unpublished. Its influence, of course, was limited by that fact. Hendry and Mizon noted its insight, but also said Engle et al. (1983) 'originally failed

to build' (ibid.: 362) on Phillips's work. Pagan (2000) noted the similarity of Phillips's approach to that of Box and Jenkins (1970), but also said that Phillips's paper was 'probably the clearest analysis of how to construct a full information estimator of the parameters of simultaneous equation models with moving average errors' (Pagan ibid.: 420). That, I suppose, might be expected to get it noticed, but Peter Phillips (2000a) called Phillips's algorithm one of 'creative improvisation' (ibid.: 346), although leading to a technique which is no longer favoured, and noted that Hannan and Deistler (1988) (an advanced textbook) do not refer to Phillips's paper.

These authors' conviction that Phillips's work showed great insight is clear. But they make equally clear that he made rather little impact and important ideas were seen in his work only when they had been independently rediscovered. Insightful, clever, even brilliant, he may have been; important he was not.

7 Phillips's Last Paper and the End of His Career

Phillips (1968) was a paper, of rather obscure publication, hardly noted at all until it became the subject of historical interest,¹¹ but which has occasionally been regarded as most important. Phillips considered the problem of simultaneously estimating and controlling a system. He argued that if there is an imperfectly estimated system, and control is applied to achieve policy maker goals, then because of problems of identification, the new data that is generated cannot be used to improve the estimates of the system. He suggested considerations that might in particular cases dissolve the problem but concluded that computational advances were required before the problem could be fully addressed.

The question that has attracted most attention is how this, together with the fragmentary Phillips (1972/2000), is related to the 'Lucas critique' of Lucas (1976). Blyth (1987: 857) said simply that it 'foreshadowed' it. Court (2000: 465) suggested that the extent to which they are similar, as well as the question of which is the more important, can be 'left to the judgment of the reader'. But Bollard (2016: 140) clearly asserted Phillips's argument was 'an early precursor to the famous Lucas Critique'. Leeson (1998: 99) went

¹¹A search for its title in Google Scholar in December 2017 produced sixteen hits—seven in works by Leeson, seven in other historical works and two in statistics.

even further, claiming that ‘Phillips’ work was not vulnerable to the Lucas Critique since Phillips authored the “Phillips Critique” several years before Lucas (1976)’.

That last is obviously a non sequitur, and certainly Phillips’s whole project was vulnerable to the Lucas critique, namely that since agents’ anticipations affect their behaviour and are formed in the light of their understanding of policy, estimated relationships based on a period when one policy operated would not generally describe behaviour in a different policy regime. Just what Phillips had presumed, starting with Phillips (1954a), or even with the Machine, was that there were such relationships which could be estimated and then could form the basis of control. But if the implementation of control is going to change the relationships, that is not so.

Anyway, as Laidler (2002) said, and as also noted by Schwarzer (2012: 995), the argument from Phillips (1968) does not depend on the idea of agents’ behaviour changing as a result of their re-optimising in the light of the policy. Phillips’s is a purely statistical argument. If a system is imperfectly understood, and then controlled, subsequent data cannot be used to improve the understanding.

If one were to look for Lucas’s idea in the consideration of the application of control theory to economics, something much closer could be found in Tustin (1953: 2) who, contemplating policy success in routinely achieving nearly full employment, said, ‘The conditions of stability about this new level are radically different because the region of operation is now within the less flexible and sharply non-linear range of employment saturation. The nature of business expectation is also profoundly modified’.

That recognition of the problems of the economist and the control theorist coming together in the modification of expectations suggests Lucas’s argument that a change of policy regime could change the structure of the model, but no such idea is in Phillips (1968), nor such a close approach to it elsewhere in his work. Phillips and Quenouille (1960: 337–340) may be the closest, in that the idea that the actions of policy makers can affect the structure of the system is noted.

In this case, though, it may be that the attempt to give Phillips credit he is not due may have distracted attention from the point made by Peter Phillips (2000a: 346–347) that Phillips’s argument is *more* fundamental than Lucas’s. A response to the Lucas critique is to attempt to estimate the ‘deep structural parameters’ which are invariant as policy changes. That may or may not offer a practical solution to the problem, but in any case, Phillips’s argument could apply just as much to that project as to one estimating behavioural relationships.

Another point, though, and a much more important one in reality is that the argument of Phillips (1968) certainly applies to Phillips's own work as well. In that, there may well be a clue as to the reason for his ceasing to work on macroeconomic stabilisation. There is an obvious coincidence of his giving, but not publishing, the Walras-Bowley Lecture in 1966 and leaving LSE—and ceasing his work on control theory—in 1967, which must have been just when he was writing Phillips (1968). The possibility of a connection is clear. The 1968 paper raised a problem of identification. Pagan (2000: 421) suggested that the Walras-Bowley Lecture was not published because Phillips was unhappy with the section on that question, and it was that section that was missing when Phillips's friends came to publish the paper. It is all very much consistent with the idea that in his 1968 paper, Phillips felt he had found a problem he could not solve. Indeed, Lancaster (1979: 634) reported, apparently from personal knowledge, that Phillips gave up working on control theory because he believed 'the necessary techniques were beyond his grasp'. It might be, then, that little noticed as it is, Phillips gave up control theory because of his findings in the 1968 paper.

That would be understandable since the message of that paper is particularly damaging to one of Phillips's picture of the problem of designing econometrically based policy. In a world susceptible of simple enough econometric representation—the world of Klein and Goldberger (1955), as it were—it just *might* be hoped that the structure of the economy could be understood sufficiently well that improvements in understanding after control had begun would be unimportant. But that is certainly not true in the world of proportional, integral and derivative control, where lags are not just of unknown length, but unknown structure too, and where precise knowledge of those lags and their structure is essential to effective control. In the world of Phillips (1954a, 1957), the argument of Phillips (1968) posed a very serious problem. It was the character of his own contributions that made the 1968 paper so damaging. Authorship, even when genuine, gave him no protection from the implication of the argument.

8 Conclusion

The attention often given to Bill Phillips's poor degree has suggested to some that the speed of his ascent at LSE is in need of special explanation. This is not such a mystery. Clever individuals do sometimes have poor degrees and go on to great work, and that was more true in the less-credentialist 1940s and 1950s than later. In Phillips's case, the support of Meade and

particularly Robbins has often been noted and must have been powerful. Less noted but perhaps just as important is the attitude of Maurice Kendall, who wrote to Robbins supporting both Phillips's Readership and his Chair. In the former case, he said of Phillips's work that it,

makes possible the study of the behavior of an economic system under alternative types of disturbance in an exact manner ... I have high hopes that Dr Phillips' work may at last bridge the gap between the physical sciences and economics and that it may place at the disposal of the economist a wealth of resources which has hitherto been inaccessible to him. Dr Phillips has just those qualities which are necessary to pursue this line of development, an expertise in electrical engineering and mathematics, a penetrating insight into economic problems and a great ingenuity in adapting the former to the latter.¹²

That surely captures the aspiration with which Phillips began his work: he was aiming at last to bridge the gap between the physical sciences and economics and to treat the latter in an 'exact manner'. It would have been no small achievement.

Phillips did not achieve that of course. He could not, writing as little as he did. The signs are, though, that Kendall continued to hope since when the question of reviving the Tooke Chair came up, he wrote again to Robbins saying he thought Phillips 'on the threshold' of 'fundamentally important contributions to econometrics' and that he needed to be freed from other responsibilities to pursue his research so that reviving the Chair for him should have 'first priority' on the School's funds for new appointments.¹³ That view is also evident in Kendall (1960). It was very powerful support, although as it turned out, Phillips wrote even less after promotion than before.

So, rather than questioning how Phillips did so well with such a poor degree, it would be better to ask why he achieved so little. He had such enthusiastic support from Meade, Robbins and Kendall; he had a base at LSE, which might have been expected to give him some prominence; and clearly, the best of his work was of a very high standard. Yet, as has become apparent, it was very much less noted than it might have been.

The mathematical sophistication of Phillips's research must be part of the reason it was not more noted. Some of it was well ahead of its time and most of it would be of a kind that many economists of the 1950s and 1960s

¹²Phillips Staff File, BLPES Archives: letter of 3 May 1954.

¹³Phillips Staff File, BLPES Archives: letter of 2 August 1957.

would have found indigestible or, quite possibly, thought too abstract to be useful. It may also be that he was a little shy in promoting it, and certainly in terms of pushing into publication as much as he might have.

There is a further possibility, though. Phillips (1958a) is much easier, and surely it was widely read, at least after about 1968 when the terminology of the ‘Phillips curve’ came to be so widely used, and many must have presumed a close relation of the idea to his paper. The impression those readers would have formed would not have been a good one. If one reads it as they would have—as neither the work of a fool who did not know nominal from real variables, nor of a great surpassing genius of his age, but just as someone writing about the relationship of wage change or inflation and unemployment—then it is easy to see that it is a rather poor paper, and certainly a shabby one. There was much better work on that topic available to those readers. Indeed, read as advancing the idea of an unchanging 100-year relationship, it is also a silly paper. The reader of *that* paper would feel no reason to read much else by Phillips and certainly not anything as forbidding as most of his work was. In that way, Phillips (1958a) may have done him real harm.

Beyond that, as things turned out, economics moved well away from the presumptions stimulating Phillips’s work. Certainly, stabilisation policy seemed disappointingly ineffective to many, and various authors suggested that it had in fact been destabilising—not just Friedman (1968), but also Dow (1964). Those things might seem to give strength to positions like Phillips’s. But on the other hand, outcomes were nothing like so volatile as some of his work might have seemed to suggest was likely. The Lucas critique did in due course deliver another and distinct blow against the practical possibilities for stabilisation policy and although interest in the application of control theory to macroeconomics eventually brought forth Ball (1978), both economists and, to some extent, policy makers turned towards the view that simple rules promoted stability. That was another world from Phillips’s conception. Moreover, the fact that Phillips himself gave up work on stabilisation policy is also a part, albeit a small part, of the story.

In the end, then, Phillips achieved fame through the attaching of his name to an old idea and had it doubled by a fictitious story and the many transformations through which the idea of the ‘Phillips curve’ passed, and then redoubled by those who have tried to aggrandise his standing in the economics of the post-war world beyond any historical recognition. His reputation was certainly damaged by the idea that his paper promoted inflationary policy, with or without his support, but it is also a poor paper, and that can

only have harmed him further. In addition, there is the point that those who have tried so hard to talk up his brilliance, and his heroism, may succeed in getting a message across, but in too many respects it is not an accurate message, and these too really do Phillips no good; they certainly make the historical appreciation of his career rather harder. It is possible to make a balanced assessment, though, free of the baggage of myths about the curve and of the later hero worship. Thus, it can be said that Phillips was innovative, quirky and clever. Several pieces of his work evidently showed great expertise as well, and some of them were well ahead of their time, much deserving of greater recognition than they had. Those things make Phillips a figure of some note. But he never wrote enough, and what he did write never had impact enough for him, despite his fame, to be more than that.

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Ezra J. Mishan (1917–2014)

Euston Quah and Yew-Kwang Ng

1 Introduction

Ezra J. Mishan was born in Manchester in 1917, the second of five children of David, a textile importer from Cairo who had moved to Britain during the First World War, and his wife, Freda. He went to Manchester Grammar School, then to Salford Technical College to pursue a City & Guilds qualification in textile technology. After serving in the RAF in the Second World War, Mishan gained a degree in economics at Manchester University (1946), then went on to the London School of Economics (LSE) for two years and subsequently to Chicago to do his PhD under the supervision of Milton Friedman, achieving his doctorate in 1951.

Mishan worked first as a Lecturer, then a Professor, at LSE (1956–1977), and later took up positions at a number of American universities, retiring in 1983. He continued to write until 2010, his work appearing in major newspapers and magazines in the UK and USA, such as *Economica*, *Journal*

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of *Transport Economics and Policy*, *Journal of Political Economy*, *Economic Journal* and *American Economic Review*. In 1951, Mishan married Ray (Rayzel) Blesofsky. She died in 2008. Mishan died in 2014 and was survived by four children, seven grandchildren and four great-grandchildren.

Ezra 'aka Edward' J. Mishan's contribution to economics was immense, in particular in welfare economics and cost–benefit analysis. He was one of the first economists to argue that there are significant downsides to economic growth. His book, *The Costs of Economic Growth* (Mishan 1967), maintained that increases in GDP and real income were compatible with declines in happiness and social welfare. In fact, he found that growth often brought less of the non-material things that make us happy: peace of mind, space, greenery and clean air, for example. More controversially, Mishan argued that growth led to more hedonism and a permissive society, which he saw as detrimental to welfare. His thesis preceded the rise of the environmental movement and remains persuasive and relevant to the realities of the twenty-first century, when strong economic growth is often associated with environmental degradation. Much of Mishan's writing focused on what economists call negative 'externalities', the adverse side effects of economic transactions. The parties involved in an exchange may inadvertently create costs for others not connected with it. In 1972, Mishan produced his celebrated textbook, *Elements of Cost-Benefit Analysis*, and the revised fifth edition¹ is still being used around the world today. Governments in both the developed and the developing world use the cost–benefit technique to make informed decisions in areas ranging from infrastructure development to health and education. At worldwide summits, international lending agencies also require some form of cost–benefit analysis in approving projects for developing countries.

Both of us have already written about Mishan's important contributions. In particular, Quah (2016) discussed Mishan's important contributions to the literature pertaining to cost–benefit analysis and welfare and normative economics. Mishan's influence on the economic literature in his own fields of competence has been profound and he was often recognised as among the foremost authorities in the field of resource allocation. He also enjoys an international reputation as a popular writer on the impact of modern economic growth on social welfare. Ng (2016) focuses on the possibility of immiserising growth of which Mishan (1967) was an outstanding early advocate, with Ng (2003) an obvious convert. In this paper, Quah (Section 2)

¹The fifth edition of *Cost-Benefit Analysis* (2007), published by Routledge, is co-authored with Euston Quah. The sixth edition is in preparation.

focuses mainly on Mishan's contribution to cost–benefit analysis and Ng (Section 3) focuses on the qualifications to Mishan's arguments questioning globalisation and immigration. Section 4 concludes.

2 Three Classic Predicaments of Cost–Benefit Analysis

Of Mishan's myriad valued and oft avant-garde contributions to the vast realm of economic thought, one of his more significant was his strong stance on the use of cost–benefit analysis in public policy. As evident from his many published books on cost–benefit analysis, Mishan had long been a proponent of using the cost–benefit method in evaluating government welfare and environmental projects. Here, we discuss Mishan's contributions towards three of the classic predicaments of cost–benefit analysis, namely the accounting stance, non-conventional spillover effects and the value of statistical life.

2.1 The Accounting Stance

Before delving into the nuances of conducting cost–benefit analyses, one must make clear Mishan's stance on how an economist should approach such studies. Mishan believed that an economist should focus on maximising society's welfare, in contrast with that of an individual rationally maximising his/her own personal welfare. It is with the optimisation of the summed individual benefits across all collective members of a community that Mishan concerned himself with.

In conducting cost–benefit analysis, there is an inherent difficulty in identifying the members of a society that have an accounting stance, i.e. for whom the costs and benefits accrued by a particular project should be accounted for. Undeniably, the exclusion of specific individuals from consideration in cost–benefit analysis may affect one's results. As such, it is crucial for a cost–benefit analysis to be very clear as to which accounting stance should be taken so that the results are not skewed by the influence of personal agendas.

As an example, suppose that the government is planning to build a nuclear plant near its border. Certainly, the cost–benefit analysis accounting stance should include the country's local residents who face the cost of increased radiation risk. However, what is less clear is whether the government should include the costs incurred by local residents of the

neighbouring country. As we can see, the accounting stance can be based on a multiplicity of factors, from geographical proximity to nationality, and to some other form of segmentation of society.

Another example of an accounting stance predicament would be the case of a proposed dam in a country upstream of a transboundary river. In a narrow version of the accounting stance, one would only take into account the cost and benefits to the country's citizens affected by the dam; no account would be taken of the country or countries located downstream. If, however, the accounting stance was broader than the benefits of the dam, it would also accrue to those citizens located downstream. Thus, the accrued costs and benefits for a regionally defined accounting stance would be greater.

Besides the accounting stance, Mishan was also concerned with issues relating to equity. In the example of the dam, it is clear that in a national accounting stance, the dam may generate a net benefit for society. However, poorer, rural populations, where the dam is sited, may have to bear most of the costs in the form of relocation and lost livelihoods while the comparatively richer, urban populace reap the benefits of cheaper electricity. Should a project that largely benefits the more wealthy strata of society, with the burden of the costs laden on the poor, be undertaken? This is often the case when it comes to siting facilities which benefit the wider society but harms the immediate residents around the facilities. Such phenomena are often encountered in cases of Nimbyism (Not In My Back Yard) (see Quah and Tan 1998, 2002, 2008).

Mishan wisely advocated conducting a strictly impartial project evaluation while raising the real possibilities of such ethical dilemmas to policy makers. Unfortunately, cost–benefit analyses frequently stray into the realms of morality and philosophy. Yet recognition of these issues is required as they unquestionably affect society.

2.2 Non-conventional Spillover Effects

Addressing non-conventional spillover effects is another essential component of cost–benefit analysis that Mishan shone much-needed light on. In his seminal, *The Costs of Economic Growth* (Mishan 1967; reprinted several times), Mishan brought a raised awareness to the conundrum of the increasingly wealthy yet dissatisfied First World. In trying to unravel this puzzle, Mishan looked beyond mundane figures such as levels of GDP and proposed a greater consideration for the much-neglected field of social welfare. He suggested that one factor for the sustained discontent was the 'keeping

up with the Joneses' effect. According to Mishan (1980: 704), the problem was that, 'Once subsistence levels are exceeded, the possession of more goods is neither the sole nor the chief source of men's satisfaction'. On the contrary, the acquisition of accumulative material goods would lead to deepening dissatisfaction among neighbours, resulting in a less-than-perceived increase in social welfare. Mishan advocated the idea of including the often-neglected social cost in cost–benefit analysis decisions and railed against the fallacy of 'growthmania', which regarded 'economic growth as an ultimate good in itself' (Mishan 1972: 3–8).

One popularised solution is the use of questionnaire surveys in determining social welfare. In Mishan's own words: 'Surveys based on the questionnaire method may be suspect, but they are sometimes better than guesswork, and assuredly better than no information at all' (*ibid.*: 109). Despite the implicit speculative nature of surveys, stringent adherence to proper surveying techniques would still render the resulting figures not altogether implausible, and thus valuable in a cost–benefit analysis.

Perhaps surprisingly, Mishan was at one time a research graduate at the University of Chicago, one of the world's most market-oriented economics departments. He was always circumspect about rightist, non-interventionist prescriptions for the economy and governance, and believed that there had to be checks and balances, and the 'correct' amount of government intervention.

Mishan's writings often remind readers of significant market failures resulting in welfare losses. In addition to the social malaise that may accompany a freely and seemingly unregulated competitive market system, Mishan also questioned the effectiveness of market-derived solutions in the modern world of rapidly changing technology.

2.3 Value of Statistical Life

Mishan's work on the statistical valuation of human life also made an impact. His paper, 'Evaluation of Life and Limb: A Theoretical Approach' (Mishan 1971), set out the argument that policy makers could assess the benefits of a particular measure by weighing its cost against how much it would reduce the risk of death. Such studies deriving the value of human life from people's willingness to pay—on the basis, for example, that an expressed readiness to pay £100 to reduce the risk of death by 0.1% values that individual's statistical life at £100,000—are widely used in public project evaluations. These findings are crucial to public sector investments in health care, as well as in transport projects.

When evaluating the value of a human life, it is intrinsically impracticable to establish a definite figure. After all, as Mishan puts it, ‘the value of a person’s life to himself is unlikely to be finite; no sum of money, no matter how large, will induce him to surrender his life’ (Mishan and Quah 2007: 201). Yet, in dealing with cost–benefit analyses that involve alterations in the incidences of death, disablement or disease, it is crucial to have a feasible number to base project evaluations on.

Mishan proposed that our only recourse is to calculate the pertinent values by looking at the compensatory sums required for people to assume a heightened risk of harm. Hence, for a project to be acceptable, the benefits of the project must outweigh the sum of the compensating variation of all affected individuals (see Mishan 1971). This basis for using the summation of the individual willingness to pay has become ‘the standard for calculating the value of statistical life’ (Biausque 2012: 5), the popularisation of which has been credited to Mishan, among others, although it was first formulated by Drèze (1962).

The approaches to three of the classic dilemmas of cost–benefit analysis covered in this section only reflect a very small part of Mishan’s contribution towards this mode of investigation. For his tireless contributions, Mishan has been justly recognised as a pre-eminent authority on cost–benefit analysis with ‘highly influential works that have attempted to reconcile development with the environment’ (Papadakis 1998: 313). Cost–benefit analysis might have been formalised well before Mishan’s time by Alfred Marshall (see Gamsakhurdia 2013), but it was Mishan, among other great welfare economists, who refined many aspects in the use of such analysis for applied welfare economics. This is particularly important with respect to matters of public policy.

3 Three Commonly Ignored Benefits of Immigration/Population Increases to the Existing Population

As correctly observed by Sinclair (2016: 2–3), Mishan is regarded as ‘the first academic economist of note to voice serious worries about economic growth’. Also, Mishan impressed observers as a remarkable mixture of ‘a man of the left...a thorough-going economic liberal...an uncompromising social conservative...contrarian and sage’ (ibid.: 7–8). This section addresses Mishan’s (2005, 2006, 2009) ‘uncompromising social conservative’ view against globalisation and immigration. Readers should keep in mind that this view reflects only a small part of Mishan’s wider views.

Globalisation involves many other issues, but this section will focus on the effects of immigration (and also population growth through more births, as they have some similar effects in increasing the population size) that were also a main concern for Mishan. To begin with, we concede a possible problem with immigration. If social harmony is seriously compromised due to immigration, serious challenges may arise. Thus, our discussion below applies mainly to cases, like Singapore, where harmony is largely maintained. Alternatively viewed, our analysis is confined to examining the economic effects only of immigration, which were also the main focus for Mishan.

Discussing the effects of globalisation (mainly free trade and the movement of people), Mishan (2005: 69) concluded that:

Unless the South-to-North movement of peoples, legal and illegal, can somehow be curbed, it may not be possible in Western countries to maintain over the foreseeable future current levels of employment among their indigenous workers, especially those with limited skills—at least, not without their eventual acceptance of some decline in their real wages.

This argument was repeated in Mishan (2009: Chapter/Fallacy 3.)

Mishan's argument is based on well-known basic economics. From the viewpoint of a country of the richer North, the import of labour-intensive goods, the export of capital and skill-intensive goods, the immigration of labour, especially unskilled, and the export of capital all serve to lower the marginal product of labour domestically and hence lead to unemployment and/or lower wages. However, in a comment on Mishan, Meadowcroft (2006) correctly advances some real-world factors (including the monetary and non-monetary costs of migration, higher productivities of domestic workers and the desirability of freedom) that limit the effects emphasised by Mishan. While we agree with virtually all the points noted by Meadowcroft, we believe that he missed out three of the most important factors which are themselves in opposition to the case made against immigration (as well as free trade and population increase) advanced by Mishan and many others, as discussed below.

3.1 Immigration/Increasing Population Need Not Lower Per Capita Income

The simple economic model used by Mishan and others is based on constant returns to scale, and a doubling of all inputs exactly doubles output, both at the firm and the economy level. An increase in population size either through immigration or natural increase then tends to decrease income per

head, if technology and land (or natural resources) are given, since land per head will be reduced. If immigrants bring in capital less than the domestic average capital per head, capital per head is also reduced and makes the reduction in income per head worse. Even in this unfavourable case of a reduction in income per head for the domestic economy, we argue below (Section 3.2) that immigration does not usually make the native population worse off. Here, we first argue that, in a more realistic model taking account of increasing returns (especially those at the economy level through economies of specialisation from a higher division of labour), a rise in population may not decrease income per head.

The simple economic model based on perfectly competitive firms allows some properties of a market economy to be established more simply and rigorously under ideal conditions. In particular, the efficiency of market coordination (the invisible hand) is shown as the first theorem in welfare economics (see Ng 2015 for an exposition). While providing useful insights and a benchmark, the assumption of perfect competition is very unrealistic. A perfectly competitive firm faces a horizontal demand curve for its product. A definite equilibrium requires an upward-sloping marginal cost curve. This implies that the firm in a profit-maximisation equilibrium does not want to sell any more units at the same price, as the additional costs involved will be larger than the additional revenues. We have not encountered any firm or seller behaving this way in the real world. Rather, almost universally, we see something like \$10 for one and \$16 for two, suggesting that the marginal cost is likely some 40% or more below the price, implying the existence of substantial market power or that the demand curve for the product is downward-sloping. Perfect competition rules out increasing returns at the firm level. This is so as increasing returns imply a declining average cost curve. With a horizontal demand curve, profits may then be increased indefinitely by just increasing output. However, perfect competition seldom prevails and increasing returns are omnipresent. One common source of increasing returns is the presence of significant fixed costs. Just to sell one unit, you need the size of your shop to be big enough to allow consumers to come in. Otherwise, you have to sell to ants and bees, but they do not have cash! Also, one can seldom obtain shop rentals for a period of less than one or three months. (Online commerce reduces the importance of such constraints, but not completely.) With some substantial amount of fixed costs, the average cost curve is sharply downward-sloping (especially at low output levels), giving rise to large increasing returns.

Increasing returns also prevail at the industry and economy levels. In particular, external economies between firms and within an industry, including

through the learning of skills, lead to increasing returns to scale at the industrial level, as discussed by Marshall. On the other hand, economies of specialisation from the division of labour lead to increasing returns at the economy level, as discussed by Adam Smith and others. (For more recent analyses, see Yang and Ng 1993; Giles 2016.) In addition, there may also be increasing returns at the international level when we introduce international economic relationships. Obviously, for large items like jumbo aeroplanes, being able to sell to many countries significantly reduces the average cost of production.

The negative effects on the per capita income of a larger population through a lower level of land or resources per head may be more than offset by the positive effects of increasing returns at various levels. In addition to this static effect, we may also have the dynamic effect of a higher level of technological advance with a larger population, including through the Mozart effect: the larger population size increases the number of geniuses.

Do the positive effects offset the negative effects? Conceptually, either case is possible. Obviously, if we have very excessive population density, especially if the increase in population is sudden and without enough time for a corresponding increase in infrastructure, the negative effects will likely dominate. However, there is casual empirical evidence to suggest that, for most normal cases, the positive effects dominate. Within an individual country, people in densely populated cities have higher incomes and more convenient transportation than people in sparsely populated rural areas, and that people in a much more densely populated continent (e.g. Europe) have higher incomes and more convenient amenities than people in a sparsely populated continent (e.g. Africa). The Industrial Revolution started in a densely populated area of the world (Western Europe, notably the UK).

Also, when offered a similar job with a similar salary, most people prefer one in a big city than one in a small town, despite much higher rental expenses in the former. As reported in February 2016, a medical doctor Dr. Alan Kenny in the small town of Tokoroa (less than 14,000 inhabitants) in New Zealand attempted unsuccessfully for two years to hire an assistant doctor. Despite offering a very attractive annual salary of NZ\$400,000 (around \$265,000), four working days a week, 12 weeks of annual leave and no night or weekend shifts, not a single person applied for the job, this despite Kenny contacting four medical job companies.² Also, according to

²Kenny subsequently received hundreds of applications (the vast majority of which were unsuitable) but only after the international media had reported the story.

official data of Japan released on 26 February 2016, the total population of Japan decreased by 947,000 persons compared to 2010; however, bigger Tokyo has an increase of 510,000–3,613,000. If a larger population is undesirable, why do bigger cities get bigger, and fewer people want to reside in towns and the countryside?

When the roads are congested, most people think: ‘If the number of cars or passengers were halved, how nice it would be!’ However, given the amount of taxes per head, road size and the number of buses would also be roughly halved. We are likely to have more congestion and less convenience. Ng did his first degree at the old Nanyang University in Singapore from 1962 to 1965 when the population in the city state was less than two million. If the students missed the only bus out of the campus, they had to wait half an hour. Now, with more than three times the population (5.6 million), when Ng first tried to catch the 179 bus out of what is now the Nanyang Technological University he missed two of them. He thought he had to wait at least 20 minutes. But the third 179 came in less than two minutes. Infrastructure advantages and other benefits that often accrue to larger population centres are often overlooked.

3.2 A Larger Population Benefits the Indigenous Population Economically

We argue above that a larger population may not decrease per capita income. In this subsection, it is posited that, even if a larger population serves to lower per capita income of a country/city, normal economic effects (not counting factors which may possibly cause disharmony) act to make the indigenous population better off. The basic point is that the lower per capita income of the larger population applies to the ‘newer’ population, i.e. immigrants and the newly born. If we focus on the per capita income of the indigenous population before the population increase, their incomes are actually raised by the expansion in population. For example, a country of six million persons has a per capita income of \$50,000. An increase to eight million persons may reduce the per capita income to \$48,000. However, this may be consistent with: the two million new parts of the population having a lower per capita income of \$40,000 and the indigenous population six million having a higher per capita income of \$50,670. The main reason for such a result is that immigrants cannot obtain the assets (ignoring crimes) owned by the indigenous population without paying for them. Their higher demand pushes up prices, making the indigenous population economically better off.

To see this point more simply, consider the simple textbook case of constant returns to scale, perfect competition, no external effects, no government and payment to factors of production in accordance with marginal productivity. Consider the immigration of unskilled labour without capital or any other economic ability, such as entrepreneurship, a case probably regarded as least favourable. For simplicity and concreteness, but without any significant loss of generality for this simplified case, suppose that the production function of this relevant economy is $Y=L^{1/2} K^{1/2}$, where L is unskilled labour and K is the composite of all other factors which are held constant at $K=100$. Before immigration, $L=100$, $Y=100$, and with the normalisation of one person as one unit of labour supply, the per capita income is one, with, on average, each person earning half of their unit of income from L and another half from K , and with the price/wage rate for L and K (being equal to $\partial Y/\partial L$ and $\partial Y/\partial K$, respectively) both equal to 0.5. Now introduce the immigration of ten persons each with one unit of L but no K . The total output after immigration increases from 100 to approximately 104.9, but the per capita income decreases from 1.0 to 0.9535. Have the original 100 local residents been made worse off economically? No. The marginal product of L decreases from 0.5 to 0.4876, but the marginal product of K increases from 0.5 to 0.5244. For an average local who owns one L and one K , their income increases from 1 to 1.0011. In other words, they gain from immigration. This is due to the fact that, even ignoring other possible positive factors like increasing returns, the immigration of a particular factor decreases the marginal product of this factor but increases the marginal products of complementary factors by more. Thus, the indigenous population as a whole gains economically from immigration. This is so despite a possible decrease in per capita income as this applies to all people, including new immigrants. Focusing on per capita income hides the fact that the indigenous population may gain despite a decline in per capita income calculated to include immigrants.

This gain may also be seen through the following: the ten immigrants earn the marginal product of L (MPL) when $L=110$, but their total contribution to production equals the integration of MPL from $L=100$ to $L=110$. With diminishing marginal productivity of L (as K is held constant and assumed constant returns to scale), MPL is higher at $L=100$ than at $L=110$. Thus, the total contribution to production made by the ten immigrants is higher than their total earnings. Their contribution net of their incomes must therefore be positive. The original 100 residents must benefit as a whole. If these 100 residents do not own the same amount of L and K , those who mainly or exclusively only own L may lose out, but

those who mainly or exclusively own K must gain by more. The indigenous population benefits from immigration even in this simple model with no increasing returns (discussed in Section 3.1 above) and no public goods (Section 3.3 below).

However, our simple model above does not allow for negative factors, such as congestion and pollution. Does the introduction of these factors make local residents worse off in the presence of immigration? As shown by Clarke and Ng (1993), if external costs like congestion and pollution are taxed according to marginal damage they create, even if immigration worsens levels of congestion and pollution, the indigenous population still benefits from the larger population size. However, this positive result does not apply to immigrants who rely on government subsidies that cannot be offset by their future tax liabilities.

There is a possible distributional consideration not covered above. Thus, while the indigenous population as a whole gains, owners of L may lose. If they belong to a lower income group, inequality may increase. The loss by the poor of $\$X$ may more than offset the gain of the rich of $\$2X$ in welfare terms. However, with respect to the possible concern regarding the fairness or equality of a larger population driven by immigration, one may invoke the principle of treating a dollar as a dollar on specific issues, leaving the objective of reducing inequality to the tax/transfer system as being more efficient (Pareto optimal), even taking account of the disincentive effects of the tax/transfer system (see Ng 1984). (We are not against the promotion of more equality, just in favour of using more efficient ways for such promotion so that more equality may be achieved at any given amount of efficiency cost. This does not preclude the possibility that certain measures may promote both efficiency and equality.) This is so because specific equality-oriented policies may also have disincentive effects, though popularly ignored. Moreover, though the immigration of unskilled workers into Country A may make the distribution of income within that country less equal, it actually makes distribution more equal globally.

3.3 Reducing the Per Capita Cost of Public Goods

Another often ignored economic benefit (including in the Mishan-Meadowcroft exchange) of population increase is that a larger population reduces the per capita cost of providing public goods. Extending pure public goods to more people involves negligible marginal (additional) costs. Especially for a small country like Singapore, the magnitude of these cost-sharing benefits is likely to be very big, in particular in important areas like defence, research,

innovation and broadcasting. In addition, there are impure public goods, such as education, entertainment, health and medical care, where serving twice as many people at the same quality level involves higher, but less than double, total costs. (Estimating the likely gains from a larger population for specific cases is, however, beyond the scope of this paper.)

Our discussion of the economic effects of immigration, though throwing some light on the issue, has not been sufficiently in depth to provide an answer to the general question of the desirability of immigration, even if considered just from the viewpoint of the indigenous population alone. This is because there may be negative non-economic effects that could more than offset any benefits. Obviously, if immigration leads to serious social disharmony or even outright conflict, all people involved may be made seriously worse off overall, even if per capita income increases significantly.

4 Conclusion

In conveying his anti-growth views, Mishan may have emitted the dreary vibes of a modern-day Luddite railing against technological progress. To quote him: ‘I am not one of those who believe that the original Luddites were wholly wrong’ (Mishan quoted in Greene 1971). However, a fairer evaluation of Mishan might be that of a visionary who foresaw and warned of the more far-reaching impacts of technological advances and their consequent effects on the environment and our lives.

Mishan’s unorthodox stance is especially praiseworthy as it was set against the growth zeitgeist of his time. Such a stance, unfortunately, drew considerable flak from his contemporaries. To this end, Mishan’s willingness to stand against the conventions of the mainstream in defence of his own beliefs is surely much commendable. Perhaps the most telling testament as to whether Mishan was justified in his approach is that his views continue to remain influential and are ‘a fount of inspiration’ for economists today (Sinclair 2016: 1).

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25

James Durbin (1923–2012)

Andrew Harvey and David Bartholomew

1 Introduction

James Durbin was born into a non-academic family in Widnes on 30 June 1923. ‘Jim’, as he was universally known except in the most formal circumstances, showed considerable promise at school where he excelled in most subjects, except physics. However, he developed strength in mathematics and it was in that subject that he obtained a scholarship to St. John’s College, Cambridge, which he entered in 1942. This was in the middle of the Second World War when special arrangements were in force for those eligible for war service. Jim completed five terms of residence after which he worked in the Army Operational Research Group until 1945. On the strength of this, he was awarded a ‘wartime’ BA which was subsequently

David Bartholomew died in October 2017.

This chapter is a modified version of the obituary that appeared in *Biographical Memoirs of Fellows of the British Academy* (Harvey and Bartholomew 2016). In preparing the British Academy obituary, we drew on the ET Interview of Durbin conducted by Peter Phillips in *Econometric Theory* (Phillips 1988), Siem Jan Koopman’s obituary in the *Journal of the Royal Statistical Society* (Koopman 2012), the speech of Alan Stuart at Jim’s retirement seminar at LSE in 1988 and our own recollections as Jim’s colleagues at LSE. We are grateful to the British Academy for permission to use the obituary here.

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turned into an MA in the usual Cambridge fashion. He then spent a period (1946–1947) working for the British Boot, Shoe and Allied Trades Research Association. On returning to the Department of Applied Economics, Cambridge, in 1948, it was Jim's intention to take Part II of the Economics Tripos. This would have taken two years but he was advised by his College tutor, an economics lecturer, that his time would be better spent taking the one-year Diploma in Mathematical Statistics. He therefore completed his formal university education in a little under three years which, as he later used to point out, must have been a shorter time than for any of his peers.

Jim joined the newly created Department of Applied Economics (DAE) at Cambridge in 1948. Richard Stone was Director. A good deal of research was done on time series problems with researchers such as Guy Orcutt and Don Cochrane but also with passing visitors, including Hendrik Houthakker, Larry Klein, Michael Farrell and Ted Anderson. In the summer of 1949, a young Australian researcher, Geoffrey Watson, arrived in Cambridge for a year to do some work for his PhD. He and Jim started to discuss the testing of serial correlation in regression residuals. The idea of a bound test came rather quickly and the mathematical details were developed in the following months. This work led to the famous Durbin–Watson test statistic, which had a big impact on applied econometrics because of its role in assessing the fit of an estimated regression equation.

2 LSE

The next step in Jim's career was determined when, in 1950, Sir Maurice Kendall wrote to Henry Daniels at Cambridge enquiring whether he knew of anyone suitable to fill a post of Assistant Lecturer in Statistics at LSE. With the apparent ease and speed with which such appointments were handled in those days, Jim was duly appointed. Thus began his lifelong association with the School where he rose through the ranks to become Professor in 1961 and from which he retired in 1988. In his interview with Jim, published in *Econometric Theory* in 1988, the distinguished econometrician Peter Phillips wrote: 'In 1950, Jim Durbin joined a newly established statistical research unit at LSE and, in retrospect, it is clear that his appointment broke a new dawn for the LSE ... Jim Durbin's research has had an extraordinary impact on the application of statistics' (Phillips 1988: 125–126).

Jim took his turn as Convenor (LSE's name for the rotating headship of department in those days) but he firmly held to the traditional LSE view that the professors of a department were collectively responsible and that the Convenor merely acted only with their consent and on their behalf. He had no liking or particular aptitude for administration and he did not fill any of the many managerial offices which often come the way of academics in their employing institutions nor in the many academic societies in which he took a leading part. Jim was a forceful character who was at his best as a leader or advocate of causes which were near to his heart. He often served as Council member or Chairman of committees but never as Secretary or in any other organising capacity. His strength was in advocating a course of action rather than as the patient negotiator and 'fixer'. He was not in sympathy with many of the trends in university management which had already become apparent before his retirement. He enjoyed the cut and thrust of debate on all manner of subjects, much of which took place in the LSE Senior Common Room. His own position was never left in doubt, but there was no sense of superiority or superciliousness in his forays. He would often hold forth on such things as the declining economic position of university staff as evidenced by their inability to afford various cuts of meat or the skiing holidays which were easily attainable in his younger days.

Unusually, perhaps, for someone who later travelled the world and greatly valued his international connections, Jim's academic base was firmly fixed in LSE as his domestic life was centred on his home in North London where he and his wife, Anne, brought up their three children. Jim had the physique of a sportsman and one might have envisaged him as a rugby player, but as a young man mountaineering was his interest. After marriage, he decided that this was not a suitable family activity and he turned instead to skiing. But in his late fifties, he climbed Kilimanjaro, the highest mountain in Africa. A few years later and only after three months of preparation that mainly consisted of cycling between his home in Hampstead and his work in Central London, he climbed Mont Blanc to celebrate his 60th birthday. He is alleged to have seen one of the attractions of academic life as the opportunity which it offered for such activities.

When Jim retired from LSE, after nearly thirty-nine years of active service in the Department of Statistics, a special seminar was organised for him on 15 December 1988. He became an Emeritus Professor and from 2007 he was an Honorary Professor of University College London (UCL) and a Fellow in the Centre for Microdata Methods and Practice (CeMMAP). He continued to pursue his research interest in time series and the second edition of his book with Siem Jan Koopman appeared in May 2012, shortly before he died on 23 June at the age of 88.

3 Journals, Societies and Honours

Jim carried out editorial work for a number of statistics journals. He assisted with *Biometrika* for many years, starting in 1960 as an editorial assistant, then as deputy editor in 1962 and, finally, from 1964 to 1967 as associate editor. He was on the Editorial Committee of the *International Statistical Review* from 1958 to 1962 and served as an associate editor of the *Annals of Statistics* from 1973 to 1975 and the *Journal of the Royal Statistical Society, Series B*, from 1978 to 1981.

He had a strong commitment to the Royal Statistical Society (RSS). He was also active for the Institute of Statisticians (as member of the Council, 1960–1963) before it merged with the RSS. His involvement with the RSS culminated in the award of the Society's Guy Medal in Gold in 2008 for a lifetime's achievement in statistics. (He had been awarded the Bronze Medal in 1966 and the Silver Medal in 1976.) This medal, named in honour of Sir William Guy, is 'awarded to a Fellow of the Society judged to have merited a signal mark of distinction by reason of their innovative contributions to the theory or application of statistics' (Royal Statistical Society undated). The citation read:

The Guy Medal in Gold is awarded to Professor James Durbin, FBA, for a lifetime of highly influential contributions which have given him outstanding international recognition as a leader in our field, taking particular account of his pioneering work on testing for serial correlation in regression, on estimating equations, on Brownian motion and other processes crossing curved boundaries, on goodness-of-fit tests with estimated parameters and on many aspects of time series analysis especially in areas relevant to econometrics, and also his remarkable service to the wider statistical profession on the international stage (cited in Koopman 2012: 1064).

Within the RSS, Jim held a number of offices, but his main interest and contribution were to the Research Section on whose Committee he served on three occasions before becoming its chairman in 1972–1973. He was a natural choice for President of the Society several years later in 1986–1987.

With his great interest in statistics at the international level, Jim was active in the International Statistical Institute, of which he became President in 1983. From its origins, the Institute had catered mainly for official statisticians, and many mathematical statisticians did not find it their natural home. With the growth of mathematical statistics, there was an obvious need for a focus on this aspect outside the USA. Although the Institute of Mathematical Statistics professed to be international, it was

largely seen as an American organisation and could not easily meet the needs of statisticians in other parts of the world. The vacuum was initially filled by a European region of the Institute of Mathematical Statistics; this then became a new section of the International Statistical Institute. Finally, it developed into a fully fledged European Society in 1975 called the Bernoulli Society for Mathematical Statistics and Probability. Jim was Treasurer from 1975 to 1981.

In the course of time, Jim acquired a whole string of fellowships. These included the Institute of Mathematical Statistics (1958), the American Statistical Association (1960), the Econometric Society (1967), the London School of Economics (1993) and, of course, the British Academy (2001). His somewhat belated election to the Academy probably stemmed partly from the fact that it was never entirely clear what kind of statistician he was. Was he a mathematical, economic or social statistician? At various stages of his career, a good case could have been made for any of these designations and indeed it is not clear where Jim, if pressed, would have placed himself. However, by the late 1990s it was recognised that econometricians had a firm place in the Economics Section of the British Academy and, on this basis, Jim was duly elected. In 2001, he was awarded an honorary doctorate from the National University of Tucumán, Argentina.

4 Contributions to Statistics and Econometrics

Jim's research in statistics and econometrics covered a wide range of topics, including time series analysis, sample survey methodology, goodness-of-fit tests, probability theory, simultaneous equations models and the philosophy of statistics. Many of his papers appeared in top journals, such as *Biometrika*, *Journal of the Royal Statistical Society*, *Journal of Applied Probability*, *Econometrica*, *Journal of the American Statistical Association*, *Annals of Mathematical Statistics* and *Annals of Statistics*. Although Jim is best known to academic statisticians and econometricians for his theoretical contributions, he was always concerned with the way in which statistical methods could be applied in practice and used to influence policy. His early work on testing for serial correlation was done in response to problems encountered in fitting regression models by members of Richard Stone's group at the DAE and later work on seasonal adjustment while the effects of the car seat belt law stemmed from his contacts in the civil service. More

generally, an underlying theme in much of Jim's work was the development of statistical methods that would be useful in practice. In reading his papers, one never feels that he is using mathematics just for the sake of it. It is there for a purpose because there is an important problem to be solved. Similarly, his talks and lectures did not dwell on unnecessary detail but rather tried to present the bigger picture and convey an intuitive understanding of what was going on.

As already noted, the Durbin–Watson test (see Durbin and Watson 1950, 1951) was developed at the beginning of Jim's research career when he was at the DAE. The test is designed to detect first-order serial correlation in regression residuals. If such serial correlation is found, it might be 'corrected' by modelling the error as a first-order autoregressive process, a technique that had recently been proposed by Cochrane and Orcutt who were colleagues at the DAE. Alternatively, the equation could be modified in other ways, perhaps by introducing more explanatory variables or changing the functional form. The von Neumann ratio had been developed a few years earlier to test against first-order serial correlation in a time series which, under the null hypothesis, is assumed to be normally distributed and serially uncorrelated. However, when applied to regression residuals its distribution depends on the explanatory variables used. The ingenious idea of Durbin and Watson was to develop a bounds test for which they were able to derive and tabulate the upper and lower limits for the critical values.

After joining LSE, Jim worked on sample survey theory. For example, his 1953 article for the *Journal of the Royal Statistical Society (JRSS)* (Durbin 1953) develops a general form for the estimation of sampling variances in multistate samples with unequal probabilities. He also collaborated with Alan Stuart, his closest colleague at LSE, in producing papers on experimental surveys and on rank correlations (see Durbin and Stuart 1951, 1954, respectively).

Jim returned to time series testing in 1957 with the publication of an article in *Biometrika* (Durbin 1957). In it, he adapted the Durbin–Watson test for use in a system of simultaneous equations. Such systems had only recently been introduced into econometrics and estimation by the method of two-stage least squares was not yet in widespread use. Jim's approach is based on the method of limited information maximum likelihood, which is technically more demanding, and what he did illustrated his capacity for devising a clever solution to a difficult problem. The intuition for Jim's thinking may well have come from his ability to think in geometric terms, an approach which is now somewhat out of fashion.

The Durbin–Watson test was arguably the first diagnostic statistic to be routinely used by applied economists to assess the adequacy of their regressions. Unfortunately, its success led to it being used for models where the underlying statistical assumptions did not hold. In particular, it began to be quoted for regressions containing a lagged dependent variable. Although Jim’s papers clearly excluded this situation, he felt a responsibility to put matters right and this led to his important 1970 *Econometrica* article on the h-statistic (Durbin 1970). In order to encourage its use by applied researchers, the h-statistic was presented as a modification of the Durbin–Watson statistic that could be easily calculated from standard computer output. The general principle of this test was later recognised as a Lagrange multiplier procedure.

Durbin–Watson is concerned with first-order serial correlation. One way of testing higher-order serial correlation is based on the cumulative periodogram. The cumulative periodogram, and the desire to get a distribution theory for it, provided the motivation for Jim’s work on boundary crossing problems and the weak convergence of the empirical distribution function (see Durbin 1968, 1971). It further led to the development of a limiting distribution theory of Cramér–von Mises statistics for cases where parameters are estimated. This work was joint mostly with Martin Knott of LSE and appeared as two papers in the *JRSS* (Durbin and Knott 1972; Durbin et al. 1975). However, Jim was not satisfied with the underlying weak convergence theory. He therefore developed a new theory of weak convergence of stochastic processes and published this material in his 1973 book for the Society for Industrial and Applied Mathematics (SIAM) (Durbin 1973).

Jim’s 1975 paper with Brown and Evans (Brown et al. 1975) proposed another regression diagnostic, this time for detecting structural change in a time series regression. The test statistics are based on cumulating recursive residuals or their squares, and the underlying distributional theory has features in common with that of the cumulative periodogram. They are widely used in econometrics computer packages. The recursive residuals are the standardised one-step-ahead prediction errors obtained by running a regression recursively, with each observation added one at a time. The algorithm for computing the recursive residuals turns out to be a special case of a more general filtering algorithm, known as the Kalman filter. The application and generalisation of the Kalman filter were to play a central role in Jim’s later work with Andrew Harvey and Siem Jan Koopman.

Jim’s paper on errors in variables, published in 1954 in the *Review of the International Statistical Institute* (Durbin 1954), describes the construction of a test statistic that is a basic version of an exogeneity test in econometrics.

A more general test was later developed by Jerry Hausman in a 1978 *Econometrica* article (Hausman 1978). This test, which is widely used, is now usually referred to as the Durbin–Hausman, or Durbin–Hausman–Wu, test.

In 1963, Jim gave a presentation at the European Meetings of the Econometric Society in Copenhagen that showed how the full information maximum likelihood estimator in a simultaneous system of equations can be interpreted as an instrumental variable estimator. Although Jim did not publish his results at that time—partly because he could not find the assistance in producing a numerical example—it nevertheless had an impact on the econometrics profession. The work was discussed in Edmond Malinvaud's classic textbook, *Statistical Methods of Econometrics* (Malinvaud 1966), and in important papers such as Hendry (1976). The Copenhagen paper was published 25 years later in *Econometric Theory* (Durbin 1988).

The dominant time series paradigm during the 1970s and early 1980s centred on the Box-Jenkins methodology. Jim became convinced in the mid-1980s that the methodology based on structural or unobserved components time series models was the way forward in applied work. He was very supportive of the work being carried out at LSE by one of us (Harvey). This research programme developed a complete methodological framework for unobserved components models, based on state space models and the Kalman filter. When Jim was asked by the UK Department of Transport if he was interested in carrying out a statistical investigation of the newly introduced seat belt law, he jumped at the opportunity to collaborate with Harvey in developing unobserved components models for this purpose. This led to an article in the *JRSS* describing the new methodology and a report for the Department of Transport. The seat belt law had been initially introduced for a three-year trial period and the report had a strong influence on the decision to make it permanent (see Durbin and Harvey 1985, 1986).

The seat belt project included an analysis of time series of small counts, such as the number of van drivers killed each month. It sparked an interest in Jim to develop methods for the treatment of time series with non-Gaussian features. He started to collaborate with Siem Jan Koopman, who having completed a PhD with Harvey, had moved on to become a Lecturer at LSE. The collaboration led to a series of important publications on nonlinear and non-Gaussian state space models from the mid-1990s onwards. Notable examples include the papers on importance sampling for state space models published in *Biometrika* in 1997 and the *JRSS* in 2000 (see Durbin and Koopman 1997; Koopman and Durbin 2000). Following on from this work, Jim was keen to write a book on state space methods with the

aim of promoting the unobserved components alternative to the Box-Jenkins methodology and showing how the methods could be extended to deal with nonlinear models. The book was published by Oxford University Press in 2001 (Durbin and Koopman 2001).

Although Jim's research interests had been sharply focused for most of his career, he latterly developed a broad interest in statistics and, beyond that, into science and philosophy. His reading convinced him that one could not understand the philosophical foundations of mathematics apart from the evolutionary origins of human reasoning. There is no doubt that he would have liked to have pursued these ideas further.

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26

Michio Morishima (1923–2004)

Naoki Matsuyama

1 Introduction

Michio Morishima, a mathematical economist, was born on 17 July 1923, in Osaka, Japan. He was the son of Kameji Morishima, an office employee of China Airlines, and his wife, Tatsuo Morishima (née Mikawa). Morishima spent his childhood in Kobe and was educated at Naniwa Senior High School (now Osaka University) in Osaka. Morishima then went on to Kyoto Imperial University (now Kyoto University) in 1942 to learn sociology, philosophy and economics. However, because of conscription during the Second World War, in 1944 he was forced to work for the navy as a

This chapter mainly refers to the *Collected Works of Michio Morishima*, published in Japanese in 2003–2005. The author has also used Morishima's unpublished lecture notes, which are in the possession of the Morishima Library, Citizen's College of Cultural Policy and Urban Renewal, Kyoto, Japan. In this regard, I am most grateful to Michio Morishima's wife, Yoko Morishima for her kind permission to consult the manuscripts in the Morishima Library. Moreover, she has relayed to me some valuable information about Morishima. My thanks go to Robert Cord for the invitation to contribute to this volume and to Atsushi Komine. I also wish to thank Yuichi Goto, Mizuki Tsuboi and Sir David Hendry for their helpful comments. This work was supported by JSPS KAKENHI Grant Number 15H03330, 16K17097.

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cryptographer. Even on his military base, he seemed to find time to study the books of Henri Bergson and Henri Poincaré. After the war, in 1946 he graduated from the School of Economics, Kyoto Imperial University, and enrolled in the Graduate School of Economics, Kyoto University, as a Research Fellow at the Education Ministry. At that time, Morishima started modelling the dynamic version of general equilibrium theory under the supervision of Hideo Aoyama.¹ After studying at Kyoto University, Morishima was appointed Assistant Professor at the same institution. When Morishima left the University in 1951, he became an Associate Professor at the School of Law and Economics, Osaka University.

Immediately after being appointed Professor at the Institute of Economic Research (now the Institute of Social and Economic Research) at Osaka in 1963, Morishima decided to leave again because of intense arguments with his colleagues, including Shinichi Ichimura, over the management of the Institute. Taking the advice of Joan Robinson, he eventually made up his mind to continue his research in the UK. Another reason why Morishima chose to work in the UK was his previous links with John Hicks at All Souls College, Oxford.² In 1968, Morishima secured a Visiting Professorship at the University of Essex,³ and, around two years later, with the assistance of Nicholas Kaldor and Frank Hahn, he was appointed Professor of Economics at LSE.⁴ At LSE, Morishima not only engaged in research on economics but also contributed to the establishment of the Suntory and Toyota International Centres for Economics and Related Disciplines (STICERD). In 1982, he became the Sir John Hicks Professor of Economics at LSE and retired from the University of London in 1988. In the same year, he became Emeritus Professor there, and 'in 1991 [he] received the rare distinction of an LSE honorary fellowship' (Middleton 2008). After his move to the UK in 1968, Morishima returned to Japan only occasionally and, even after his retirement, he continued to contribute to the work of STICERD.

Morishima had other notable achievements. In 1960, with Lawrence Klein, he established the *International Economic Review*, in 1965 he became the first Japanese President of the Econometric Society and in 1976 he

¹Aoyama specialised in mathematical economics and Max Weber's sociology.

²In 1956, Morishima visited Oxford as a Fellow of the Rockefeller Foundation. During his second visit in 1963–1964, Morishima's main task was to give Hicks comments on the draft of his *Capital and Growth* every week (Morishima 1991 [2014]: 38).

³In 1969, Morishima's Visiting Professorship was changed to the Keynes Visiting Professorship.

⁴When he heard that Morishima had moved to Essex, Kaldor held a party for Morishima in Cambridge. At that party, Hahn, who had been a student of Kaldor's, invited Morishima to work at LSE as a successor to Bill Phillips (Morishima 2005: 275).

received the Order of Culture from the Emperor of Japan.⁵ Furthermore, he was frequently talked about as a potential recipient of the Nobel Prize.

While Morishima became a distinguished mathematical economist, at first, he had no intention to study economics at university. Indeed, when he was a high school student, he was deeply interested in the works of a sociologist, Yasuma Takata⁶ and of a novelist, Soseki Natsume.⁷ At around the same time, he was also surprised to hear from a high school teacher that an alumnus, Shinichi Hattori,⁸ had studied mathematical economics at Kyoto Imperial University. Further, in the process of reading various books of Hajime Tanabe, a philosopher familiar with the natural sciences, Morishima began to think of trying to study under Tanabe. As part of this, he began to work towards enrolment at Kyoto Imperial University, where Tanabe, as well as Takata, had been working.

After joining the School of Economics at Kyoto Imperial University in 1942, Morishima visited Hideo Aoyama's home. Since Morishima wanted to be a teacher at a higher educational institution (Morishima 1985: 150), he sought Aoyama's advice. Aoyama, however, urged Morishima to 'read Hicks's book. Hicks is the best. His *Value and Capital* was published three years ago, and it is the book you must read as soon as possible' (Aoyama quoted in Morishima 2005: 49). Takata also advised him that 'while young, you must not study sociology. Mathematical economics lies at the centre of social sciences' (Takata quoted in *ibid.*: 430). Following their advice, Morishima started to read Hicks's *Value and Capital* and then studied Wicksell's *Lectures on Political Economy* with his friends. He also learned the fundamentals of mathematical economics at Kyoto Imperial University in the 1940s.⁹

⁵Morishima received an annual pension with the Order of Culture. He used it to establish a scholarship fund for graduate students who were studying social sciences at LSE. Incidentally, the other person who received the Order of Culture in 1976 was the distinguished film director, Akira Kurosawa.

⁶Takata studied under Shoutaro Yoneda, a pioneer of sociology in Japan. Takata's masterpiece *Power Theory of Economics* (Takata 1940 [1995]) was published in English in 1995 in part due to Morishima's efforts.

⁷Morishima was greatly impressed with the scientific theory of literature contained in Natsume Soseki's *Bungaku-Ron* and *Bungaku-Hyoron*, which presented the idea that even Japanese people could properly evaluate English literature (Morishima 2005: 37).

⁸Hattori was educated at Naniwa Senior High School and Kyoto Imperial University. Later, he was appointed Professor of Economics at Toa Dobun Shoin, China (now Aichi University, Japan). Unfortunately, he died suddenly of dysentery in 1939. Hattori is known as the first translator into Japanese of Wicksell's *Interest and Prices*. He also published papers on the dynamic version of the monetary theory of Wicksell and Myrdal.

⁹Morishima once described the School of Economics at Kyoto Imperial University as 'the oriental LSE'. In the 1940s, Takata lectured on Böhm-Bawerk and Wicksell, Aoyama delivered his supervision on Hicks, and Masazo Sono, who was a mathematician specialising in algebra, gave lectures on Slutsky and Pareto (Morishima 1991 [2014]: 43).

Thus, young Morishima, though interrupted by the Second World War, devoted all his energies to the study of mathematical economics.

This chapter focuses on Morishima's wide-ranging research interests,¹⁰ carried out after his graduation from Kyoto. The discussion proceeds in the chronological order of Morishima's life. Section 2 gives an overview of his economic methodology. Section 3 introduces three sorts of economic theory that Morishima analysed in studying the dynamic version of the general equilibrium theory at Osaka University in the 1950s and 1960s. Section 4 discusses his research in the history of economic thought at the University of Essex and LSE during the latter part of the 1960s. Section 5 reviews his work on 'symphonic economics', which began to be systematically constructed in the 1980s. Finally, Section 6 concludes with an outline Morishima's efforts to establish STICERD.

2 Morishima's Economic Methodology

Morishima (1994: 36) pointed out that 'the foundation of social sciences is to consciously use *idealtypus* and to separate value judgement from scientific reasoning'. Walras's approach has been developed exactly along these lines.¹¹ Morishima regarded Léon Walras, the founder of general equilibrium theory, as the economist who had done most to make economics a mathematical science. In the Preface of his *Éléments d'Économie Politique Pure* (henceforth, *Elements*), Walras insists that '[i]t is already perfectly clear that economics, like astronomy and mechanics, is both an empirical and a rational science' and that, in the twentieth century, 'mathematical economics will rank with the mathematical sciences of astronomy and mechanics' (Walras 1874 [1954]: 47–48). For Walras, science was an investigation into the truth of natural forces that are unintended and destined (see *ibid.*: 62). As a result, Walras referred to mathematical economics, which is a positive and rational science, as 'pure economics'. He clearly distinguished it from

¹⁰Hendry and Mizon (2000: 71) stated that 'The eclectic nature of Michio's extensive publication makes it impossible to choose any topic on which he has never written'. To the best of my knowledge, only Hirose (2006) has covered Morishima's various works, which were published in Japanese and English, in any detail so far. Thus, this chapter partly draws on Hirose's study.

¹¹As his lecture notes show, Morishima frequently taught Walras's economic methodology, at least, at the University of Essex, LSE and the University of Siena in 1968–1970.

‘applied economics’, which examines utility and profit in real societies, and from ‘social economics’, which deals with justice and fairness with respect to, for example, the nationalisation of land. Walras’s pure economics, therefore, completely ignores the idea of economic welfare; in his general equilibrium theory, Walras discusses the mathematical representation of economic transactions only when demand equals supply in goods markets, financial markets and the labour market. Morishima positively evaluated Walras’s economics: ‘Walras’s economic theory is...entirely sound and legitimate’ (Morishima 1984: 56).

Morishima’s economic methodology became crystalised after his Inaugural Lecture in 1965, when he was appointed President of the Econometric Society.¹² In his Lecture, Morishima talked about the turnpike theorem. On the very same day, however, Ragnar Frisch, one of the founders of the Society, severely criticised the turnpike theorem:

To study such economics using mathematics does us absolutely no good. When we face economic problems, we use mathematics to solve them. But, mathematical economists have a mathematical curiosity first, and they fabricate economic problems that can be adopted to such a mathematical analysis. For me, econometrics is not like that. First, econometrics faces the problem, then describes it in detail, and finally analyzes it using mathematics. Ironically speaking of the planometrics of the Soviet Union, recent econometrics is nothing but playometrics (Frisch quoted in Morishima 2005: 198–199).

Frisch’s criticism led Morishima to strongly believe that there was an order and a scope when introducing mathematical expression into the analysis of economic reality.¹³ That is, to analyse particular economic phenomena just in order to employ sophisticated mathematical formulas has no validity in economics as a branch of science. As such, Morishima kept in view an essential meaning of Frisch’s criticism from the standpoint of an outsider in mathematical economics (see *ibid.*: 199).

From the late 1960s, Morishima had been deeply concerned about unrealistic theorems and assumptions in economics. In particular, he regarded modern

¹²Morishima became a Fellow of the Econometric Society in 1958.

¹³In his *Economic Theory of Modern Society* (Morishima 1973 [2004a]), Morishima examines social and economic modernisation via the axiomatic method; he also states that, ‘the book is my answer to Frisch’s critique’ (Morishima 2005: 199).

economics, which relied on the excessive use of mathematics, as ‘suffering from anaemia’ (Morishima 1984: 65). Most economic models that adopted the general equilibrium theory undervalued the abstraction from empirical facts that must, in reality, be relevant to various institutions. Morishima further argued that ‘theorems’ in economics and ‘theorems’ in mathematics are intrinsically different.¹⁴ That is, in mathematics, ‘theorems’ mean theory, which employing axioms and definitions, show no logical inconsistency in proof. On the other hand, mathematical economics, based on subjective propositions, attempts to explain rationally and deductively the causality of an economic phenomenon. It is certain that ‘theorems’ in economics do not have universal validity in contrast to, say, Pythagoras’s Theorem.¹⁵ In addition, every market has its specific norms and rules that govern all economic actors within it. Economists need to observe carefully the historical economic and social behaviours related to particular customs and must formulate and analyse them (*ibid.*: 65). Through such an analytical process, economic theory requires that historical and institutional knowledge be combined with mathematics. Accordingly, Morishima suggested that economics should be ‘institutional-analytical’.

In later life, Morishima maintained that economic analysis should more explicitly entail the findings of sociology and historical studies. He always observed real economic phenomena carefully, explained rationally the internal structures specific to the market and analysed them econometrically. However, no matter how much the general equilibrium theory was advanced dynamically, Morishima felt that it could not capture the secular trends of actual economic movement. As a consequence, he tried inventing ‘economics as a grand integrated body of knowledge’ (Morishima 1984 [2004]: 9–10) which could capture the qualitative changes that take place in an economy and in society in an ultra-long-run framework. This could be made possible by Morishima extending his research scope into the history of economic thought and symphonic economics.

¹⁴In addition, Morishima noted that, ‘In mathematical economics, there are some conclusions of mathematical analysis that are called “theorems”, but they are misleading’ (Morishima 2005: 216).

¹⁵For instance, the value of labour is not reflected in market price and capitalists do not think that profits are maximised by the efficient substitution effect (see Morishima 1984: 63).

3 Dynamic General Equilibrium Theory¹⁶

Morishima's characteristic approach to economics in his early career can be found in his lectures. One of his seminar students looks back on Morishima's teaching at the graduate school at Osaka University in those days:

Prof. Morishima said, “the main point of [Hicks's *Value and Capital*] lies in its reliance on mathematical expressions, both in the small and in the large”. He advised us to translate every word in the textbook into a corresponding mathematical expression. And then look at the mathematical Appendix and check out whether yours is correct ... [H]e also taught us with tireless effort the process of “globally stable convergence”. Thanks to Prof. Morishima, we could understand mathematically how the convergence of the system depends on the initial conditions and analytical characteristics (Komuro 2004: 254–255).

Morishima was focusing on understanding Hicks's *Value and Capital* (1939 [1946]), looking for coherence for the most part within its appendix. His study of *Value and Capital* gave the impetus to Morishima's own magnum opus, *Dynamic Economic Theory* (Morishima 1950 [2004]), which was translated into English in 1996. As his lecture notes show, Morishima gave most of his lectures based on his research publications, notably *Dynamic Economic Theory* but also *Equilibrium, Stability, and Growth* (Morishima 1964 [2003]).

Dynamic Economic Theory, which was Morishima's first book, contained a variety of innovative ideas. According to Morishima (2005: 116), he got hold of the English edition of *Theory of Oscillations* (1949) by Andronov and Chaikin, and then, thanks to this book, succeeded in establishing stability theory in the field of economics. His theory was based on a physical approach consisting of ‘the stability theory of equilibrium’ and ‘the stability theory of motion’ (Morishima 1950 [2004]: 136).

Morishima's second book, *Equilibrium, Stability, and Growth*, is a collection of his papers on dynamic general equilibrium analysis. The book is characterised by a dynamic version of Leontief's input-output model, the extension of von Neumann's general equilibrium theory and a complete

¹⁶In order to confirm the validity of his dynamic version of general equilibrium theory, Morishima developed its econometric content as well (see Morishima 1971 [2005]). Most of his contributions to econometrics were based on collaborative works with Japanese colleagues, such as Tetsuya Nosse, Mitsuo Saito and Yasuo Murata.

proof of the turnpike theorem. All of these helped Morishima rise to world-wide fame. In what follows, while examining the aforementioned two books, I identify the characteristics of Morishima's economics prior to the year 1968 with the following concepts: structural stability of the economic system, dynamic stability of paths and the turnpike theorem.

3.1 Structural Stability of the Economic System¹⁷

Research on the stability of the economic structure, assuming an economic system changes from equilibrium (stable state) to disequilibrium (unstable state), reveals under what conditions the system reaches equilibrium again. However, 'the problem of stability conditions is literally the problem of differential equations in mathematics', and, as Morishima noted, 'in those days, the theory of nonlinear differential equations had not matured enough' (Morishima 2005: 116). Before 1950, the mathematical formulation of structural stability in economics was still problematic and, in most cases, resort was made to the stability conditions of Hicks or Samuelson. Hicks (1939 [1946]) introduced the idea of static stability conditions of equilibrium, such as 'perfectly stable' and 'imperfectly stable', in a multi-goods market. Subsequently, Samuelson, in his *Foundations of Economic Analysis* (1947), dynamically demonstrated the sufficient conditions of equilibrium. He found that Hicks's stability conditions did not explain the adjustment process of the economy. Samuelson's dynamic stability conditions showed that the linear approximation of differential equations makes it possible to prove the stability conditions of equilibrium.

In his *Dynamic Economic Theory*, Morishima pointed out that Samuelson's dynamic stability conditions of equilibrium cannot be satisfied without the assumption of the structural stability of the economic system. Morishima applied the analytical framework of Andronov and Chaikin, which adopted the Liapunov function to analyse 'structural stability', to the analysis of the principles of price determination in nonlinear markets. In this framework, the system is said to be structurally stable if and only if, after expressing the

¹⁷Investigations into structural stability had taken place in Japan prior to morishima's important contributions. aoyama attempted to develop a general equilibrium theory dynamically in 1938 and Sono investigated the stability conditions of dynamic analysis in multiple markets using oscillation theory in 1944. Also, as Weintraub (1987) notes, Takuma Yasui adopted Liapunov method in his analysis of the dynamic stability conditions of economic equilibrium in 1948 (Yasui 1948a, b). Further, Yasui provided a mathematical proof of his ideas on the dynamic stability condition of general equilibrium theory (see Yasui 1949).

nonlinear dynamical system in differential equations, the solution to these coincides qualitatively with that obtained by linearising the differential equations. Morishima, based on this idea of physics, analysed and revised Samuelson's stability conditions. In particular, he revealed that, to generalise the analytical method of Samuelson, that is, to linearise nonlinear economic phenomena, it is necessary to suppose that all nonlinear economic phenomena are structurally stable. Hence, Morishima increased the sophistication of Samuelson's conditions of structural stability of the economic system and greatly contributed to the development of dynamic general equilibrium theory.

3.2 Dynamic Stability of Paths

Having refined stability conditions in general equilibrium analysis, Morishima next focused on the transition of an economic equilibrium. As Hicks (1939 [1946]: viii) indicated, 'the economic system is always in temporary equilibrium, [it is] always more or less out of equilibrium over time'. Both Hicks and Samuelson attempted to explain this through 'comparative statics'. By contrast, Morishima emphasised the importance of the transition path of temporary equilibrium, that is, 'comparative dynamics'. Put differently, 'the theory of Samuelson cannot reveal the process in which temporary equilibrium prices fluctuate over time' (Morishima 1950 [2004]: 45). Morishima further argued that Samuelson's theory is applicable 'only when we wish to understand the fluctuation of a nominal price in an auction' (ibid.: 45). This set Morishima on to his next research area, namely the 'dynamic stability of path', that is, an examination of whether the transition between temporary equilibria generated by the *tâtonnement* process is stable.

Following Hicks's *Value and Capital*, Morishima employed the notion of a 'week'¹⁸ in his discussion. In order to describe the buying and selling of goods in the market as realistically as possible, he separated the transaction price into a 'nominal price' and a 'sales price'. As seen in Hicks's analysis, the price in the fluctuation process was commonly regarded as the nominal price generated in the process of selling and buying. On the other hand, Morishima considered the price as the sale price when the transaction takes place; the unique sale price, which is determined at the end of the week for

¹⁸Hicks defined a 'week' as the period in which prices are constant, hence achieving a temporary equilibrium within a dynamic framework (see Chapter 9 of Hicks 1939 [1946]).

each good, has to be determined based not only on information available during the week, but also on that available next week. As such, Morishima identified the price determination mechanism of the sale price that varies with the state of nature (Morishima 1950 [2004]: 103). Furthermore, by noting that the time shape of this sale price is the unstable factor, Morishima found that it was possible to posit it as a factor in explaining business fluctuations.

3.3 Turnpike Theorem¹⁹

John von Neumann's paper (1945–1946), 'A Model of General Economic Equilibrium', originally published in German in 1937, played a crucial role in Morishima's research on dynamic general equilibrium theory.²⁰ In the 1950s, Morishima attempted to expand Hicks's general equilibrium analysis. Hicks had used comparative statics, which utilised differential calculus and matrix theory to prove the law of demand. In contrast, Morishima took an algebraic approach in order to deal with the 'global change' in comparative statics. He further noticed that, in a Leontief-type framework, the economic model was divided into two types, an 'input-output model' and a 'price-value model'. As a result, final demand was exogenously determined. By adopting Walras's analytical approach, Morishima made final demand endogenous and developed a way of treating the 'input-output model' and the 'price-value model' in the same, coherent way. However, as with neo-classical growth theory, this system had shortcomings, in particular the coefficient on the depreciation rate of each capital good should have been exogenously given (see Morishima 1969 [2005]: 113). To overcome this weakness, Morishima focused on the von Neumann model, which allows for the possibility of joint production. This was because 'with [joint production], we can treat capital goods as follows; ... we can express capital goods as different goods in the various stages of depreciation' (Morishima 1964 [2003]: 157). Morishima insisted on using von Neumann's methodology because 'in order for growth models to be a realistic model, they should be a multi-sector model and properly deal with joint production' (Morishima 1969 [2005]: 118).

¹⁹This section partly draws on Komuro (2004) and Hirose (2006).

²⁰In his *Theory of Economic Growth* (Morishima 1969 [2005]: v–vi), Morishima described von Neumann's contributions to dynamic general equilibrium theory as 'von Neumann's Revolution'.

In treating joint production, the dynamic production process should be complex. For instance, von Neumann introduced a variety of assumptions: ‘constant returns (to scale)’, ‘the natural factors of production, including labour, can be expanded in unlimited quantities’, ‘All income in excess of necessities of life will be reinvested’, and ‘Consumption of goods takes place only through the processes of production which include the necessities of life consumed by workers and employees’, so that he could treat the state of balanced growth as an equilibrium (von Neumann 1945–1946: 2). These assumptions implied that ‘by letting prices and interest rates be constant inter-temporally, the level of production activity grows or decays at a constant, geometric rate’ (Morishima 1964 [2003]: 159). Morishima understood that ‘von Neumann’s theory of capital accumulation, from the formal or mathematical viewpoint, seems to be the natural extension of the dynamic input-output analysis’ (Morishima 1969 [2005]: 113). Further, it made it possible to describe ‘the long-run equilibrium state in which an economy changes its scale only, while keeping its composition constant because every economic sector interacts with each other harmonically’ (ibid.: 132). He was convinced that von Neumann’s treatment of joint production should be better appreciated.

In their *Linear Programming and Economic Analysis* (1958), Dorfman, Samuelson and Solow argued that, when the state of an economy changes from one point to another, rapid economic growth is potentially attainable if the economy is in the neighbourhood of the path of von Neumann-type balanced growth. Thus, as a theory of the path that achieves faster growth, the framework of von Neumann-type of balanced growth is referred to as the ‘Turnpike theorem’. Morishima provided a brief description of it as follows:

Suppose that we want to move from Point A to Point B. Then, we would directly go to Point B through the country road if Point B is not far. However, if Point B is distant, the most efficient way of travelling is that we first use the turnpike, then stay on the turnpike as long as possible, and finally switch over to the country road in order to arrive at Point B. Likewise, in the case of economic growth, if Point B is the final destination of the long-run growth path, we first ride on the accelerating growth path, continue to experience strong growth as long as possible, and finally adjust so that we can reach the state of the economy we wish to achieve. To grow rapidly, the composition of capital should be appropriate. Thus, we need to convert the composition at the outset so that we can obtain rapid growth. Once we do so, all we have to do is to keep the composition of capital unchanged and to increase the capital stock as soon as possible. During that period, the economy continues to grow rapidly. And finally, we alter the composition of the capital stock to attain our ideal one (Morishima 2005: 174–175).

The ‘Turnpike theorem’ can be easily explained. However, its mathematical formulation was not easy and was one of the main preoccupations of mathematical economics during the 1950s and 1960s. As noted, Dorfman, Samuelson and Solow first presented the analysis of the path of von Neumann-type balanced growth and attempted a mathematical formalisation of the turnpike theorem. However, in 1961, Morishima pointed out that their proof failed to exclude the possibility that the convergence of growth paths depends on the initial level of capital stock. While deliberately examining each possibility of non-convergence, Morishima distinguished between cases that converge and those that do not converge, and finally found what is termed ‘Morishima’s exception’. He expressed the optimum economic growth path using elementary functions rather than global differential geometry. The turnpike theorem was, therefore, accurately proved by Morishima.²¹

Morishima’s study of dynamic general equilibrium theory achieved its peak in the 1960s. After moving from Japan to the UK for his further research, Morishima re-evaluated various economic theories of the past from a modern viewpoint, amplifying the practical implications of mathematical economics, and making further original contributions.

4 The History of Economic Thought

In the UK, Morishima extended the scope and method of his economic analysis by studying the history of economic thought. The titles of his lecture series on the history of economic thought were ‘Marx in the Light of Contemporary Economic Analysis’ at the University of Essex and LSE and ‘Marx, Walras, and Keynes in the Light of Contemporary Economic Analysis’ at LSE. The goal of his lectures was to demonstrate how Say’s Law had been broken in the course of history. Based on these lectures, Morishima published three books²²: *Marx’s Economics* (Morishima 1973 [2004b]), *Walras’s Economics* (Morishima 1977 [2004]) and *Keynes’s Economics* (Morishima 1984 [2004]). In this section, while also considering Morishima’s masterpiece *Modern Economics as Philosophy* (1994), the discussion focuses on how he understood the economics of Marx, Walras and Keynes, respectively.

²¹In a lecture at MIT, Samuelson once said that ‘my theory [of the turnpike theorem] has some algebraic inaccuracies, but Morishima corrected them’ (Samuelson quoted in Komuro 2004: 255).

²²After his retirement from LSE, Morishima published *Ricardo’s Economics* in 1989. The book discusses Ricardo’s theory of differential rent as a prototype of marginal analysis. It has caused a controversy which has not yet been settled (see Fukuda 2011).

4.1 Morishima on Marx

At the University of Essex, Morishima gave his lecture series in which both ‘economics’ and ‘mathematics’ were taught interchangeably.²³ One day, he was asked by a colleague who had been studying statistics to teach ‘Marx’s economics’ for students in mathematics; he accepted the offer. As a matter of fact, just before going to the UK, Morishima had put some effort into studying Marx’s *Das Kapital*; ‘I spent the last few months in Japan studying *Kapital* before coming to the University of Essex’, adding that, ‘I could read his *Kapital* while comparing it with my own growth theory’ (Morishima 2005: 425).

To understand the relationship between general equilibrium theory and Marx’s *Kapital* in Morishima’s economics, the specific research background in Japan should be considered. For instance, prior to the Second World War, Kei Shibata²⁴ attempted to integrate the reproduction schema in Marx’s economics with the general equilibrium theory found in mathematical economics (see Shibata 1933). After the war, Nobuo Okishio,²⁵ based on his knowledge of mathematical economics, demonstrated that the tendency of the rate of profit to fall, which Marx argued in Volume III of *Kapital*, was not mathematically consistent (see Okishio 1961). His proof of this is known as Okishio’s theorem. Furthermore, Hirofumi Uzawa,²⁶ based on the discussion of the theory of capital accumulation and extended reproduction in Volume II of *Kapital*, constructed a ‘two-sector growth theory’, which became one of the core concepts of neoclassical economics (see Uzawa 1965). It was against the background of this intellectual atmosphere that Morishima constructed his interpretation of *Kapital* (see Morishima 1973 [2004b]: vi).

In the Preface to *Marx’s Economics*, Morishima stated that ‘my aim in this book is not to reproduce Marx’s arguments, but to give them rigorous

²³In that course, after students had learned linear algebra, Morishima taught them input-output theory and game theory. Or, after learning the mathematical theory of maxima and minima, the students were taught the theory of production and of consumption (see Morishima 2005: 273).

²⁴Shibata was educated at Yamaguchi Senior High School of Commerce (now Yamaguchi University) and Kyoto Imperial University. Later, he was appointed Professor of Economics at Kyoto.

²⁵Okishio was educated at Kobe College of Economics (now the University of Hyogo) and Kobe University of Economics (now Kobe University). Later, he was appointed Professor of Economics at Kobe.

²⁶Uzawa was educated at the First Higher School (now the University of Tokyo and Chiba University) and the University of Tokyo. He was later appointed Professor of Economics at the University of Chicago and at the University of Tokyo.

expression so that we can discuss Marx, just like when we discuss Walras' (ibid.: v). He further tried integrating the logic of Marx's economic analysis with general equilibrium theory by following his predecessors. Morishima thought that Marx wanted to reveal the dynamic laws of motion of capitalist society and that even the 'labour theory of value', presented in the Volume I of *Kapital*, would play a paramount role in integrating microeconomic analysis with macroeconomic analysis, such as with the two-sector model (ibid.: 29–30). Morishima noted that 'for micro analysis, Marx considered the concept of value, and the input-output analysis was for macro' ('Lecture Note for Marx's Labour Theory of Value' in 1968). Hence, he combined the discussion on enlarged reproduction, which is treated in Volume III of *Kapital*, with Leontief's input-output analysis, and finally examined the possibility of its extension to the von Neumann model.²⁷

Consequently, Morishima demonstrated that Marx was thinking about a system of dynamic general equilibrium that allows for joint production, prior to the introduction of this concept by von Neumann. However, since technological innovation, which was an engine of the capitalist economy, was exogenously given in the Marx and von Neumann's models, the model that Morishima developed was unsuccessful in making the foundation of historical materialism more realistic. Nonetheless, his approach was a remarkable contribution because he pointed out the logical continuity between Volumes I and III of *Kapital* and thereby brought out the importance of joint production in economic analysis. For Morishima, Marx was also a mathematical economist who attempted to reveal the dynamic laws of the motion of capitalism mathematically.²⁸

4.2 Morishima on Walras

In 1970, when he moved to LSE, Morishima undertook a thorough study of the economics of Walras as research in the history of economic thought rather than in mathematical economics. As part of this, he developed a new lecture series entitled 'Marx, Walras, and Keynes in the Light of

²⁷Morishima also believed that 'Marx's theory of both enlarged reproduction and reserve army of industry can be seen as a pioneering work of the saddle path of balanced growth' (Morishima 1973 [2004b]: 30).

²⁸According to Negishi (2005: 555), Morishima's *Marx's Economics* formed, together with *Equilibrium, Stability, and Growth* and *Theory of Economic Growth*, a trilogy on economic growth theory.

Contemporary Economic Analysis'. To begin with, Morishima was aware that 'recent studies on Walras's economics exclusively focused on the rigorous, mathematical formalisation of his general equilibrium theory of exchange and production. As a result, Walras's theory was completely purified' (Morishima 1977 [2004]: v). Morishima insisted that Walras's *Elements* discussed not only a system of general equilibrium but also economic methodology, criticism of Physiocracy and the theory of economic growth.

Morishima was of the view that by studying Walras's *Elements* only it would be possible to reveal the mechanism of economic progress that Walras was contemplating.²⁹ He tried to develop 'the law of price fluctuations in growing societies' by combining general equilibrium theory, presented from Chapters II to IV in the *Elements*, with capital, credit and monetary theory,³⁰ presented in Chapters V and VI. In this regard, Morishima suggested that Walras had attempted to demonstrate the qualitative changes that take place in an economy over time. Morishima focused on the following sentences in the *Elements*: 'In a progressive economy, the price of labour (wages) remaining substantially unchanged, the price of land-services (rent) will rise appreciably and the price of capital-services (the interest charge), will fall appreciably ... In a progressive economy the rate of net income will fall appreciably' (Walras 1874 [1954]: 390–391).

Morishima identified a theoretical similarity between Walras's theory of economic progress and Ricardo's dynamic law of distribution. In particular, he thought that Say's Law—supply creates its own demand—was evidence that Walras was a successor to Ricardo; general equilibrium analysis in Walras's model was based on Say's Law, which is the special characteristic of Ricardo's economics. Indeed, Say's Law led Walras to inconsistency in his economic model. While Morishima argued that 'the great contribution of Walras was that he created the general equilibrium theory which took durable aspects of capital goods into account' (Morishima 1994: 41), he also pointed out that treating capital goods as durable goods obviously contradicted the assumption of Say's Law.

Morishima noted that, when capital goods are durables, general equilibrium theory does not hold logically. This is because there are at least two markets for durable goods in a real economy. Taking motor cars as an

²⁹Although it should be noted that Walras did not develop his theory on economic progress in the *Elements*.

³⁰Morishima thought that, 'In Walras, monetary theory can exist only with economic growth theory' (Morishima 1977 [2004]: 10).

example, Morishima (1994) explained that to use a car, you can buy it or rent it. Those who want to use a car must participate in either the market for buying and selling or the market for a car rental. Suppose that the value of cars, if used for one year, depreciates by 10%. Now let P denote the price of cars, and let p be the rental price of a car. Then, the profit rate from a car rental equals $(p - 0.1P)/P$. If the profit rate from a car rental is lower than the market interest rate i , there is no entry into that business and funds remain in banks. Conversely, if the profit rate from a car rental is greater than the market interest rate i , then people will withdraw deposits from banks and participate in the car rental business. Accordingly, in order for a car rental business and a banking business to coexist in such an economy,

$$i = (p - 0.1P)/P \quad (1)$$

is the necessary condition. Solving Eq. (1) for a car rental price, p ,

$$p = (i + 0.1)P \quad (2)$$

It is thus clear that, given interest rates, there is a relationship between the price of cars P and the rental price of a car p . Nevertheless, depreciation rates of cars are determined by the physical characteristics of goods, in this case, service life. They are determined neither by the market interest rate nor by market demand and supply. Except for some special cases, in reality, the price that equilibrates two markets in which durables, such as cars, are simultaneously exchanged cannot exist. Morishima indicated such a contradiction as ‘the dilemma of durables’.

It turns out that for durable goods in the general equilibrium framework the price is, in some case, determined by the forces of demand and supply, whereas in other cases, the price does not adjust to the market sufficiently. When price does not have this adjustment factor, there is an adjustment of quantity in the real, capitalist economy. In other words, when the demand for cars is lower than its supply, the car manufacturer reduces output so that demand equals supply. When consumer demand and/or investment demand is not strong enough, a reduction in output causes an adjustment of employment. Put differently, unemployment occurs. In this way, studying Say’s Law and the dilemma of durables, Morishima pinned down a shortcoming of Walras’s general equilibrium theory. This could not have been identified by taking a mathematical approach. In fact, Morishima’s analytical view was influenced by Keynes’s economics.

4.3 Morishima on Keynes³¹

Morishima had a high regard for Keynes's work, in particular his criticism of Say's Law, which demonstrated economic disequilibrium in the form of involuntary unemployment. Morishima also believed that Keynes's analytical framework was necessary for tackling the dilemma of durables. More specifically, to gain a more realistic economic understanding of market structure, Morishima considered Keynes's framework for the mixed economy to be indispensable. The mixed economy model includes not only markets in which prices adjust flexibly, as is assumed by Hicks and Walras, but also markets in which prices are sticky, as argued by Sir John Hicks (after his knighthood in 1964) and Keynes. Morishima was deeply concerned with the analytical framework of the mixed economy in his *The Economics of Industrial Society* (Morishima 1984).³²

Later in his career, Morishima thought of the mixed economy as Keynes's economic model. As a matter of fact, when he was in the process of editing the *Collected Works of Michio Morishima*, he re-read his *The Economics of Industrial Society* and was astonished to find that 'this is actually my version of Keynes's economics!' (ibid.: v). As a result, Morishima included *The Economics of Industrial Society* within the volume entitled *Keynes's Economics* in his *Collected Works*. Following the spirit of Morishima's approach, I treat *Keynes's Economics* as being synonymous with *The Economics of Industrial Society*.

Keynes's Economics (Morishima 1984 [2004]) is the best textbook from which to learn the essential arguments of Morishima's economics and his economic methodology; the volume was, in fact, an introductory textbook for the first-year students (see ibid.: v).³³ The book includes a careful description of the basic framework which Morishima thought necessary for economic analysis. In particular, the analytical model of the mixed economy is used to analyse economies such as the UK and Japan. As basic analytical tools, the book employs not only 'the equilibrium analysis' of the price adjustment of agricultural, forest and perishable goods and minerals but also 'the full cost principle' of quantity adjustment of industrial products.

³¹It should be regarded that Morishima not only developed his interpretation of Keynes's economics, but tried evaluating the effectiveness of Keynes's proposals of fiscal policy using a macroeconomic model (Morishima 1971 [2005]).

³²Morishima once stated that *The Economics of Industrial Society* 'aimed for the analysis of such a mixed economy. If I were asked which book to leave after my death, I would choose that book' (Morishima 1991 [2014]: 48).

³³In contrast, Morishima's *Marx's Economics* and *Walras's Economics* were written for graduate students.

The book is composed of two parts. Part I discusses the fundamentals of Morishima's macroeconomic analysis and focuses on the principles of the price determination of goods in medium-sized industrial countries. As mentioned above, it deals with not only markets in which the equilibrium analysis of demand and supply is applicable, but markets in which prices are sticky and thus the full-cost principle should be applied. In the final chapter of Part I, after considering the macrodynamics of economic activity from an historical viewpoint, the volume explains the principle of exchange rate determination in the foreign exchange market.

Focusing on Say's Law and the principle of effective demand, Part II of *Keynes's Economics* explains how goods and money circulate in medium-sized industrial countries, which consists of only labourers and capitalists. Specifically, using a table of inter-industry relations, Morishima derives the principle of effective demand from an analysis of the goods market, identifying how interest rates are determined from an analysis of financial markets, and presenting a theory of unemployment through an analysis of the labour market. Finally, the book provides a discussion of fiscal and monetary policy. In order to throw light on the changes that take place in the real economy, Morishima applies his knowledge of economic history and the history of economic thought. In formulating *Keynes's Economics*, Morishima had the following goals in mind:

My own belief is that economics is not a single pure science, but a grand integral body of knowledge. Therefore, in order to gain an understanding of economic theory it is not enough merely to be conversant with the mathematical framework of the theory. There must also be some considerable knowledge of the social, institutional, and historical foundations of that theory ... At appropriate junctures in this book, therefore, pages are devoted to the explanation of economic systems and attempts made to consider matters from a historical standpoint ... I have adopted this sort of approach here in the hope of persuading the readers of this book that economics is an integrated science. It expresses my own antipathy towards the way in which theoretical economics has become no more than a mathematical skeleton (Morishima 1984 [2004]: 9–10).

For Morishima, his ideal of economics was 'institutional-analytical economics'. Through this book for beginners, he wanted to show that economics is 'the grand integral body of knowledge', which takes account of social, institutional and historical factors. Furthermore, he believed that institutional-analytical economics should be understood as 'symphonic economics', to which he devoted his later life.

5 Symphonic Economics

Morishima once said that ‘after obtaining a license in mathematical economics, I was looking for the chance to do research on subjects other than mathematical economics’ (Morishima 2005: 430–431). After 1965, he particularly focused on constructing the dynamic economic theory to explain real movements in the economy. Nonetheless, he was always aware that economics ‘had fatal weaknesses as a long-term theory’ (ibid.: 429). In order to overcome its shortcomings, Morishima believed that it was necessary not only to develop mathematical analysis but also to learn related subjects, such as sociology, history, religious studies and pedagogy. He wanted to combine the analytical views obtained from his economic research with interdisciplinary historical and sociological factors, and he believed that complex economic phenomena could not be explained by ‘solo’ disciplines like mathematical economics. Therefore, he needed to construct ‘symphonic economics’ as the grand, universal knowledge.

Symphonic economics had to theoretically describe the situation in which ordinary activity is maintained with various adjustments happening among economic, social and political organisations, beyond the economic framework of the production of wealth and distribution. Morishima created symphonic economics by referring to Marx’s historical materialism and Takata’s analytical approach.

In his *A Contribution to the Critique of Political Economy*, Marx argued that there was a basis, that is production relationships, in capitalist societies on which various ‘upper’ structures, such as politics, culture and arts, are constructed. However, Morishima was of the view that Marx did not devote enough discussion to the cooperative mechanism that exists between this basis and the upper structures.

To address this issue, Morishima adopted the third historical view of Takata, which was based on population. Specifically, population is defined as comprehensively encompassing both the number of population in the quantitative sense and the quality of population, such as national traits. Morishima went on:

Because the economy exists, population lives, and vice versa. Undoubtedly there is a correlation between the economy and population. However, when we ought to ask which is more primitive, it is clear that population is more primitive than the economy. The reason is that, although the economy cannot exist in the absence of population, population, to a certain extent, can live on their own even in the absence of the economic activity such as exchange and/or production (Morishima 2005: 441).

Population, used in Morishima's economic analysis, was an entity with ethical habits, not the individualistic, mechanistic, rational agents assumed in usual economic analysis. Throughout history, the ideology surrounding people's ethical habits has played an important role and is regarded as the definitive, idiosyncratic factor that determines the state of the economy and society. Introducing Takata's historical view, considering population as a basis, Morishima tried to formulate how the ideology generated by a population forms nations and societies while affecting the production relationship over time, in Marx's framework of the basis and the upper structure. This is the methodological foundation of Morishima's symphonic economics.

Morishima's first attempt to define symphonic economics was presented as 'A Historical Resolution of the Technological Gap: Japan and the West' published in *Economic Notes* by Banca Monte dei Paschi di Siena in 1975 (Morishima 1975). The main theme of the paper, the 'technological gap', was then extended to 'the problem of Japanese spirit with Western learning' in his Marshall Memorial Lecture, 'Ideology and Economic Activity', at the University of Cambridge in 1981. This lecture mainly discussed 'the reason why Japan, and only Japan, first became a capitalistic society in Asia' and attempted to 'present the revised view to the solution to Max Weber, in whom I was interested' (Morishima 2005: 357). Thus, Morishima wanted to highlight the ultra-long-run analysis, that is, not only of 'material modernization' in the Meiji era but also of 'spiritual modernization' after the Second World War, when Japan tried to reconstruct its economy (see *ibid.*: 358).

In 1982, his Marshall Lecture was revised and published as *Why Has Japan 'Succeeded'?* In this volume, Morishima focused his main attention on the idea of Confucianism in Japan³⁴ and its conflict with Western civilisation. In particular, after the Taika Reform in the year 645, Japan established her ideological foundations based on Confucianism, which was imported from China. After that, Japanese governments 'always encouraged nations to learn Confucianism and to cultivate their virtues such as integrity righteousness, loyalty, proper life, knowledge, filial piety' (Morishima 1982 [2004]: 52). During the period of the Tokugawa Administration (1603–1868), Japan was still governed by the elite samurai, educated in Confucianism. However, in the latter part of the nineteenth century, 'Japan was in the position that she had to always think about the overwhelming,

³⁴Morishima argued that 'in Japan as an island nation, Confucianism as it is in China could not be disseminated in Japan' (Morishima 1982 [2004]: 16) and 'as time passed, the difference between Confucianism in Japan and Confucianism in China became bigger and bigger' (*ibid.*: 19).

cultural and/or technological gap with foreign countries such as China and/or West European countries' (ibid.: 215). In this regard, Morishima insisted that, due to the lack of alternative religions to Confucianism, which identifies individualistic opportunities for human salvation in the world, Japanese people were perfectly willing to support the material prosperity of the nation. This was because Japanese people were, on the one hand, nonreligious and materialistic, and on the other, were nationalistic (see ibid.: 216).

After the Second World War, this Japanese type of Confucianism was altered to the individual's habit of devoting their working life to one company. Most big companies developed a 'Japanese Management Culture', epitomised by lifetime employment, the seniority system and the enterprise union. In the 1980s, Japan came to be known as 'Japan as Number One', which was followed by the Japanese economic miracle. It is, however, clear that the Japanese economic miracle was not based on rational economic agents, such as *homoeconomicus*, but her traditional Confucianism. Morishima concluded 'in the book [*Why Has Japan 'Succeeded'?*], I tried releasing economics from the process of *homoeconomicus*' (ibid.: 223). Put differently, 'the view of this book is that, if we unveil the process of economic development, we may be able to find the way to release economics from the assumption of *homoeconomicus*' (ibid.: 224). Symphonic economics thus had to consider human beings as ethical, conventional entities. By taking such an approach based on a population view of history, we can embark on the full-blown construction of an analytical framework which reflects real economic and social movements.

6 Conclusion

This chapter has described the academic life of Michio Morishima and his representative contributions to economics from the standpoint of his methodological transition in 1965. In closing, I would like to provide further information about his life, especially how he single-handedly made possible the establishment of STICERD at LSE.

In 1978, Morishima founded STICERD.³⁵ He wanted to create a universal research centre related to all the different disciplines being taught at LSE rather than on one discipline, such as the Institute of Economic Research at Osaka University, the focus of which was on mathematical economics.

³⁵The creation of STICERD was based on an original idea by Terence Gorman.

Morishima believed that he had to found a research institute funded from Japan in order to help resolve trade frictions between Japan and the UK, which were becoming evident in the 1970s.

Needless to say, the foundation of a research centre required a significant amount of money. Morishima focused on the 3rd G7 Summit held in London in 1977. He breakfasted with Japanese Prime Minister Takeo Fukuda and other politicians and introduced the idea of a research Institute. Morishima claimed that 'if we only continue to increase our trade surplus, Japan will be hated by other countries. So, we have to make research Institutes in many countries by partially using the surplus' (Morishima 2005: 326). However, none of the politicians that were present took Morishima's claim seriously. It became clear that he would not get financial support via this route. Morishima then visited Japan and attended meetings at the Ministry of Foreign Affairs in Tokyo. After discussions with many of the departments within the Ministry, Morishima failed to get any positive responses. He then went to the Japan Business Federation. Nihachiro Hanamura (then Chairman) advised him that the 'Federation is not capable of such a job. Do it by yourself without our cooperation' (Hanamura quoted in *ibid.*: 329). Since it was not possible to secure any help from politicians, government officials or financial interests located in Tokyo, Morishima went to Osaka, the City of merchants.

After arriving at Osaka, Morishima first visited the soft drinks company, Suntory, because the then President of the firm was Keizo Saji, an alumnus of Naniwa Senior High School. After listening to Morishima for only five minutes, Mr Saji decided to give him the conditional financial support of ¥500 million. The condition was that Morishima had to get a promise of financial support for a further ¥500 million from another company, since he was asking for ¥1 billion in total. Morishima asked what would happen if the other supporter turned out to be one of Suntory's competitors. Mr Saji simply answered, 'No big deal' (Saji quoted in *ibid.*: 330).

After that, Morishima met the then Director of the Toyota Foundation, Yujiro Hayashi, and the then President of the Toyota Motor Corporation, Eiji Toyoda. Eventually, they also decided to donate ¥500 million, thereby making STICERD a reality. For his part, Morishima continued to engage in the management of STICERD in later life.

Morishima passed away just before his 81st birthday in 2004. Today at STICERD interdisciplinary research on the social sciences continues to be carried out with vigour (see Bandiera 2015), bringing together development and growth, public economics, psychology and economics, theoretical economics, econometrics, Japanese studies, the economics of industry and political economy. The academic spirit of Morishima is here to stay.

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John Denis Sargan (1924–1996)

David F. Hendry and Peter C. B. Phillips

1 Introduction

John Denis Sargan played a key role in LSE's astonishingly rapid emergence as the world's leading centre for econometrics during the two decades from the early 1960s to the mid-1980s. This period produced an enduring legacy for the profession. Denis's theory contributions spanned much of the econometrics spectrum and included asymptotic and small-sample distributions

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of estimators and tests, as well as Edgeworth expansions, identification of parameters in models, the existence of moments of estimators, continuous time analyses, semiparametric estimation, the properties of instrumental variables (IV) and related estimators, Monte Carlo methods, numerical methods and computing, and dynamic models with autocorrelated errors that produced major insights on methodology. He also undertook important empirical studies yet devoted a great deal of his time to doctoral training. Indeed, many of his innovative ideas were implemented by his students, and he produced a cadre of brilliant and technically trained doctoral graduates. Within this active milieu of research and mentorship, Denis rose to prominence at LSE and gained worldwide attention.

Following a brief description of his life, we review Denis's major research accomplishments, the work of the students that he supervised, and the research environment at LSE that he helped to create.

2 The Life of John Denis Sargan

John Denis Sargan was born on 23 August 1924, in Doncaster, Yorkshire, where he spent his childhood. Harry, Denis's father, was the youngest of eight surviving children, who grew up on a farm and smithy in Conisburgh near Doncaster. Harry had gained a place at the local grammar school, but the family could not afford further education for him. As he had an ability to handle horses, Harry joined the Life Guards Cavalry on the outbreak of war in 1914, and when the war ended in 1918, he became a mounted policeman in Doncaster. Denis's mother, Gertrude Porter, was one of four children, and being musically gifted, she loved to sing in the parish church at Askern (near Doncaster) where her father was the organist and choirmaster.

Denis and his sister were brought up in a household where money was scarce. He attended the local Church of England primary school and then won a place at Doncaster Grammar School for his exceptional ability in mathematics. He taught himself to play the piano and enjoyed playing it throughout his life. Aged just 17, Denis won a State Scholarship to St John's College, Cambridge, attended at the same time by both Sir David Cox and Jim Durbin, albeit in different years. On completing his degree in mathematics, Denis was drafted into war work on testing new weapons systems. In 1945, he read John Maynard Keynes's *General Theory of Employment, Interest and Money* which made him decide to apply his knowledge of mathematics and statistics to tackle the many economic problems facing the UK

in the post-war years. He first returned to Cambridge to read economics, completing his BA degree in a single year and then accepted a Lectureship (Assistant Professorship) in the Economics Department at Leeds University, where he met Mary Millard whom he married in 1953.

Denis was awarded a two-year Fulbright Scholarship in 1958, so (with all the family) visited the Economics Departments of the Universities of Minnesota and Chicago, which kindled his interests in econometrics and the use of computers therein. He returned to Leeds University as a Reader (Associate Professor) in July 1960 and then moved to the London School of Economics as a Reader in Statistics in 1963 (where Jim Durbin was a Professor: see the chapter by Andrew Harvey), and in 1965, joined A.W.H. (Bill) Phillips (see the superb biography by Alan Bollard (2016) and the chapter in this volume on Phillips by James Forder) as Professor of Econometrics in the Economics Department where he remained for the rest of his career.

Even though Durbin, Phillips, Roy Allen (see the chapter by Jim Thomas), Richard Lipsey (see the chapter by Max Steuer) and Rex Bergstrom all worked at LSE at the time, and an earlier tradition around Arthur Bowley and William Beveridge (see the chapters by Adrian Darnell and Atsushi Komine respectively) was empirically centred, LSE was a surprising choice of institution for an econometrician in the early 1960s. Certainly, quantitative research was respected by the Methodology, Measurement and Testing (M2T) group (see the chapter on Richard Lipsey by Max Steuer). However, the tradition following from LSE's two most famous economists Lionel Robbins (see the chapter by Sue Howson) and Friedrich Hayek (see the chapter by Peter Boettke and Ennio Piano) was far from favourable to econometrics, and notoriously against attributing a substantive role to empirical studies, as discussed by Hendry and Morgan (1995: Chapter 4). Nevertheless, in 1965, Denis helped introduce a Master's level course in Quantitative Economics and Econometrics which set new standards for advanced teaching, creating a generation of econometricians trained to high technical levels. Many of these students went on to undertake doctoral research with him: in total Denis supervised 36 theses (including our own).

The combination of Denis's own outstanding research with the production of a cadre of brilliant and technically trained doctoral students rapidly raised LSE to be the world's leading econometrics centre during the period 1965–1985, with an enduring legacy for the profession. Other key figures in econometrics at LSE during that period included Terence Gorman, Ken Wallis, Grayham Mizon, Jim Thomas, Meghnad Desai

(see the chapter by Raja Junankar), Steve Nickell (see the chapter by Jan van Ours), Jan Magnus, Cliff Wymmer, Mary Morgan and David Hendry, many of whom were attracted to the School by Denis's presence, as were numerous distinguished visitors, including Frank Fisher, Jean-François Richard and Ted Anderson. This group was complemented by an evolving and growing phalanx of LSE doctoral students, almost all of whom were supervised by Denis, who contributed in concert a substantial group of econometrically oriented participants at LSE seminars in economics, transforming discussion and research to evidence-based analysis and econometrically informed thinking.

From 1982 to 1984, Denis was Tooke Professor of Economic Science and Statistics, and on retirement in 1984, he became Emeritus Professor of Economic Science and Statistics at the University of London. At that time, an international conference was held in his honour at Oxford University (with the proceedings published as Hendry and Wallis 1984). He died at his home in Theydon Bois, Essex, on Saturday 13 April 1996. His spouse Mary died on 1 May 2013, and their daughter Barbara died in 2015, but they are survived by their two sons, John and David, and three grandchildren.

We have many fond memories of Denis, as a teacher, a supervisor, a colleague and a giant of the discipline. He ran a workshop for PhD students, at which Hendry remembers starting a presentation on polynomial matrices when a senior professor—who had earlier remarked that econometricians were ‘failed mathematicians’—asked what they were, and Denis leapt up to take revenge and used all the rest of the time lecturing to him! Mention a technical problem to Denis at lunch and an innovative solution sketched on a slip of paper came under one's office door later that afternoon. Denis was renowned at LSE for his unequalled generosity to faculty and to doctoral students. He promptly got all his doctoral students engaged in research and sometimes ended up essentially proving the most challenging theorems of their theses. One student told us years later that he couldn't prove a key result needed for his thesis, so provided Denis with a ‘proof’ that worked carefully from the front and from the back but filled in the middle with junk. A week later Denis gave the student some handwritten notes, saying in his typical euphemistic way that he ‘didn't think the previous proof worked, so here was a new one that did’. This story typified Denis's style and approach to research, which was remarkably supportive of colleagues and students while single-mindedly pursuing his own goals in research.

3 Denis Sargan's Contributions to Econometrics

For the first two decades of his career, Denis worked on his own and sole-authored all his publications, half of them in *Econometrica*. From the early 1970s, Denis worked regularly with his students, inspiring them to take on challenging problems in a host of different fields, covering a broad swathe of the discipline of econometrics as it stood at the time. Much of Denis's later research came through his many doctoral students. After a discussion with him about potential projects when none had been well received, Denis would open a desk drawer, take out a draft of a paper and suggest you thought about that, an approach that often led to a successful thesis. In what follows, we have noted the PhDs of his students by using the notational system 'name (date, PhD)'.

Previous overviews of Denis's contributions to econometrics include the interview of him by Phillips (1985a), the laudatio by Espasa (1993), Hendry and Pesaran (2001), Ericsson et al. (2001), Hendry and Phillips (2003), Hendry (2003), Phillips (2003), and Robinson (2003), with several obituaries including Desai et al. (1997). His lectures are published as Sargan (1988a), and his collected works are published as Maasoumi (1988). Denis's research work was celebrated on his 60th birthday in Hendry and Wallis (1984). LSE contributions to econometrics, including those by Denis, are described by Jim Thomas in Chapter 1 of this volume. Recently, Stan Hurn uncovered and reconstituted some video recordings of Denis lecturing at the Australian National University: see <http://www.ncer.edu.au/resources/historical-archive.php>.

While we have slotted his research into a variety of pigeon holes, it must be remembered that many of Denis's papers spanned several of these categories. Moreover, despite the appearance of disparate contributions, there is the overriding theme of improving the quality and reliability of empirical modelling. That required Denis to venture into issues of model specification (Section 3.10), modelling methodology (Section 3.11), methods of estimation, inference and evaluation (Sections 3.5 and 3.6) in samples of the size then available for macroeconomics (Section 3.3), the theoretical properties of those methods (Sections 3.2 and 3.7, includingv their asymptotics, Section 3.1) for different types of data (Section 3.4), how to evaluate them in practice (Section 3.8), their operational implementation (Section 3.9), and the resulting findings in a wide range of empirical studies (Section 3.12).

3.1 Asymptotic Distributions

The major intellectual influence from past econometric work in Denis's early career came from the research of the Cowles Commission in the 1940s, which was largely embodied in two monographs, Koopmans (1950), and Hood and Koopmans (1953). Most of that work focused on simultaneous equations and simple time series dynamic models. Identification, estimation, computation and, to a more limited extent, testing were the main preoccupations. Given the mathematical complexity of these models in comparison with linear regression equations with fixed regressors, it was natural to develop asymptotic theories, although the short historical time series samples available to researchers at that time meant that finite-sample properties were always acknowledged to be of great importance. This intellectual background cast a long shadow over econometric research for the next few decades and empowered much of Denis's research from his early contributions on instrumental variable (IV) estimation (Sargan 1958a, 1959) in the 1950s through to his work on exact distributions and asymptotic expansions in the 1970s (discussed in Section 3.3).

The two Sargan papers on IV estimation just noted developed limit distribution theory in general cases that opened the way to an inferential framework whose legacy now includes the vast literature on generalised method of moments (GMM) estimation following Hansen (1982), including the Sargan test for overidentification (now more generally known as the J test from Hansen *ibid.*). Also foreshadowed in Denis's papers on IV is a large body of subsequent work on autocorrelated errors, intensively studied in Hendry (1970, PhD), and issues of near-unidentification that Denis later systematically investigated in his 1980 World Congress Presidential Address (Sargan 1983a) and that gave birth to work on partial and weak identification (Phillips 1989; Staiger and Stock 1997) and associated asymptotic theory that connects closely with much ongoing research in microeconometrics (overviewed by Tamer 2010). Even more general work on the asymptotic theory of estimation in the presence of autocorrelated errors was pursued in Toni Espasa (1975, PhD) and published in Espasa and Sargan (1977). That work pursued a semiparametric approach to the estimation of simultaneous equation models and is one of Denis's two contributions to the field of semiparametric estimation mentioned below (in Section 3.5).

Denis's early and later work on IV allowed for nonlinear-in-parameter estimation, where matters of computation as well as weak identification are manifest. Computational algorithms and numerical optimisation methods are important in practical work on nonlinear modelling and at LSE

they played a sustained role in Denis's research and teaching (reflected in his lectures on advanced econometrics in Sargan 1988a), as well as his thesis supervision of Grayham Mizon (1972, PhD), who developed approaches to tackle the nonlinearities intrinsic in estimating vintage-capital models (see Jerzy Sylwestrowicz 1975, PhD; Bahram Pesaran 1977, PhD; Yock Chong 1982, PhD). For estimation and inference in nonlinear models, asymptotic theory is now a universal econometric tool for guidance, informing empirical researchers about statistical properties in both correctly specified and misspecified settings. Denis's contributions, and those of his students David Hendry (1970, PhD) and Peter Phillips (1974, PhD), to IV and other estimator limit theory (Sargan 1971a, 1975a), continuous time econometrics (Section 3.4), reduced-form estimation by Esfandiar Maasoumi (1977, PhD), and large models (Sargan 1975a) all influenced subsequent work within this general avenue of asymptotic research.

A further contribution by Denis to our understanding of simultaneous equations estimation arrived with his 1964 *Econometrica* paper (Sargan 1964a), which analysed the two systems estimators three-stage least squares (3SLS) and full information maximum likelihood (FIML). This paper established the asymptotic equivalence of 3SLS and FIML, thereby confirming the asymptotic efficiency of 3SLS under the same conditions as FIML and providing empirical researchers a short iterative journey to efficient systems estimation. Related work at LSE by Jim Durbin (1988) (first presented in 1963) (see the chapter by Andrew Harvey) and, somewhat later and more generally, by Hendry (1976) showed the remarkable property of FIML as an IV estimator with appropriately designed updated instruments. This is an idea that has much more recently been vigorously pursued in work on continuous updating estimators in GMM-based econometric procedures (Hansen et al. 1996).

3.2 Identification

The theory of parametric identification for linear simultaneous equations models was first addressed rigorously in the Cowles Commission research of the 1940s, where the textbook rank and order conditions were originally developed. By the early 1970s, the subject was thought to be reasonably mature after further extensions to some nonlinear models by Fisher (1966), inclusion of system-wide restriction information by Wegge (1965), and formulation in terms of information matrix criteria by Rothenberg (1971). Yet somewhat surprisingly by that point, only Sargan (1959) seems to have

considered cases of near-unidentification and examined the possible implications for asymptotic theory failure. In that early work on IV estimation asymptotics, Denis briefly mentioned singular cases of nonlinear parameter estimation where the extremum function manifested quartic rather than quadratic behaviour in the limit, thereby producing a reduction in identification capability that manifested in a reduction of the usual $n^{1/2}$ convergence rate to $n^{1/4}$ and led to non-normal asymptotic theory. Nor do such failures of normal asymptotics seem to have been considered in statistical work, which favoured general treatments of locally asymptotic quadratic likelihoods (LeCam 1986; LeCam and Yang 1990) and generally did not treat these further complications.

Denis's first attempt (Sargan 1975a, 1988b) at a general treatment of identification appeared in discussion paper form in 1975 at LSE. Numbered as A1, this paper initiated the famous red-covered small booklet LSE discussion paper series in econometrics. The paper addressed identification in terms of a general extremum estimation framework involving the finite-sample objective function and (upon suitable normalisation) its limit function, which was permitted to have a set of 'asymptotic maxima'. This is the first appearance, to our knowledge, of the notion of set identification, on which there is now a large literature in microeconometrics, although this paper by Denis is little known and has sadly never been cited in this subsequent literature. The paper goes on to analyse the limit properties of local and global maxima of the finite-sample objective function, sufficient conditions for a unique maximum, and applies the theory to linear models with analytic restrictions, as well as simultaneous equations models.

In other work on this topic, Sargan (1981, 1983b) considered problems of identification in dynamic models with autoregressive errors, where there exists potential for common matrix polynomial factors between system and error dynamics, and explored conditions that enabled the separation of these dynamics. This subject relates to statistical and engineering research on model identification (Hannan and Deistler 1988). In doctoral thesis work at LSE, some related problems had been earlier considered in the study of solutions to matrix polynomial equations in economic models with vector autoregressive (VAR) errors by David Hendry (1970, PhD) and later Julia Campos Fernandez (1982, PhD) studied IV estimation in similar structural dynamic models with VAR errors.

Denis's World Congress Presidential Address (Sargan 1983a) to the Econometric Society also focused on identification, exploring linear models that are nonlinear in parameters where there is first order lack of identification, resulting in non-normal limit distribution theory in place of

conventional asymptotics. This was a major paper that pursued the idea and implications of near-unidentification that Denis initiated in his 1959 article. The new paper dealt with the special case of a one-dimensional rank deficiency in the first-order condition for identification, so that the immediate implications of the rank failure could be isolated in a single new parametric coordinate after transformation, with spillover effects impacting the remaining parametric coordinates. With his customary algebraic flair, Denis works out the asymptotic theory, shows some intriguing limit theory involving Cauchy-like distributional behaviour and the possibility of double minima that leads in turn to a combined estimator with ‘improved’ convergence from the coupling. The paper ends with an exploration of one of Denis’s favourite nonlinear IV models and a simulation study that shows clear evidence of the relevance of the new asymptotics. This Presidential Address is a tour de force with echoes that have persisted through to the latest work on weak identification in econometrics.

3.3 Small-Sample Properties and Edgeworth Expansions

As emphasised earlier, Denis’s work was motivated by a thematic vision of where econometric research might usefully progress in the future towards the general goal of aiding our understanding of economic phenomena. His research largely followed a path inspired by this vision, an important part of which arose from a desire to improve inferential methods in a manner that respected the relatively small samples of data being used in econometric work. Most empirical models at the time involved structural and dynamic elements, so endogenous and lagged endogenous variables featured prominently in the regressor set. These complications produce formidable difficulties in the development of exact distribution theory for econometric estimators and tests. In the early 1960s, only work by Basmann (1961) and Bergstrom (1962), the latter then at LSE, had penetrated the field, examining single equation estimates of small static simultaneous equations models with two endogenous variables. That research revealed interesting small-sample properties and distributions quite distinct from the asymptotic normal, notably in miscentering and the heavy tails associated with potential non-existence of moments. General theory in this field of research was to come more than a decade later.

In the meantime, the complications of exact theory led Denis to develop a substantial research agenda that started in the mid-1960s at LSE

concerned with asymptotic approximants obtained by Edgeworth expansions. This approach, while still of great complexity, at least enabled the consideration of estimators and test statistics in structural and dynamic models whose realism matched that of models typically used in empirical practice. The feasibility of this approach also opened the field to Denis's doctoral students, beginning with the thesis work of William Mikhail (1969, PhD) on Edgeworth expansions for IV estimators, which partly appeared in published form as Denis's first joint research paper with one of his students (Sargan and Mikhail 1971). By the mid-1970s, significantly general results on the validity and algorithmic construction of these approximants arrived in two major papers by Denis in *Econometrica* (Sargan 1975b, 1976a) dealing with expansions of asymptotically normally distributed criteria, well suited to econometric estimators and t -tests. This work was followed by a further major contribution dealing with approximants for asymptotically chi-squared criteria (Sargan 1980a). Articles by Phillips (1977a, b) opened the door to valid Edgeworth expansions in dynamic models and Denis's doctoral students worked on this topic with theses by Steve Satchell (1981, PhD) and Yiu Kuen Tse (1981, PhD) on expansions in various dynamic models, some of which appeared in a joint paper with Denis in *Econometrica* (Sargan and Satchell 1986) and in published volumes (Sargan and Tse 1981, 1988a, b). A later thesis by Ignacio Mauleón Torres (1983, PhD) dealt with extensions and applications of the expansions for asymptotic chi-squared criteria.

One significant contribution by Denis to this field of asymptotic expansions that remained unpublished for nearly two decades was his 1970 World Congress paper on Edgeworth expansions for FIML estimators in simultaneous equations models, which eventually appeared in his collected works volume (Sargan 1988c). Another late contribution, Sargan (1993a), considered alternative approaches to approximation. A further important contribution, this time to exact distribution theory, was given by Denis in an Appendix of his Walras-Bowley Lecture (Sargan 1976a) that appeared in *Econometrica*. This Appendix provided the first published attempt to find the long-sought exact distribution of the IV estimator of a single equation in a simultaneous equations model, allowing for an arbitrary number of endogenous variables and succeeding in obtaining the analytic form for the just-identified case. The completely general result for IV estimation that allowed for an arbitrary degree of overidentification as well as any number of endogenous variables was obtained by Phillips (1980) and for limited information maximum likelihood (LIML) in Phillips (1985b), completing this line of research. Later work in the 1990s proved the broad relevance of this research for practical

work by showing that the finite-sample theory delivered the correct asymptotic theory for applications in which the instruments used for estimation are weak or even irrelevant.

3.4 Continuous Time

While Bill Phillips and Rex Bergstrom were at LSE, interest emerged in the development of methods for estimating econometric models formulated in continuous time as systems of stochastic differential equations. Early work on this subject was done at the School by Phillips (1959) and Phillips and Quenouille (1960). Bergstrom (1966) later opened up the investigation of the asymptotic effects on estimation and asymptotics of the misspecification induced by non-recursive discrete approximations to the differential equation system. Subsequently, in a long paper, Sargan (1974a), that was characteristically circulated in an even longer form in 1971 (ultimately appearing in part as Sargan 1976b), Denis provided a full asymptotic development of the effects of such misspecification. An important element in Sargan's approach that has subsequently been heavily utilised in statistical research on infill asymptotics is that the misspecification asymptotics were studied by allowing the sampling interval (h) between observations to tend to zero. This artifice conveniently enabled a full set of asymptotic results to be obtained for a suite of single and multiple equation econometric estimators of the discrete approximate system.

During this period at LSE two of Sargan's doctoral students produced theses on continuous systems. First, Cliff Wymer (1970, PhD) examined higher-order systems of differential equations, applying the results to models of financial markets where data were, even at this time, observed far more frequently than macroeconomic time series. Wymer's contribution to continuous time estimation via discrete approximations also involved the substantial development of nonlinear estimation algorithms and software, which had a substantial influence on the success of the empirical programme of research on continuous time econometric models of national economies such as the model of the UK by Bergstrom and Wymer (1976). Importantly, that software included one of the first developments of computerised algebra and calculus, which facilitated use of the software for users who only needed to enter code for the specific nonlinear functions appearing in the system to be estimated. Second, Phillips (1974, PhD) developed methods and associated asymptotic theory for estimating the exact discrete model corresponding to a continuous time system, covering models with

identities, exogenous variables, and both stock and flow data. This approach was first explored in Phillips (1972) and has now become widespread in practical work, especially with financial data, modern forms using nonparametric methods (Bandi and Phillips 2003). Phillips's thesis also showed that the aliasing (identification) problem of fitting continuous systems with discretely observed data may be resolved, including close diophantine approximations to the true system, by simple exclusion and other restrictions on the coefficients of the original continuous system, some of this work appearing in Phillips (1973).

Research on continuous time modelling at LSE during this time was a morning star of the major role that continuous stochastic process econometrics later came to play in the analytic study of nonstationary time series and the emergent field of financial econometrics. The unit root and cointegration revolution (Phillips 1987; Engle and Granger 1987) forever changed the face of time series econometrics through the use of functional limit theory to stochastic processes and long-run balance among integrated processes. With the advent of ultra-high frequency data, financial econometric methods now make extensive use of infill ($h \rightarrow 0$) as well as large-span asymptotics in studying the limiting form and properties of the trajectories of financial asset prices. This vast body of subsequent research owes a debt, at least in part, to early work on continuous time econometrics originating at LSE.

3.5 Semiparametric Estimation

The first statistical work on semiparametric estimation was done by Whittle (1951), who developed the so-called Whittle likelihood (see Hannan and Deistler 1988), and by Hannan (1963), who introduced the method of spectral regression. Both developments have been influential in econometrics, but spectral regression was immediately relevant in econometrics because it offered a very general way of estimating a regression model with stationary errors by treating the error process nonparametrically, so that it was not necessary to specify (or approximate) the data generating process of the errors. Denis saw the potential of this approach and, working with Toni Espasa (1975, PhD), succeeded in extending the approach to handle simultaneous equations models in which the structural coefficient matrices were parametrically specified and the errors were treated nonparametrically as stationary processes. This work was an early econometric contribution to semiparametric methods.

In a second contribution to this topic, Denis wrote an article on large econometric model estimation (Sargan 1975a) for a symposium on large macroeconomic models of national economies and emerging work on global economic models. At the time his paper was written in the early 1970s macroeconomic models had grown to huge systems of many hundreds of equations that were continually growing in size to satisfy the demands of policy analysis and forecasting. Estimation of such systems was consequently conducted in a framework of a very high-dimensional system, with vast numbers of endogenous and (presumed) exogenous variables. Denis's first contribution was to formulate the system in operator form in terms of infinite matrices, giving an infinite dimensional structural and reduced-form system but requiring only a finite number of variables (and hence parameters) in each equation. This framework may therefore be viewed as semiparametric, as suggested in a fine exposition of the paper by Robinson (2003). In spite of the finite number of parameters in each equation, use of estimation methods such as IV involves projections on instruments and these projections have properties that depend on the infinite dimensional reduced-form system. Sargan succeeded in showing that, under certain regularity conditions, use of iterative types of IV procedures designed for large systems produced estimators which were asymptotically equivalent to the (infeasible) versions that utilised the full reduced-form system. Robinson confirmed the same finding for conventional two-stage least squares estimates and gave a central limit theory justifying inference. In addition to studying IV procedures in this paper, Denis verified a standing conjecture by Lawrence Klein concerning the data requirements for FIML being that the total number of variables in the system be less than the sample size.

This line of research on infinite dimensional systems broke entirely new ground at the time and anticipated later modelling and estimation concerns by several decades. Since the turn of the century, much attention has been given to high-dimensional modelling methods and the application of machine learning in statistical models. Denis's work on high-dimensional structural systems foreshadows some of the big data concerns that have recently occupied the statistics and econometric professions. Moreover, working on large dimensional models constituted a major departure from the mainstream of Denis's own research and illustrates his remarkable versatility, revealing a capability to fashion whatever mathematical tools might be required to address provocative new lines of research, while remaining alert to the needs of practitioners.

3.6 Instrumental Variables (IV) and Estimation Methods

Two of Denis's early contributions to econometrics, Sargan (1958a, 1959), developed IV estimation for single equations with possibly autocorrelated errors. Several special cases were already known, such as indirect least squares (see Tinbergen 1930), and there had been precursors (see Reiersøl 1941) or related methods like LIML (see Anderson and Rubin 1949), but no general treatment existed before Denis's research. His formulation included 'method of moments' estimators (see Hansen 1982), although that aspect was not emphasised. Denis then began to focus more on the small-sample distributions, as in Sargan (1964a), Sargan and Mikhail (1971) and Section 3.3, and on full-information estimators (Sargan 1964b). However, others of his papers also indirectly contributed to a more complete understanding of the properties and limitations of IV methods, especially Sargan (1964c).

Other areas Denis fostered included recovering missing data in a likelihood framework by Emmanuel Drettakis (1971, PhD), comparing estimators of seemingly unrelated regressions by Tony Hall (1976, PhD), and likelihood estimation of models with unobservable variables by Kirti Mehta (1979, PhD).

3.7 Existence of Moments

As the formulae for many econometric estimators, including IV, require inverting matrices that are not guaranteed to be non-singular for all possible values of the data, the issue arises as to whether or not they will have finite moments (means, variances, etc.) in small samples. In Sargan (1974b), Denis considers when expansions of moments as used by Nagar (1959) will be appropriate. He offers a new interpretation, which transpires to be highly relevant in Monte Carlo distribution sampling, as discussed in the next section. Although separated in time, Sargan (1978, 1988d) are closely related, the former establishing the absence of moments in small samples of reduced-form estimators derived from 3SLS and the latter showing those existed for FIML, whereas the converse held for 3SLS and FIML estimators of the parameters of the simultaneous equations representation. In his thesis, Esfandiari Maasoumi (1977, PhD) showed how to improve estimates of the 3SLS reduced-form coefficients, using data-based combined estimation, a subject that has recently received much renewed attention.

3.8 Monte Carlo Methods

Hendry and Trivedi (1972) in fact drew on inspiration from both Denis and Bill Phillips to use more sophisticated simulation methods than just distribution sampling, including antithetic variates. These had the potential to lead to more general findings that did not depend on the specific parameter values used in the simulations, and indeed immediately led to such a result for forecasting. Control variables used in Hendry and Harrison (1974) were based on an unpublished memo by Denis and these, together with the insights in Sargan (1982) on settings where finite-sample moments might not exist for the methods being simulated, led to the survey in Hendry (1984).

3.9 Numerical Methods and Computing

The complicated calculations for Sargan (1964c) required Denis to hard-wire the then available computer, and on arrival at LSE he programmed in Atlas Autocode for that machine. The small word length, little memory storage and limited speeds forced him to think carefully about the appropriate numerical methods for the estimation and tests he required, an interest that persisted. Two notable studies are those by his doctoral students Jerzy Sylwestrowicz (1975, PhD) and Yock Chong (1982, PhD), where the latter developed analytic differentiation software, but Denis's influence was far broader, extending to a large number of studies by his students responding to his encouragement to develop appropriate software, including, for example, Hendry and Srba (1980) and Sargan and Tse (1988b).

3.10 Dynamic Models and Autocorrelated Errors

Directly, and through his doctoral students, Denis advanced the analysis of dynamic models with autocorrelated errors in many key ways. His first incursion in Sargan (1953) was describing non-random time series, but his early research included economic-theory analyses in Sargan (1955, 1958b, 1961a). However, beyond the papers noted in Section 3.6 and the following subsections, perhaps the key paper is Sargan (1961b), which tackles the estimation of systems of dynamic equations with autoregressive errors, leading to David Hendry (1970, PhD). In addition, Sargan and Drettakis (1974) and Emmanuel Drettakis (1971, PhD) addressed using dynamics to improve

estimates of missing data, whereas Bahram Pesaran (1977, PhD) tackled the problems created by measurement errors in dynamic models. Denis also considered modelling rational expectations in Sargan (1993b) and John Hunter (1989, PhD), as well as specification tests for dynamic models in Sargan and Mehta (1983) and Neil Ericsson (1982, PhD). An overview is provided by Hendry, Pagan and Sargan (1984).

Although he had considered a special case of distinguishing between observable-variable dynamics and autoregressive errors in Sargan (1964c), the general case of 'common factor dynamics' (Comfac) was not published until Sargan (1980b). In its simplest form, consider the following data generation process (DGP):

$$y_t = \beta_0 + \beta_1'x_t + \beta_2y_{t-1} + v_t \quad (1)$$

where:

$$v_t = \rho v_{t-1} + \epsilon_t \quad \text{with} \quad \epsilon_t \sim \text{IN}[0, \sigma_\epsilon^2] \quad (2)$$

where $|\beta_2| < 1$, $|\rho| < 1$, x_t is a vector of n strongly exogenous variables and $\text{IN}[0, \sigma_\epsilon^2]$ denotes an independent identically distributed normal random variable with mean zero and variance σ_ϵ^2 . Then substituting (1) into (2) and rearranging:

$$y_t = (1 - \rho)\beta_0 + \beta_1'x_t - \rho\beta_1'x_{t-1} + (\beta_2 + \rho)y_{t-1} - \rho\beta_2y_{t-2} + \epsilon_t \quad (3)$$

Comparing (3) to an unrestricted dynamic relation between the same variables:

$$y_t = \gamma_0 + \gamma_1'x_t + \gamma_2'x_{t-1} + \gamma_3y_{t-1} + \gamma_4y_{t-2} + e_t \quad (4)$$

reveals that there are $2n+3$ parameters in (4) but only $n+3$ in (3) as (2) entails $\gamma_2 = -\rho\gamma_1$. Thus, although at first sight (2) appears to generalise (1), autoregressive errors are a restriction on a dynamic relation, and that restriction is testable. Despite the date on Sargan (1980b), this idea was developed by Denis during 1977 and explained in advance by Hendry and Mizon (1978) and Mizon and Hendry (1980), but its fundamental implications still had to be reiterated by Mizon (1995).

Since Bill Phillips (1966) had researched moving average errors, Hendry and Trivedi (1972) investigated how well each form (autoregressive and moving average) approximated the other, and concluded that having the correct order (say 2nd) was more important than knowing the correct form.

By the 1980s, attention in time series analysis had started to focus on non-stationarity, and in Sargan and Bhargava (1983a) and Alok Bhargava (1983, PhD), Denis reconsidered the issue of unit roots and the age-old contentious problem of model specification in levels or differences, the latter being (for example) the special case of (3) above when $\rho=1$. Antecedents in that debate included Hooker (1901), Smith (1926) and Granger and Newbold (1974), with a key clarification of the last of these in Phillips (1986). As ever, the multifaceted Sargan (1964c) plays a central role by introducing empirical models of what are now called equilibrium correction mechanisms (EqCMs), where past real wages deviating from productivity fed into determining changes in wages in the next period. This class of models was a precursor to formal cointegration analysis and redirected the debate to one of the significance or not of the relevant EqCM in a model otherwise in differences: for a recent reappraisal, see Castle and Hendry (2017). Returning to (4) above, reparametrize it as:

$$\Delta y_t = \gamma_1' \Delta x_t - \gamma_4 \Delta y_{t-1} + \lambda (y_{t-1} - \kappa_0 - \kappa_1' x_{t-1}) + e_t \quad (5)$$

where $(y_{t-1} - \kappa_0 - \kappa_1' x_{t-1})$ is the EqCM, with $\lambda = (\gamma_3 + \gamma_4 - 1) \neq 0$, $\kappa_0 = -\gamma_0/\lambda$, and $\kappa_1 = (\gamma_1 + \gamma_2)/\lambda$ where the κ_i represent the ‘long-run’ effects, and would correspond to a cointegrating relation when the process determining $\{x_t\}$ was integrated of first order, $I(1)$. Conversely, when $\lambda = 0$ in (5), the equation reduces to one in first differences of the variables. However, tests of $\lambda = 0$ are non-standard although appropriate critical values have been tabulated by Ericsson and MacKinnon (2002), and are often coded in econometric modelling software.

The possibility of a root on the unit circle in a moving average was investigated by Sargan and Bhargava (1983b). Another source of non-stationarity from time-varying parameters had also been investigated by Denis’s students in Michael Fitzpatrick (1976, PhD) and Louisa Franzini-Bhargava (1983, PhD). Panel data dynamic models were also considered, as in Manuel Arellano Gonzalez (1985, PhD), relating back to differencing equation specifications.

3.11 Econometric Methodology

Denis made major contributions to the methodology of econometrics with many lasting insights. The first was in Sargan (1957) which discussed three methodological issues that concerned him about Fisher (1956): (i) that economic theory was highly abstract despite economic data being

complicated; (ii) all too often this led to over-simplified regression models which probably excluded substantively important variables; and (iii) the interpretation of tests was doubtful when large numbers of hypotheses had been investigated. Both Sargan (2001a), which describes Denis's insights into model selection, and Sargan (2001b), which contributes to understanding and avoiding what is often called 'data mining'—namely finding adventitiously significant coefficients due to testing many effects at inappropriate significance levels—followed up his worries in 1957 about that last issue, although these two papers were only published long after being written.

As Hendry (2003) explains, however, Sargan (1964c) changed existing approaches substantively in a valuable direction. As noted above, in that paper Denis introduced early forms of both Comfac and EqCMs, and based the formulation of the latter on an economic analysis of long-run behaviour, as well as developing misspecification tests applicable to dynamic equations and providing a way of comparing linear with log-linear formulations. He formulated an iterative algorithm to estimate the parameters of the resulting specifications—using a nonlinear-in-parameters IV approach designed to counteract the adverse effects of data measurement errors—and proved that it would converge with near certainty, all embodied in a computer program he also developed. His test for instrument validity has been widely used. These developments fostered other innovations at LSE (discussed in the chapter in this volume by Jim Thomas), but here we note the emergence of an emphasis on general-to-specific modelling (see Mizon 1977; Davidson et al. 1978), since extensively developed (see Hendry and Doornik 2014) with rigorous diagnostic testing (Meghnad Desai described AUTOREG—see Hendry and Srba (1980)—as destroying pet theories), and the associated requirement of encompassing (see Mizon 1984; Mizon and Richard 1986; Bontemps and Mizon 2008).

3.12 Empirical Studies

Denis's eclectic interests led to a wide range of empirical analyses, including several prior to his arrival at LSE in M.M. El Imam (1957, PhD), J. Hortala-Arau (1966, PhD) and Madan Handa (1968, PhD). Once he was settled at the School, a veritable flood of work appeared, including three studies of demand systems in Ray Byron (1968, PhD), Julia Hebden (1974, PhD) and Ranjan Ray (1977, PhD) all with distinct theoretical orientations, as well as Ross Williams (1969, PhD) modelling the demand for consumer

durables. There were also analyses of inventory and investment behaviour by Pravin Trivedi (1969, PhD) and Robin Rowley (1969, PhD) respectively and of the demand for imports by Michael Feiner (1970, PhD). There were three studies estimating production functions by Eleftherios Charatsis (1970, PhD), Sargan (1971b), which is the origin of the trans-log production function, and Mizon (1972). There was even a study of the world rubber industry by K.C. Cheong (1972, PhD).

A major empirical interest of Denis's that lasted throughout much of his career was the econometric modelling of wages and prices in the UK. Again, Sargan (1964c) represents an important development, where his empirical wage-price model broke new ground and provided new insights into their joint determination. That research was followed by studies in Keith Vernon (1970, PhD), Sargan (1971c), Toni Espasa (1975, PhD), and the companion papers Sargan (1980c, d). His approach provided a marked contrast to the nominal wage model in Phillips (1958), and Hendry remembers vivid discussions about the appropriate specification between these two giants (Bill Phillips and Denis Sargan) in the Quantitative Economics module of the LSE MSc in 1966–1967.

4 Conclusion

Denis Sargan was remarkable for what he achieved, but even more remarkable for what he was capable of doing, even if he did not get around to doing it. No problem seemed too difficult for him to conceptualise and, once conceptualised, to solve or to create approximants that opened up new understandings. His track record of research is replete with examples. One that is still unappreciated is that, while many of the world's leading statisticians were working on constructing general theories of locally asymptotically normal large-sample theory, Denis was tackling the finite-sample properties of FIML. He did this not just with asymptotic approximations but with refreshingly original insights into exact small-sample properties. In short, undaunted by technical difficulty, his creative mind saw nothing as impossible.

Denis was almost unique in the profession at the time for his extraordinary intellectual power, his technical insight, his originality and his prescience regarding productive research directions. Standing on the frontier of the discipline in the 1950s, he saw the massive untapped potential of IV. In the early 1960s, he carved a new path forward in empirical research with a boldly innovative approach to dynamic specification in an econometric methodology that gave birth to the 'LSE approach' to modelling. In the late

1960s and 1970s, his work on the finite-sample properties of econometric estimators took the subject to previously unimagined heights of generality. The mid-1970s and 1980s brought forth studies of identification that explored entirely new regions of nearly unidentified models that have subsequently opened up an industry of research.

Resonating through all his research and the advanced mathematics that he used with such detached confidence in its deployment was Denis's deep concern to develop econometric methods for the betterment of empirical research. Denis Sargan's primary gift to the profession is as a consummate toolmaker. He was peerless in his time at creating econometric methods. His lasting legacy is the benefit that these methods have since bestowed on the econometric community. That achievement is buttressed by the huge effort he devoted to bringing forward a generation of technically trained doctoral students who further advanced his approach, often starting from one of Denis's original insights.

Writing this essay has given the authors a further opportunity to reflect on Denis's genius, his generosity to his students (not least ourselves), his colleagues and his remarkable contribution to creating the unique habitat for econometrics that existed under his aegis at LSE over two decades from the mid-1960s. In conjunction with Denis's talents, it was this habitat for scholars that propelled LSE to extraordinary heights, putting it by 1980 at the forefront of the global expansion of econometrics.

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28

Ralph Turvey (1927–2012)

Roger Middleton

1 Introduction¹

Ralph Turvey combined an unorthodox intellect with a conviction that economic theory should serve practical ends. He graduated with a First in the BSc(Econ) from LSE in 1947,² having produced finals papers which became legendary.³ Indeed, Lionel Robbins considered him ‘a young man of quite outstanding attainments...[even] before graduation’ (Howson 2011:

¹I would like to thank Nicholas Turvey for providing me with a copy of Anon (2012a), which assisted greatly my writing his father’s entry for the Oxford Dictionary of National Biography (Middleton 2016), and for answering subsequent queries; Professor Jon Stern for detail on the Regulation Initiative (RI); and Dr. Aled Davies for compensating for my locational disadvantage with research assistance.

²Howson (2011: 653) notes that Turvey ‘had come to LSE in October 1944 as a second year student, having passed the Intermediate examination externally, so that by 1946/7 he was effectively a [post] graduate student’.

³Maurice Peston, who matriculated in 1949, said in his obituary of Turvey (Peston 2012: 32): ‘In the immediate post-war period, Ralph was regarded as the most brilliant LSE undergraduate. When I arrived there as a student not many years later, he was already a legend, not least because of his final examination papers. Each of his answers was set out on less than one side of a sheet of paper. Richard Sayers, the professor of money and banking, told me that the examiners did not see how they could possibly award him a first. It then dawned on them that his answers were, in fact, perfect’. John Dunning (2009: 58), a research student at UCL who also attended LSE lectures in 1951/1952, said of Turvey at this period that ‘most students believed [he] was born clutching an economics textbook in his hand – an advanced one at that!’.

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653). Awarded a DSc by LSE in 1971, Turvey was of the last generation of British academic economists who did not routinely undertake a PhD and who, in his own words, ‘Started as [an] economic theorist in the days when it was possible to keep up with everything in economics’, only later specialising as his discipline changed and he found his comparative advantage in the ‘applied aspects of efficient resource allocation’ where he ‘endeavoured to combine economic analysis with industry-specific knowledge’ (Blaug 1999: 1117). He was ‘a classic British intellectual of his era...[who] did not suffer fools lightly, and had a nice dry way of showing it, without ever displaying malice or opportunism’ (Anon 2012a). As Shepherd (1973: 680) observed in a review of two of Turvey’s most influential books (Turvey 1968a, 1971a), he provided powerful demonstrations ‘that theory can be both clear in content and useful in hard-pressed practical situations’, though he was clear that it was ‘The job of the politician...to take decisions for which no rational foundation is possible’ (Anon 2012b: 29).

Turvey was born on 1 May 1927, in Birmingham and died in Bishopstone near Salisbury on 7 April 2012. His parents were Quakers, his father an engineer, and he was educated at the Quaker Sidcot School, Somerset, before matriculating at LSE in 1944 (at that point still in its wartime residence at Peterhouse, Cambridge). As per the cosmopolitanism of LSE, Turvey spent the summer of 1946 in Sweden as a praktikant, during which he initiated a meeting with Erik Lindahl,⁴ a leading member of the Stockholm School and foremost expert in public finance. Returning to Uppsala as a postgraduate for the 1947/1948 academic year, he acquired fluency in Swedish and great expertise in Swedish economics.⁵ This Swedish connection would bear sustained fruit, influencing Turvey’s research agenda for a decade or more, and leading him to undertake translations of key Swedish texts for the International Economic Association (IEA).⁶ On returning to LSE, he was appointed an Assistant Lecturer in 1948 and became a Reader (with special reference to public finance) in 1951,

⁴Turvey would contribute to Lindahl’s Festschrift (Turvey 1956a) and an appreciation for *Ekonomisk Tidskrift* (Turvey 1960a). Since 1976 the latter has been the *Scandinavian Journal of Economics*, such title being used for all references at the end of this chapter.

⁵Turvey (1960a: 5–6) covers this period and is the nearest we have to a published autobiographical fragment.

⁶Between 1950–67 the IEA published twelve volumes of English translations of important foreign journal articles. As Haberler said in the Foreword to the first (Peacock et al. 1951: v): ‘In the field of economics the dissemination of new ideas and theories is seriously retarded by the lack of ability, or at least facility, in reading contributions in foreign languages’ with ‘Economists in English-speaking countries...especially guilty of what is going on in other languages’. Turvey was one of the editors for the first ten volumes.

by which point he had already published in *Economica* and the *Economic Journal*, made a significant contribution to Baumol (1951) on economic dynamics, and edited and translated his first book, *Wages Policy Under Full Employment* (Turvey 1952), with contributions by Erik Lundberg and others. He was also ‘at that time the youngest Reader in Economics by far’, being in age comparable to many of the returning servicemen becoming LSE undergraduates (Alford 2009: 200).

As Peston (2012) noted in his obituary, Turvey was an ‘economist with many careers’. From the early 1950s onwards, he augmented his broad theoretical interests (publishing two well-received books, *The Economics of Real Property* in 1957, and *Interest Rates and Asset Prices* in 1960), with more microeconomic, policy-oriented research. He also loosened his ties with, though in reality never really left, LSE. He spent part of 1953 and the academic year 1958/1959 in the USA as a Visiting Professor (at Johns Hopkins and Chicago respectively), but then in the 1960s shifted direction, being seconded first to the Treasury’s Economic Section (1960–1962) and then to the Center of Planning and Economic Research in Athens (1963), where he studied Greek taxation, publishing *Break and Turvey* (1964). He next served as Chief Economist at the Electricity Council (1964–1967) and was a member of the National Board for Prices and Incomes (NBPI) between 1967–1971, becoming Deputy Chairman between September 1968 and March 1971.⁷ At the Treasury his work was varied but included taxation and cost–benefit analysis of a Channel Tunnel; at the Electricity Council, he was important in recasting pricing along long-run marginal cost lines (which, for such a highly capital-intensive sector, needed to incorporate capital costs), this finding expression in the 1967 White Paper on the Nationalized Industries (HMSO 1967); and, at the NBPI, he deepened his analysis of optimal pricing on an economy-wide basis. The publication of his *Optimal Pricing and Investment in Electricity Supply* (Turvey 1968a) and *Economic Analysis and Public Enterprises* (Turvey 1971a) confirmed Turvey’s reputation among academics and policy makers, one reviewer writing of his ‘prismatically lucid treatment of economic criteria for public enterprises...[which made him] required reading for everyone in the field’ (Shepherd 1973: 679). Noteworthy also from this period was Turvey’s most cited paper,⁸ ‘Cost-Benefit Analysis: A Survey’ (Prest and Turvey 1965).

⁷Initially, Joint Deputy Chairman, 1 September 1968–31 October 1969 (Fels 1972: Appendix A).

⁸A Google Scholar search conducted on 5 April 2017 has 1008 cites, followed by Turvey (1963a) at 405 and Turvey and Anderson (1977) at 312.

In the 1970s, Turvey published a widely adopted introductory textbook (*Demand and Supply*, Turvey 1971b) and shifted employment sector, first into private consultancy (as economic adviser with Scientific Control Systems, 1971–1975; now known as Scicon)⁹ and then became an Economic Adviser and subsequently Chief of the Department of Labour Information and Statistics at the International Labour Office (ILO, 1975–1989), during which time he transformed international conventions for both labour market statistics and consumer price indices (CPI). Expertise in the latter led to roles on CPI advisory Councils in Britain, Canada, Sweden and New Zealand; it also provided an enduring interest in retirement as he pursued long-standing historical interests in market failure and the economic history of London, using his LSE base to publish on such disparate topics as eighteenth- and nineteenth-century London refuse and the cost of living.¹⁰ In retirement, he was also much involved with various economic think tanks, and in particular served as Chairman of the University of Bath's Centre for the Study of Regulated Industries, from which, with colleagues, he continued to make significant contributions, notably Burns et al. (1998) on the behaviour of the firm under alternative regulatory constraints.

Turvey's publication record amounted to at least 150 items: eight books, of which three were joint- or multi-authored; four edited books and a further 10 edited volumes for the IEA; at least 75 papers; an unknown number of contributions to conference volumes; at least 34 book reviews; and countless reports for overseas governments and other agencies, some of which were published in full (e.g. Turvey and Cook 1974) or in Turvey and Anderson (1977) which included a number of World Bank-financed case studies in electricity economics (see *An Inflationary Interlude and Then Back to Public Enterprise* below). While precise categorisation is difficult, for convenience, we have organised Turvey's work into macroeconomics (Section 2), welfare economics (Section 3) and, beginning with the ILO, his later career (Section 4). This chimes with his own description in Blaug (1999), cited earlier (see 'Introduction'), but also maps well to the second and third parts of his career which were in economic advice, public and private sector, domestic and international. Above all this is a career best considered in chronological sequence.

⁹Turvey was on the shortlist to become the first Director of the Central Policy Review Staff established within the Cabinet Office in 1971 and disbanded in 1983 (Agar 2011: 222). This probably prompted his move into the private sector as the Turvey family remember his complaint about 'excessive political interference in government appointments' (e-mail from Nick Turvey, 13 March 2014).

¹⁰The LSE Library contains a collection of these pieces (Turvey 2008).

2 Macroeconomics

In the immediate post-war years LSE was not a stronghold of Keynesian economics. Peacock (2010: 544–545), who arrived at LSE in 1948, reflected much later that it was ‘more open to ideas on macroeconomic policy devised by the Stockholm School and to criticisms from Continental Europe more directly entertained by Hayek...and Robbins than by Meade and Paish, all of them both open-minded and approachable’, such that ‘Out of the fiery discussion of Keynesianism there emerged a fiscal model of varying degrees of sophistication that owed a great deal to Netherland and Scandinavian writings on the logic of economic policy’. Here, fresh from Sweden, Turvey was to be key in bringing a Walrasian influence to emerging LSE macroeconomics,¹¹ with even undergraduates aware that Swedish economics mattered (Corry 1997: 179).

Turvey’s first papers, published in *Ekonomisk Tidskrift* and *Economica*, were on the inflationary gap and the multiplier (Turvey 1948a, b; 1949a, b). All applied the Stockholm approach. They also displayed what would become characteristic of all of his work: a focus on current policy issues, with sensitivity to their political economy and with theory as the handmaiden of policy. An early willingness to collaborate was also evident in a major contribution to Baumol (1951)—indeed, so much so that in the second edition, Baumol thought Turvey ought to have been acknowledged as a joint author (Baumol 1970: ix)—and in Turvey and Brems (1951).¹² Much of this work took as its foundation Lindahl’s (1939) period analysis, being attempts to work through microfoundations and to achieve clarity about concepts and accounting identities with appropriate attention to expectations and the classification of equilibria. This decomposition of macroeconomic concepts to give complex systems of lags and functions meshed with Bill Phillips’s (1950) early work on stabilisation policy and physical modelling in economics at LSE (the first Phillips Machine was completed in September 1950) and, elsewhere, Goodwin (1949: 553) on the multiplier

¹¹Peacock (1982) has written further on LSE economics between 1948 and 1956. See also De Marchi (1988), Lipsey (2000), Alford (2009), Howson (2011: especially Chapters 19–20), Forder (2014), and Bollard (2016: Chapters 6–8).

¹²In the Preface to the first edition, Baumol recorded that Turvey wrote most of Chapter 8 (‘Period Analysis’), the appendix to Chapter 5 (‘Uncertainty and the Equilibrium of the Firm’) and ‘made his influence felt throughout the volume’ (Baumol 1970: xi). In the Preface to the second edition, he began by ‘correcting a fundamental omission from the...First Edition’, that Turvey was not listed as joint author, and noted that ‘I shall not venture to speculate whether his categorical refusal represented excessive modesty or an act of judicious criticism’ (ibid.: ix).

Table 1 Turvey (1951a): four types of inflationary process

Type	Prices	Wages
A	Cost-determined	Cost-determined
B	Flexible	Cost-determined
C	Cost-determined	Flexible
D	Flexible	Flexible

Source: Turvey (1951a: 533)

as a matrix which had the powerful policy conclusion that, with respect to the ‘income flow in a society’, ‘there is no *one* lag but many, endlessly compounded’ (italics in original). In his most developed paper on the multiplier (Turvey 1953a), he followed the contemporary Metzler (1948) formulation of three lags (Income → Spending; Spending → Production; Production → Income) but arrived at a similar position to Goodwin with his insistence (pp. 294–295) on the multiplicity of goods and factor markets.

LSE was to be an important locale for the generalisation and elaboration of the Keynesian system in the post-war period. In the late 1940s and early 1950s, tight labour markets, price-income and other retained wartime controls set a very particular institutional context for this work, and especially for someone like Turvey who always had to the fore policy relevance. In Turvey (1951a), he applied his insights into how markets actually operate to elaborate four different types of inflation (Table 1). These, in turn, underpinned his earlier thesis that ‘Definition and analysis of inflation in terms solely of the Inflationary Gap is inadequate’ (ibid.: 534) but also led him to revise his earlier conclusion in Turvey (1949b) that a fall in employment necessarily involved deflation (see Turvey 1951a: 540, fn. 1). What mattered was how prices and wages were determined in practice, and here, as in later work, Turvey was very much aware of how the new full employment commitment would influence wage-setting. Cost-price sequencing was given considerable emphasis, as was—with some novelty—the role of productivity, but additionally Turvey developed the idea, as expressed by Forder (2014: 113–114) in his exhaustive study of the Phillips curve, that ‘inflation – and cost-push inflation particularly – could be seen as the outcome of the incompatibility of various social groups’ aspirations as to income’.¹³ Turvey thus made an important contribution to what would later become the demand-pull/cost-push

¹³Turvey (1951a: 534–535; italics in original) defined inflation as the ‘*process resulting from competition in attempting to maintain total real income, total real expenditure and/or total output at a level which has become physically impossible or attempting to increase any of them to a level which is physically impossible*’.

paradigm for modelling inflation. Inflation as a distributional struggle was also something that a decade later would have much day-to-day resonance once he was at the NBPI.

The conditions for wage stability were the central concern of the collection *Wages Policy Under Full Employment* (Turvey 1952) which brought Scandinavian approaches to an English-speaking audience: one that emphasised distributional concerns but also the virtues of the price mechanism and the potential disbenefits of detailed government controls. Unsurprisingly, this was reviewed very positively by *The Economist* (1952), but also by academic economics journals. Turvey's macroeconomic writings in the first half of the 1950s also began to range more widely; indeed, it would be inaccurate to describe him as a macroeconomist. Such purist labels were, in any case, anachronistic for LSE at this time—Robbins's Wednesday afternoon general economics seminar exemplified the School at this point—and, in practice, in his teaching, he was as committed to microeconomics, notably the first-year microeconomics lectures (Lipsey 2000: 120). A comment is also appropriate here about methodology. Turvey's hallmark was clear writing and not formulism; his style was discursive, there was little algebra, scant empirical material, and almost no econometrics,¹⁴ though he did have a very good eye for penning an instructive diagram. Unsurprisingly, he was not to be one of the founders of the M²T Seminar, though he was an occasional attendee (De Marchi 1988: 155). Turvey's general position is well described in the opening paragraph of Turvey (1960b: 13), that theory should meet three requirements¹⁵:

[I]ts relation to ordinary economic theory should be apparent, it should be simple and, finally, it should be directly relevant to the real world. More specifically, it should be based on the same sort of assumptions as other parts of economic theory; it should contain relatively few variables and it should be testable.

Beginning in the early 1950s, Turvey worked on the theory of interest (Turvey 1951b, 1954), culminating in his short book, *Interest Rates*

¹⁴In the Preface to Turvey (1960b), he acknowledges the electronic digital stored-program computer UNIVAC as having done the computation for Table V which reported multiple correlation results.

¹⁵In closing, Turvey (1960b: 107)—who is here citing Baumol's (1959: 1) strictures on methodology—noted: 'I have endeavoured to set out a simple model which is suitable for incorporation in textbooks. The approach followed is a simple and common-sense one whose relevance to the real world is direct and obvious. I have shown how it can be applied and tested and have pointed out some of the directions in which it might be developed. The analysis has not built "irrefutable theorems into an empty edifice of compound tautologies" but has employed "questionable premises...to obtain questionable conclusions" which seems to me to be the right way to approach economic phenomena'.

and Asset Prices (Turvey 1960b), written while a Ford Foundation Visiting Research Professor at Chicago. This emphasised multiple asset stock equilibrium and was part of a burgeoning interest in asset price theory generally (see Section 3). Additionally, he contributed both directly (Turvey 1953a) and indirectly to the balanced budget multiplier debate (Baumol and Peston 1955; Peacock 1956), and on the interaction between fiscal and monetary policy (Turvey 1956b). Thereafter, there would be occasional macroeconomic contributions: a demonstration that there was no evidence that the transactions demand for money was a stable function of the level of national income (Turvey 1956a); a reminder—at the outset of the more inflationary decade of the 1960s—of the power of the inflation tax (Turvey 1961); and, in a review of Keynesian interest rate theory, the suggestion of a new type of investment function which substituted comparisons of expected yields in relation to the interest rate with the concept of relating second-hand and new asset prices (Turvey 1965: 172).

3 Welfare Economics: The ‘Correct Approach to Costs’¹⁶

Urban Economics: Turvey wrote one of the first texts on urban land economics, *The Economics of Real Property: An Analysis of Property Values and Patterns of Use* (Turvey 1957), described by one reviewer as an ‘attempt...to bring Ricardo up to date’ (Bailey 1958: 179). Certainly, it was a characteristic work in that Turvey sought to bring elementary economic tools (the price mechanism) to a field (estate management) sorely in need of enlightenment and to do so with the minimum of words and maximum of clarity. Moreover, he was addressing core contemporary issues such as rent controls and planning legislation where the Conservative government’s Town and Country Planning Act of 1954 had just abolished the development charges on planning gains in Labour’s 1947 Act which, Turvey (1953b: 299) considered, had been ‘an ingenuous attempt to deal with the compensation-betterment problem’ for social advantage. This new field also marked the beginnings of his publications in economic history, with his first foray into this literature (Turvey 1953c) being on nineteenth-century London street improvements.

¹⁶The subtitle comes from Peston’s (2012) obituary and relates principally to Turvey’s Treasury period in which, focusing much more on micro- than macroeconomic issues, he sought to embed opportunity cost as the key reference point for decision-makers.

There was a clear, generalisable public policy message from Turvey's work in urban economics: '[A]ny scheme which is constructed without regard to the working of the property market and the technicalities of valuation is unlikely to work well; legal elegance is a poor criterion by which to assess proposals' (Turvey 1956c: 122). While Turvey did keep up his interests in this area, though mainly for the *Lloyds Bank Review* and in book reviewing, with his move to the Treasury in 1960 his academic research moved more towards related issues, and especially cost–benefit analysis.

Cost-Benefit Analysis: At the beginning of the decade, the American Economic Association and Royal Economic Society (AEA-RES) initiated a series of Rockefeller Foundation-funded literature reviews. The first published was on welfare economics, with LSE author, Ezra Mishan (1960: 255), providing a rather pessimistic conclusion about the prospects for the rehabilitation of this field, and in particular, 'the notion of discrepancies between private and social benefit which, at first glance, appears a promising field for the application of welfare economics—and a salutary reminder of the limited capacity of the invisible hand—[but] when pursued in earnest reveals grave difficulties'. Turvey did not feature within this survey, but over the next few years he was to write what would become a prominent contribution on externalities which had a clear message for both economists and public policy:

It is now abundantly clear that any general prescription of a tax to deal with external diseconomies is useless. Each case must be considered on its own and there is no a priori reason to suppose that the imposition of a tax is better than alternative measures or indeed that any measures at all are desirable unless we assume that information and administration are both costless. To sum up, then: when negotiation is possible, the case for government intervention is one of justice not economic efficiency; when it is not, the theorist should be silent and call in the applied economist (Turvey 1963a: 312–313).

He was also beginning to rethink public-sector investment criteria (Turvey 1963b), in particular the economic problems of the electricity sector (Turvey 1963c, 1964a), and, with his LSE colleague, Alan Prest, published what would be one of his most influential papers (see *Efficient Investment Decision-Making* below): the AEA-RES survey on cost–benefit analysis (CBA) which inter alia argued that the case for employing CBA 'is strengthened, not weakened, if its limitations are openly recognised and indeed emphasised' and, in particular, that 'The technique is most useful in the public utility area than in the social services area of government' (Prest and Turvey 1965: 731). Then, at the end of the decade, he published his

well-known *Economic Journal* paper, 'Marginal Costs' (Turvey 1969) which, much later with privatisation, regulation and competition-promotion, would attract the attention of a new generation preoccupied with the theory and practice of measuring costs.

Efficient Investment Decision-Making: This area of Turvey's work began in 1960 with his first LSE leave of absence to join the Economic Section of the Treasury, during which time he was to begin to apply his developing approach to CBA and marginal cost pricing. His work included the Channel Tunnel project where, to judge from the official history (Gourvish 2006: 178), he brought immediate analytical benefits to a sceptical Treasury seeking to understand the real costs and projected benefits.¹⁷ After the Treasury he was diverted for a year or so by Greek taxation, but in 1964 he resigned from LSE,¹⁸ having been headhunted by his former colleague at the School, one-time Professor of Industrial Economics and now Chairman of the Electricity Council, Ronald Edwards, to become Chief Economist of the Council.

Turvey's arrival at the Treasury coincided with an intense internal debate that would soon produce the Plowden Report on the control of public expenditure (HMSO 1961a) and a White Paper on the Financing of the Nationalised Industries (HMSO 1961b) (see also Daniels 1977). Peston (1968: 128), who was to succeed Turvey at the Treasury (1962–1964), says of this and the immediately succeeding period:

One of the most remarkable developments in public policy of the past six years or so has been the growth of interest by officials, politicians, and the boards of nationalised industries in efficient investment decision-making.

¹⁷Turvey was one of a number of academic economists in Whitehall in the 1950s and 1960s who, seconded from their universities, sought to improve decision-making through greater employment of basic economic tools: for example, Ian Little, who was Deputy Director of the Economic Section (1953–1955) and whose *The Price of Fuel* (Little 1953) began to formalise some of the issues raised in the Ridley Report (HMSO 1952) about how there could be increased competition and an improved relationship between prices and relevant costs; David Henderson, at the Treasury (1957–1958) and the Ministry of Aviation (1964–1966), whose strictures on public-sector investment still repay close reading, as does his later condemnation, in his Reith Lecture, of the continuance of 'Do-It-Yourself Economics' (ersatz economics) in public policy (Henderson 1965, 1986); and Michael Posner at the Ministry of Power (1966–1967), where the dominance of coal interests constrained the adoption of marginal cost pricing. Posner (1973) would embody much of what he had learnt from his Whitehall experience together with clear guidance for policy makers of how to conduct an economically rational fuel policy.

¹⁸Howson (2011: 909) records developments in 1963–1964 at LSE which culminated in Turvey not being appointed to a Professorship despite the initial recommendation of the Appointment Committee that he be so honoured. He resigned in April 1964, though later would become a Visiting Professor and eventually Emeritus.

Several forces have been at work here. There has been Treasury concern with the financing of the nationalized industries, which allegedly imposed a burden on the tax system ... [HMSO 1961b] set out the new policy setting targets for the nationalized industries, the result of which has been to raise significantly the extent to which their investment is self-financing ... A second force not wholly independent of the first has been the activities of economists working for government, notably in the economic section of the Treasury. Academic economists have long...taken for granted that inter-temporal decision-making involves discounting future revenues and costs. It may be difficult for them to appreciate, therefore, the triumph represented by the positive acceptance on the part of the Treasury of discounted cash flow.

There is no evidence of Turvey's direct influence on the first nationalised industry White Paper HMSO (1961b) whereas many have identified his effect on the second (HMSO 1967) which established marginal cost pricing and the ascendancy of discounted cash flow (Price 1983: 360; Chick 2007: 76). More generally, in a close study on the diffusion of CBA techniques in British government, Colvin (1985: 46–47) has identified the importance of the Prest and Turvey (1965) survey and other work by Turvey. All of this accords with the finding of Coats (1981) and others that the influence of economists on policy at this time lay less with high-profile macroeconomics and much more with microeconomic decision-making.

With the move to the Electricity Council came a period of sustained research in energy economics and public enterprise more generally, beginning with Turvey and Nobay (1965) which used energy consumption as the raw material for an exploration of index number problems associated with measuring consumption at constant prices. It has been argued that hitherto 'the really important economic papers in the energy field [were at this time]...published in specialist journals rather than economic journals' (Price 1983: 360). This now began to change with Turvey a key driver. As he wrote in the Introduction to his edited volume, *Public Enterprise: Selected Readings* (Turvey 1968b: 8), a volume for the then very influential Penguin series:

Economists seldom realize how much good work is done in their field by engineers and operational researchers. Many of the most interesting economic problems of public enterprise relate to operational and investment decisions. Economists tend to think of these as trivial and to substitute their solution in given cost and production functions, leading to oversimplification and disregard of a large body of knowledge. Econometric attempts to ascertain the presence and extent of economies of scale in electricity generation, for instance, have been a complete waste of time. However, planning engineers are directly

concerned with this issue in so far as it is relevant to action, for their job lies in optimizing the expansion of an interconnected system. Thus, an attempt to infer technology from *ex post* data, even if it were adequately sophisticated and even if it gave a clear result, would merely furnish out-of-date information about what was already known.

This constitutes something of a manifesto for this stage of Turvey's career, being the foundation stone for his major study, *Optimal Pricing and Investment in Electricity Supply* (Turvey 1968a), which sought to educate both economists and electricity engineers/administrators of what each had to bring to a more enlightened understanding of rational cost-estimation, price setting and investment decisions. Put formally, the objective was to maximise the sum of producers' and consumers' surplus, though in reality a tariff structure 'which fully reflected the structure of marginal cost and included an ideal time pattern of peak capacity charge would be extremely complex' (ibid.: 102). Turvey provided realism, political and commercial, which led to tariff simplification along marginalist lines (Chick 2007). Issues of peak-load pricing as an exemplar of joint product were to attract much attention at this time in the economic journals, with Turvey (1968c) joining battle in the *Journal of Political Economy* to promote, for an international audience, both the developing marginal cost approach and the 'large-scale long-term experiments of the type now being pioneered in Britain' (ibid.: 113). Concurrently, the *Economic Journal* debate to which Turvey (1969) had been a catalyst smouldered on into the new decade with Turvey (1971c) an answer to his critics.

An Inflationary Interlude and Then Back to Public Enterprise: Turvey's move from the Electricity Council to the NBPI in autumn 1967, and thence in 1971 into private consultancy, broadened considerably his understanding of general economic policy and the political economy of distributional conflict in particular. His time at the NPBI did not produce much in the way of new writing, and so far as is discernible his general view of inflation remained similar to that he had held in the 1950s, as for example in his comments to a Money Study Group (established by Harry Johnson in 1969) LSE conference in 1971 on inflation in which he subscribed to the then dominant interpretation of exogenous wage push: '[I]t is a question of relative deprivation and the perception of social justice which is the key to understanding what has happened' (Turvey 1971d: 200), although later in the decade he expressed scepticism that any such mechanism could be the cause of accelerated inflation (Turvey 1978: 196).

His earlier interest in how productivity issues impacted upon wage-setting were also much evident, and he was necessarily preoccupied with how the

new inflationary environment affected pricing policy and business sustainability in cases that came before the NBPI (Turvey 1971e: 489, 1974) (on sustainability, he would in 1974 become a member of the Sandilands Committee on inflation accounting which reported just as inflation was peaking in the UK in the summer of 1975 (RPI inflation was then 26%) (HMSO 1975). Above all, however, this was a period of extreme controversy with Turvey having a much higher public profile. This was often uncomfortable, not least in late 1968–early 1969 when the NBPI report on academic salaries (HMSO 1968)—which were a standing reference for the Board—was widely condemned, and not just within academia. Even a Conservative former minister responsible for universities described it as ‘inept and superficial’ with academics having ‘every right to be thoroughly disappointed’ (Boyle 1969: column 1341). One can but guess at the motive for Turvey’s inclusion of academic salary-determination—‘an occupation where earnings play a relatively small role in adjusting supply and demand’—as a case study in his little demand and supply textbook (Turvey 1971b: 97).¹⁹

With the change in government in 1970 the fate of the NBPI was obvious. Turvey completed his *Economic Analysis and Public Enterprise* (Turvey 1971a) in January 1971, just before the Board was dissolved. For the next four years, he was in private-sector consultancy and his published work was almost all amplifications and/or extensions of the new book to additional sectors (the volume had begun life as a series of lectures given in Norway in 1969 and was informed explicitly by Turvey’s NBPI experience) (ibid.: 9).²⁰ Here is the fullest expression of Turvey’s pursuit of optimal pricing, not through the ‘intellectual elegance’ of general equilibrium, but via second-best optimisation, partial rather than general equilibrium and the ‘rough and ready’ that is appropriate in practice to operate and analyse public enterprises (ibid.: 21). While electricity featured in Chapter 4 on simple versus complex price structures, the range of utility undertakings encompassed is now much broader, while in any case he was still working on electricity economics and would publish, in a very productive partnership with the Central Electricity Generating Board engineer-turned-economist Dennis

¹⁹This period throws up something of a curiosity. Aubrey Jones, who chaired the NBPI throughout its life (1965–1971), published a number of pieces on the Board, not least in his widely cited Penguin Special on the new inflation but made no reference to Turvey save to Turvey (1971c) (see Jones 1973: 108, fn.*).

²⁰From 1967 onwards, it was government policy that all major price increases in the nationalised industries be referred to the Board. The Board commissioned Cambridge’s Department of Applied Economics to investigate the effectiveness of this approach. Its report contains frequent references to Turvey’s work (see Fels 1972: xi, 214, 216, 219, 221) and his publications (Turvey 1969, 1971c).

Anderson, *Electricity Economics* (Turvey and Anderson 1977). This was widely reviewed, widely used, and described in one core literature review of public-sector pricing as ‘outstanding’ (Bös 1985: 175).

There is no extant evidence of Turvey’s period at Scicon, through from an advert in *The Economist* in late 1974, which sought to recruit a ‘young, capable and independent person to join our small team of high-powered Economists, who include Professor Ralph Turvey’, one might infer that the company expected him to stay in post a while longer.²¹

4 Later Career

ILO and the CPI: Turvey joined the ILO in 1975 as Director of its Department of Labour Information and Statistics, becoming de facto its Chief Statistician.²² The ILO, while long-established in Geneva (its origins lay with the League of Nations in 1919), has since the Second World War been constituted as the specialist agency of the UN, with the Statistics Section charged with

(a) the development of international statistical standards on the basis of the experience and requirements of the different member States; (b) the provision of advice to member States in the form of expert consultancies, training and manuals to assist them in the application of these standards; and (c) the dissemination of key labour statistics and methodological information through publications and electronic means (Laurie 1998: 5).

Turvey’s appointment brought him right to the heart of the nexus of key international economic organisations, namely UN-IMF-World Bank-OECD-EU) and he was to remain in post until he ‘retired’ (see *Retirement* below), making contributions in two key areas: labour statistics (especially a Labour Accounting System) and the development of the Consumer Price Index (CPI), the latter allowing him to revive his earlier interest in the theory and practice of index numbers (Turvey and Nobay 1965). In the context of the then highly topical debate about structural unemployment, Turvey quickly identified major shortcomings with existing labour statistics pertaining to industrial

²¹‘Scicon London Economist for Overseas Project’, *The Economist*, 21 December 1974: 107. The salary offered was upwards of £5,000; by comparison a fixed-term research post at LSE offered the salary scale £2118–£3990 plus £399 London allowance (‘Appointments’, *The Economist*, 3 May 1975: 133).

²²Turvey’s last *Who’s Who* entry records: ‘Economic Advr, then Chief Statistician, ILO, 1975–89; Dir, Dept of Labour Information and Statistics, ILO, until 1989’.

structure (Turvey 1977). Then, beginning with a series of short papers in the ILO's *Bulletin of Labour Statistics* in the 1980s, he made a number of proposals for reforming labour statistics so that better cross-country comparisons might be made, culminating in *Developments in International Labour Statistics* (Turvey 1990) which he edited. He also contributed a number of chapters, notably on labour productivity, where he made the argument (still being developed in Turvey 2001) that we needed better measures of economic change.

The most durable and influential output from this period was published upon Turvey's retirement, the ILO manual on the compilation of consumer price indices (Turvey et al. 1989). As described by Afriat (2005: xxi), 'For practice [this] one book is important and without alternative ... It is the basis for a worldwide standardization of practice'. Indeed, such has been its utility that the international Intersecretariat Working Group on Price Statistics produced a revised and expanded edition (ILO et al. 2004) which, unlike the original, also addressed theoretical issues. Turvey's practical influence also continued after he left the ILO through his membership of the UK's Retail Price Index Advisory Committee (1991–1994). Its technical recommendations (HMSO 1994) fed into the process that led to Britain's Harmonised Index of Consumer Prices which, launched by the ONS in 1996, replaced in 2003 the RPI as the measure for the Bank of England's inflation targeting and as the official inflation measure for uprating welfare benefits in 2011. Turvey was, however, something of a dissident about how housing costs ought to be incorporated, signing a minority report (ibid.: 62) that favoured including only mortgage interest payments and actual maintenance expenditures rather than what transpired (a combination of total mortgage payments plus historic cost depreciation) which was rather appropriately described by Goodhart (2001: F351) as 'Analytically...a dog's breakfast'.

The ILO manual had listed three general approaches to housing costs: net acquisition (asset based); payments (cash outflows); and user cost. For Turvey, the choice required a prior question: *the* question. What inflation measure was sought, and this within the context that, in a nice turn of phrase, 'We all face the same index number problem, perhaps best described as that of encapsulating in a single monthly number what cannot be encapsulated in a single monthly number!' (Turvey 1997: 1915). The question, here put succinctly by Goodhart (2001: F336), was: '[W]hat kind of index you should want depends on the use that you intend to make of it'. In Britain, where mortgage-finance home ownership rates were relatively high, monetary policy highly politicised, and house prices subject to cycles with high variance and significant public debate, some

economists had long contended that the RPI (which included mortgage interest payments) produced ‘perverse effects...on public perceptions of inflation, the conduct of economic policy and the success of that policy’ (HMSO 1994: 53). The treatment of housing costs, and more broadly whether asset price inflation should be incorporated, remains a live public policy issue in the UK, as elsewhere. Turvey made a major contribution towards illuminating the central issues and offering practical solutions—even if that advice was often spurned—in Britain and abroad.

Retirement: Formal retirement coincided with the coming to maturity of the Conservative governments’ privatisation policies and the regulatory challenge thereby presented. Turvey would be in his element, and after spending 1989–1990 as a Research Fellow at Statistics Canada, the national statistical agency, he joined the Centre for the Study of Regulated Industries (CRI), established in 1991 by the Chartered Institute of Public Finance and Accountancy at Bath University, serving as Chairman of its Advisory Committee from 1993 to 2009. Over the twenty years of the Centre’s activity Turvey made numerous contributions to its annual regulatory reviews, as well as producing technical and other papers, some of which were published in academic journals, with Burns et al. (1998)—which proposed a hybrid form of regulation between price cap and rate of return—the most well known and Turvey (2006) the last in a peer-reviewed journal.²³ One highlight was a joint CRI-London Business School (LBS) conference to mark the twentieth anniversary of the Littlechild Report on telecom regulation which proposed the RPI-X formula of regulation, the foundation stone of what has come to be known as the ‘UK model of utility regulation’ (Bartle 2003; Parker 2009: Chapter 12). It was entirely appropriate that, as someone whose influence on Littlechild’s research stretched back over forty years (e.g. Littlechild 1970 which was prompted by Turvey 1969), he should jointly chair this conference.

In parallel with Turvey’s involvement with the CRI, and various foreign CPI advisory committees,²⁴ he was a Visiting Professor in Regulation at LSE (1990–2009), a consultant at National Economic Research Associates from the early 1990s–2004, an Associate Director of the Regulation Initiative (the RI) at the LBS (2000–2005) and an Associate at Frontier Economics (2004–2008), the last of these a private-sector consultancy concerned with the ‘intricate relationships between companies, markets and public

²³CRI (2010) provides a complete publication and information list for the Centre.

²⁴Turvey’s *Who’s Who* entry records New Zealand, 1991; Britain, 1992–1994; and Canada and Sweden, 1992–2006.

policy'.²⁵ As part of the RI he wrote a number of working papers and organised events, typically half-day conferences which brought together business, regulators and academe. Two of these produced collections of papers which were published in *Utilities Policy* (e.g. Stern and Turvey's (2003) introduction to auctions). This network provided an appropriate conclusion to a career which, in very large part, had sought a middle way between market triumphalists and those who saw market failure at every corner. While Turvey did not write on regulatory failure per se, in his later writings he touched on the efficacy of regulation, as for example, in Turvey (2003: 95) on electricity generation capacity which accepted that 'markets *could* achieve' (italics in original) what was needed to avoid power cuts but raised searching questions about whether they would do so with the current regulatory regime.²⁶ Turvey's contributions to public debate were also informed by the renewal of his economic history interests, with his Burns et al. (1998) sliding-scale proposal influenced by his knowledge of regulation of the nineteenth-century water industry—the exemplar of a core network monopoly. In retirement, Turvey completed studies, all of London, which raised issues of externalities/regulation across a range of sectors (collected in Turvey 2008) and, fittingly, in his last LSE Working Paper, he completed a study of the cost of living in London in the century or so before 1834 (Turvey 2010).

5 Conclusion

Turvey was a very LSE economist of his time: one of the last of the generalist cohort who, in a long publishing career (1948–2010), made significant contributions in macroeconomics and, above all, applied welfare economics. He deserves to be better known among the younger generation, not just for his writings, but for his cosmopolitanism and commitment to public service. In a discipline where fads and fashions became part of the formalism of the

²⁵See <http://www.frontier-economics.com/about-us.com>. Frontier Economics was founded in 1999 and is worker-owned, with a former Treasury Permanent Secretary and head of the Government Economic Service, Gus O'Donnell, as its current chairman.

²⁶For contemporary discussions of regulatory capture in the context of British public utility regulation, see Parker (2002: especially 499–500). In a major work, Moran (2003: 105) has argued that Littlechild's RPI-X formulation embodied a 'determination to contain discretion, and therefore the dangers of special interest politics...[being] a powerful response to the old diseased club system that had wreaked such havoc in the government of the nationalised industries'. See also Mueller and Carter (2007) on the place of Turvey in the new networks of engineers, economists and management accountants (a hybridisation) which occurred in the post-privatisation, regulated utilities.

subject, he rejected empty formalism before the term had been invented. Thus, in words which could be *his* epitaph, Turvey (1960a: 8) described Erik Lindahl, his first mentor (Robbins would be the second, though with reservations):

What is so impressive is that Lindahl did not escape into empty formalism. It is all too easy for elegance to become an end in itself so that economic analysis becomes as irrelevant to the real problems of economics as the question of how many angels could dance on the head of a pin became to the problem of good and evil. Lindahl never wasted his and his readers' time.

In detailing his career and writings we have emphasised two central characteristics: expositional clarity so that economics can fulfil its potential to do good; and realism about policy if market failure is to be claimed as a justification for government interference. The former is exemplified in Turvey's stated objective, made as part of a discussion of how the desirability of price changes could be judged in practice: '[T]o restate received doctrine in a way which is simpler and intuitively more acceptable than the exposition in most of the relevant literature' (Turvey 1974: 825). The latter is taken from an introduction to a paper on optimum regulation in fisheries where:

[F]ishery regulation is one of those spheres of economic policy where what is the best thing to do depends on what can be done. This is usually illustrated by the analogy that, if one wants to climb as high as possible but cannot climb all the way up the highest mountain, the best thing to do may be to walk in the opposite direction and climb to the top of a lower one. If the *optimum optimorum* is to be reached (the highest mountain scaled), then regulation must extend not only to the scale but also to the mode of operation (Turvey 1964b: 64).

Turvey held no monopoly of these characteristics: they are part of the tradition of LSE economics of which he was a front-rank exponent. Thus, as Tribe (2009: 81) puts it: '[W]hat distinguished the exposition of economic principles' at LSE 'was the strong normative belief that such classroom principles could be translated unmediated into economic argument and policy agendas', and this at a time when 'the school's pedagogic reach was...far greater than that of Oxford, Cambridge or even Manchester'.

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29

Richard G. Lipsey (1928–)

Max Steuer

1 Preamble¹

A personal note is in order in writing about Richard Lipsey at the London School of Economics (LSE), and briefly about him in his subsequent years. It is flattering to be asked to write a piece on him. I welcome the opportunity to express my huge debt to Dick as an economist and as an individual.² At the same time, there is trepidation on my part at not being able to say anything new or revealing. Dick is an unusually self-aware scholar and has made his views on economics readily available on many occasions.³ I have only trivial differences with his views of himself and his views on economics. I joined the staff at LSE in 1959. Dick had been on the staff for four years. Though he was a young and relatively new member of the staff, he clearly

¹I am indebted to Professor Lipsey for providing some factual information. All judgements and opinions are entirely mine, and of course I am fully responsible for the accuracy of this chapter. The bibliography is an exception. That is entirely editorial, and I had no part in it. My small band of readers will note a few uncharacteristic stylistic usages, also down to editorial decisions.

²I use the more formal ‘Lipsey’ and less formal ‘Dick’ interchangeably for reasons of rhythm and style.

³See, for example, Lipsey (2001) and the especially revealing Lipsey (1997).

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had a prominent role in the Economics Department. I can only hope that in this paper some additional attention to Dick as a creative economist and as an encouraging and inspiring figure can be recorded. It was clear at the time that Lipsey would have an outstanding career. I will have something to say about that. This is not an entirely scholarly and objective undertaking on my part. I intend to sketch a picture of life in the Economics Department at the time, drawing on my memory of events and my current views on various matters. At the same time, I will endeavour to give a selective and representative account of some of Lipsey's many achievements while at LSE and in later years.

2 Early Years and the London School of Economics

Lipsey came to LSE as a PhD student in international economics in 1953. He joined the staff as an Assistant Lecturer in 1955.⁴ Dick made rapid progress to Lecturer in 1958, and Reader in 1959. In 1960, he was appointed Professor. When I joined the staff in 1959 Dick was much admired by his senior colleagues, especially Lionel Robbins, and was the leading figure among the talented young economists in the Department. They shared in his expectation that more than just doing a professional job, serious thought should be given to improving the methods of economics as a discipline. Lipsey left LSE in 1964 to take up a post as one of three founding professors of the new University of Essex, and six years later he left the UK to take up the post of Sir Edward Peacock Professor of Economics at Queen's University at Kingston, Ontario. While in the UK Dick was active on many fronts, including the National Economic Development Council (NEDC) and editing the *Review of Economic Studies*.

Certainly, for the wider public Dick is known especially for his outstanding contributions to economics textbooks. Central to these is *An Introduction to Positive Economics* (Lipsey 1963).⁵ At the time of its writing

⁴At the time, the UK's academic ranks were: Assistant Lecturer, non-tenured; Lecturer, tenured; Senior Lecturer, the career grade for those deemed to have limited research achievements; Reader, the career grade for those with distinguished achievements; Professor, most departments had only one professor who was also head of department. Larger departments often had a professor leading sub-disciplines within the same department.

⁵*An Introduction to Positive Economics* was initially published by Weidenfeld & Nicolson. At the 8th edition the publisher was changed to Oxford University Press and co-author Alec Chrystal was added. Starting with the 9th edition the book was retitled *Economics*. The 13th edition appeared in 2015.

his colleagues were dimly aware that a textbook was in progress, but there was no sign of this in terms of any diminished teaching, research and departmental activity: a testament to an extraordinary work performance. In addition to a variety of textbooks, over twelve other monographs have appeared. In my view, the most notable among these is *Economic Transformations: General Purpose Technologies and Long-Term Economic Growth* (Lipsey et al. 2005). Clearly, others support this judgement as this highly original treatise was a co-winner of the Schumpeter Prize for the best writing on evolutionary economics in the two years up to its year of publication.

While Lipsey has produced a number of important papers, I still believe that ‘The General Theory of Second Best’ with Kelvin Lancaster holds a special place (Lipsey and Lancaster 1956). It is an understatement that I am not an expert in general equilibrium theory. But apart from formalising the possibility of a self-regulating economy, I see this result as the most important finding in the field. It could be taken to be partial equilibrium, for example in the case of several distortions within a firm. However, it is more natural to think of it in the setting of many interacting relations of supply and demand in an economy. The central idea clearly grows out of Lipsey’s PhD thesis and later investigations of the welfare implications of customs unions. It is a crucial insight that removing distortions in a distorted economy need not lead to a welfare improvement. Some distortions are counterbalancing. This insight, sadly ignored in many quarters, further strengthens the need for specific information when doing policy, as against exclusive reliance on theoretical findings. The durability of this commonly cited paper was strikingly illustrated when on its 50th anniversary second-best issues were the subject of a conference at which Lipsey and Arnold Harberger were the keynote speakers (see Lipsey 2007).

Lipsey is an unusual figure among the truly significant figures in the current era of economics. For one thing, he is far less specialised than most major economists. I will return to this point. It is also the case that technical facility—by which we mean largely mathematical skills and innovations in statistical inference and econometrics—plays a smaller role in his case than in the work of most major contributors to economics. If there is one word that sums up his approach, it is ‘serious’. Of course, all important

The American edition, originally co-authored with Peter Steiner, was always titled *Economics* and went through twelve editions. The book has been translated into at least twelve languages.

scholars are serious, but in different ways. For Lipsey there is no wavering from the central goal of the endeavour: understanding what is going on in the economy. Mathematical technique plays a role, but it is in support of the main objective and not an end in itself. With Chris Archibald, Dick wrote a highly successful introduction to mathematics intended to explain more formal techniques to students trained in the more informal way that was common at the time (see Archibald and Lipsey 1967). The authors took the economics that students were assumed to know already and taught them how to formalise it as well as the mathematics needed to do so.

Peter Bauer in the Economics Department at that time was scornful of poorly trained economists, particularly in development economics. He knew how to calculate the elasticity of substitution and felt others should know as well. If they knew less mathematics than he did, that was a weakness. On the other hand, there clearly were economists who knew much more mathematics than he did. In his view, they knew too much. I do not think that Lipsey is of the view that his level is optimum, but I wonder if his doubts about mathematics for its own sake are entirely justified. Having certain skills with no current application can eventually be revealing of important problems to address and be helpful in tackling those problems. I think the problem, if there is one, is that too many of the best minds in economics are devoted to technical attainment, not that no one should be doing this. That is my view, at least, and it may well be the case that too much respect attaches to technique.

I am very taken with the, to me, huge changes in economics that have occurred over the past sixty years. Among these are the information revolution, including signalling, asymmetric information, search issues and the costs associated with acquiring information. Taking the lid off firms and the new institutionalism are other examples of major change. Macroeconomics used to be a Talmudic study of what did Keynes really mean. Not so today. Strategic considerations and game theory have transformed what used to be called oligopoly and left it at that, and has altered for the better our concepts of competition and rationality. My point is that these healthy and welcome changes have come more from the technically advanced economists than from elsewhere. But clearly there are issues.

One trouble is that excessive mathematical training, if it is excessive, comes at a cost. It takes time and energy away from other things. Of course, these 'other things' may not be teachable, or as readily teachable, as mathematics. Here I think Lipsey has demonstrated throughout his career both in academic publications and in his remarkable volume of textbooks that you can point students and others in a direction which leads to improved

understanding of how economies work by addressing real issues and facts. These things are teachable. The other trouble, namely that formal techniques can mislead, is one that is repeatedly ignored by many practitioners. In all of his work Lipsey has emphasised the point that formal methods can promote an idealised and misleading view of the often messy and complex nature of economic reality. Frequently he looks outside economics to fields such as geography, engineering, evolutionary theory and business studies for the significant facts that economics ignores at its cost. Excellent work often follows from drawing on diverse facts and theories.

As economists we are habit driven and often good at explanations. Natural scientists in biology and elsewhere are often content to find high-level facts and not to go on to explanations. Explanation is the real challenge. Paul Samuelson in his Stamp Memorial Lecture surprised his audience, including the chairperson, Lionel Robbins, by stating that pure theory is the boasting of the naive. Dealing with and accounting for facts is the real challenge. There are not many top-flight economists who take this seriously. Lipsey is one. It is the core of his work.

I once had the good fortune to be in my office when Kenneth Arrow stopped by. I mentioned that we had participated in a foreign direct investment conference. He denied it. I reminded him that several participants, including Harry Johnson, went for lunch and we had martinis. This shook his memory and Arrow said he remembered the martinis. I asked him how he felt about the general equilibrium project. He said the solution to one problem went on to suggest another problem. This is a perfect picture of what Lipsey calls an internally driven research programme.⁶ The relation between internally driven and externally driven research is a complex matter in which on balance, in my view, Lipsey leans more towards the latter.

I doubt if Dick has any recollection of a remark he made which has had a profound and lasting effect on my thinking. He once said in an offhand way that a market economy is about 30% efficient.⁷ Exactly what goes into the numerator and denominator of this ratio is unclear, as is the source for the necessary evidence. Yet there is much that I like about this remark. It is hard to model a self-regulating economy in a way that sees it as functioning less than perfectly. For many purposes a market may be a better institution than any other we are able to employ. That does not mean that it is perfect,

⁶This is set out in Lipsey (2001).

⁷In more recent exchanges, Lipsey suggests a figure of 60%. I rather like the more extreme estimate of 30% as laying down a strong challenge to market enthusiasts of an unqualified disposition.

or even close to perfect. Without careful empirical investigation, at any time and place we do not know how well a market is functioning. It is better to be aware of this.

When it comes to broad issues of left and right, the market and the government, my recollection is that Dick cast me as a right winger, perhaps somewhat in a spirit of friendly teasing. This might have been the normal way for a Canadian to view an American. But it certainly is the case that his more nuanced view of the market had a lasting effect on me and others. How well a market functions depends on the culture and social setting, in other words, on the facts of the situation. What the government should do and what the market should do is an empirical matter.⁸ This has been a lifelong theme of Lipsey's work. It relates in interesting ways to his later research and campaigning for US–Canada free trade.

Joining the LSE's Economics Department at the time that I did was a very exciting experience, and Lipsey was a major source of that excitement. The Department had its senior figures both in age and in reputation. These included among others Arnold Plant, Frank Paish, Richard Sayers, Peter Bauer, Phelps-Brown and, above all, Lionel Robbins. (Bill Phillips was an important figure but with a different background and approach to economics compared to the other leading figures.) We respected and admired them, but we did not want to be like them, or clones of their work. There was a strong feeling of revolution among many of the junior staff and Lipsey was the clear focus of this feeling. Especially closely involved were Chris Archibald and to a lesser extent Bernard Corry. I always felt that Corry was by nature an economic historian and was drawn into the circle with a focus on statistical evidence and modelling partly from the attraction of working with Lipsey. Maurice Peston was an interested participant in the movement and Kurt Klappholz a sceptical one. Lucien Foldes was then, and remains now, a deeply sceptical observer.

There were several elements to this rallying movement of the young economists at LSE. The absence of structured graduate teaching was an obvious weakness of the teaching at that time. I will return to this matter. The so-called literary tradition was dominant among the senior staff and especially favoured by Robbins. On one occasion Lionel came into the Senior Common Room (SCR) holding Stone's volume on the measurement of consumer behaviour. He held the book in a way that clearly indicated it was an

⁸Ten years of experience of privatising air traffic control in the UK provided an opportunity to compare performance while government-run and in the market (see Steuer 2010).

unpleasant object and asserted that whatever this was, it was not economics. He doubted if elasticities were stable for long enough to be estimated.

In those days Robbins ran a seminar of real importance. A number of ideas in international trade theory, for example, had their first hearing in that seminar. But when it came to empirical work, Robbins was not at his best. On one occasion, he tolerated a paper by Robert Eisner on the effect of interest rates on investment. Lionel introduced the paper saying that we would learn that investment hardly responded at all to interest rates. When the paper showed otherwise, Robbins said that this result was interesting, but we knew all along that that was the case. Peter Steiner, one of many excellent visitors to the Department, pointed out that this summing up was inconsistent with his introduction. The slip was not due to careless thinking. Lionel was a very careful thinker; rather, he just had no interest in investigations of that kind. Whether or not investment responded to interest rates was for him a matter to be decided in other ways.

Apart from his own research, the centrepiece of Lipsey's work at LSE was the seminar on methodology he organised along with several colleagues. I was not there for the founding years but benefitted immeasurably from this seminar. Dick has written fairly recently about the motivation for this activity and it is best described in his own words:

I first encountered Robbins' essay as an undergraduate in 1949 ... I read: "The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and *indisputable* facts of experience... (p. 78; italics added). If the premises relate to reality the deductions from them must have a similar point of reference (p.104)." I read and re-read this material and said to myself: "This cannot be right; facts derived from empirical observation must be more important to the development of theory than to act as *ex post* illustrations of what we already know to be true."

Some four years later I entered the LSE as a PhD student and attended Lionel Robbins' great Wednesday afternoon seminar ... As the weeks passed, however, Lionel's expressions of the then prevailing methodology described above revived my interest in [*Nature and Significance*] ... A group of us who were thinking along the same lines formed the LSE staff seminar on Methodology, Measurement and Testing in Economics and that became known as the M²T seminar. We talked to philosophers of science such as Joseph Agassi and, a bit later on, Imre Lakatos (who became a good friend of mine). Agassi introduced us to Popper and under his influence we came to reject the Robbinsian methodology of self-evident assumptions leading to

necessarily true predictions, and accepted the position that the propositions of economics were to be judged by the ability of their predictions to stand up to empirical testing ... From 1960 to 1963, I wrote *An Introduction to Positive Economics* which was designed to promote the methodology of testing as opposed to the Robbinsian methodology of intuitively obvious assumptions. The book had an immediate impact and went through five reprints in the four-year life of its first edition (Lipsey 2009: 845–846).

The driving ambition of the seminar came from Lipsey who chaired the sessions. Behind it all was a certain unease as to what economics was about. I certainly shared that unease. As an undergraduate at Columbia I went to an approachable member of the staff and explained that I could not understand what the theory of the firm, for example, was about. I could master the diagrams and pass exams, but it seemed to me to have little to do with actual firms. The teacher explained that my situation was like going to a concert and deciding to learn to play the piano, and finding little relation between learning scales and the experience in the concert hall. With much more study it all would make sense. At the time I accepted this seriously incorrect and misleading explanation of my unease.

At a similar intellectual juncture, Dick was more fortunate in coming across Hotelling and the theory of economic clustering. Here was a powerful theory of considerable generality which explained a lot. There was a firm relationship between theory and observation. This made sense. Lipsey retained an interest in this branch of economic enquiry over the years culminating in a dozen articles with B. Curtis Eaton subsequently republished as a book (Eaton and Lipsey 1997). A basic insight of Lipsey's importance in the area was the concept of defensive location applying the mini-max principle. With this qualification under specified circumstances clustering due to transport costs need not be socially adverse.

In Lipsey's textbooks, and as a lecturer, whether at advanced or beginning levels, there is never a suggestion to the effect that 'learn this now, and at a later date after much accumulated knowledge the reason for doing so will become clear'. This is a most important feature of his work. We should understand, or at least attempt to understand what we are doing. This was the beating heart of the M²T Seminar, as it came to be called. We were not just doing economics. We were probing the scientific status of what we were doing. In many ways for Lipsey this was a reaction to Robbins's essay on *The Nature and Significance of Economic Science*, as Dick has explained on many occasions. Where Lipsey differed strongly with Robbins is in thinking that this is how economics must be undertaken. For some problems a useful early

phase can be applying analysis to some intuitions about what is going on. But the claim that this in itself leads to a correct understanding of an economic matter is false. A theory should make at least one claim that in principle could be refuted. Actual testing of theories is a complex matter, but that remains a vital goal of economic science.

It is difficult today to imagine the pervasive influence of Popper at LSE at that time. Even professors of government could be seen taking careful notes at Popper's seminars and lectures, though one might conjecture that their interest was more purely philosophical than having lessons for their discipline. For Lipsey and his associates Popper offered a convincing alternative to Robbins and to the methodology of Friedman (Friedman 1953). While Friedman agreed that theories should have testable implications, his views that unrealistic assumptions were not a problem and that market-based economic theories had repeatedly passed were not accepted by the members of the methodology seminar. Archibald especially took the claim of successful passing of testing seriously and asked for the evidence (see Archibald 1961). Chicago did not take kindly to this questioning of a major tenet of doctrine.

Along with Lipsey we were all very fortunate in having Archibald as a colleague. As a true believer he articulated an uncompromising view of the falsificationist doctrine. It is useful at an intellectual juncture of that kind to have a clear spokesperson of the message. I think this was particularly helpful to Dick at the time. For one thing, he did not have to repeatedly make the argument. That was done for him. We also had the active participation of Joseph Agassi, a professional philosopher, in the Seminar. He claimed to know no economics, which was probably the case. But in retrospect, I think he was quietly engaged in a project of relating Popper's ideas, essentially of a natural science application, to the case of social science. It was a ground rule of every session that the speaker had to explain his paper to Agassi in terms that assumed no prior knowledge of the issues; in other words, no background in economics. Of course, the participants in the Seminar could benefit from this kind of exposition. I certainly did. I did not have to sit quietly in confusion over a reference to a bit of theory, or to an institutional arrangement which I did not know about at all or had only a vague understanding of. I think all seminars can benefit from this kind of back-to-basics approach. As Stephen Hawking said in a Reith Lecture, if you have to explain your ideas carefully to students, you may eventually understand them yourself.

This no nonsense and not taking things for granted was a critical aspect of the ethos that Dick brought to the methodology seminar. I believe it is also important to understanding him as an economist to recognise that he

is a 'natural', so to speak. He has, and according to his record of his undergraduate days, he has always had enormous economic intuition. Of course, he never leaves it at that, but it is a great starting point. The spirit of the Seminar and its ground rules, along with a partially unified methodological concern, made it the best seminar series that many of the participants have been lucky enough to attend, and certainly for me. This was very much down to Lipsey's approach to knowledge.

I do remember a rare instance where Dick's intuition let him down. One of the few institutions for graduate students in economics at LSE was the Oxford-Cambridge-London Joint Economics Seminar. I was told that the origin of this gathering, a rotating weekend in each term at a member university, was the desire to find out what Keynes was doing at Cambridge. Fortunately, the seminar carried on after *The General Theory* was published. More than just continued: the gatherings prospered, and to a great extent this was due to the leadership of Lipsey. For junior staff, and especially PhD students, it was a welcome opportunity to interact with Fellow researchers and temporarily get away from one-on-one doctoral supervision. On one particular occasion we were at Cambridge, the Michaelmas Term meeting, and Martin Feldstein was visiting. He gave the 'keynote' paper, to use a grandiose term. It was on a new departure, health economics, and none of us had heard anything about this before. Dick was not impressed. 'He is just applying marginal productivity to hospital beds', as if that was an unworthy thing to do. Nor did Dick anticipate such issues as what should be the objective function of a non-profit hospital. I note this in a light-hearted way as offering at least some partial balancing for my undisguised admiration, and to confess that at the time I thought if that is Dick's view it probably is right.

LSE generally, and the Economics Department in particular, benefited enormously from visitors, especially from the USA. I have mentioned Peter Steiner. Ron Jones was another. A distinguished law professor from Harvard also decided to spend most of his time with the M²T Seminar as the most interesting activity at LSE. No doubt London was an attraction to visitors, but the intellectual atmosphere Dick encouraged must have been a significant factor. In my view, methodology was a part but only a part. However, methodology has been a lifelong preoccupation of his. Dick regarded Imre Lakatos as a great friend and colleague: 'I think the untestable core and the testable protective belt was a brilliant post-Popperian idea and I used it as the organising concept in my 2000 paper "IS-LM Keynesianism and the New Classicism" in *Macroeconomics and the Real World, Volume 2: Keynesian*

Economics, Unemployment and Policy, Roger E. Blackhouse and Andrea Salanti (eds).⁹

An early paper of Lipsey's does seem to take the position that to be scientific is to postulate laws of general application (Lipsey 1962). To me this is a reasonable Popperian position. A qualified answer in Lipsey's paper is that we can have laws, in this case a law of wages, which can be stretched and pulled about by circumstances, but remain topologically the same, so to speak. In later work, Lipsey refers to and emphasises the 'qualitative' nature of some important economic principles, such as comparative advantage and opportunity cost, but certainly not all economic findings. To me, the question of which theory, or combination of theories, applies in particular situations is a problem and is not answered by reference to general laws. In holding this view, I was no doubt influenced by Dick's repeated emphasis that what matters for virtually all economic predictions or 'principles' is context.

Lipsey's approach to economic inquiry has had a significant influence on many economists. The view that economic theories, or explanations, apply to particular situations is a constant theme. With luck they may apply to other cases than the particular one initially under study. As a practitioner Lipsey works with theories in context. As I suggest, methodologists, be they of natural or social science, seem to see the essence of science as promoting useful and valid general propositions, or laws. Theories in context seem odd from that perspective, and dangerously ad hoc. This has never troubled Dick and he gets on with the job.

Lipsey appears to enjoy collaborating on research and on other writing. Of course, he has done many things on his own, but there is evidence of his pleasure in collaborating. At his initiative we did an article together testing Nicholas Kaldor's idea that the association between the rate of change of money wages and unemployment was due to both being associated with the rate of change of profits (see Lipsey and Steuer 1961). (Kaldor's conjecture failed.) Initially, my contribution was having put together a somewhat useful data set. It would not have been unreasonable for Dick to thank me for the data in a footnote and leave it at that. But we pursued many issues involved in this problem together, and I would like to think we learned from each other. For me it was a great experience, and I am sure that working with Lipsey was and is a rewarding experience for many people. Dick is a master of the Socratic method of inquiry. No doubt

⁹Lipsey e-mail to Steuer, 14 July 2016.

he enjoys the company of collaborators but bouncing the ideas of others is part of his working method.¹⁰

This way of working with others has little to do with dividing up the task. 'You do section A and I'll do section B'. Rather, it involves thinking together. This is not always an entirely smooth and unstressful process. Dick will not let go until he and his collaborator both know exactly what they are propounding. On one occasion, Dick and Frank Brechling came to my office in emotional states which involved a mixture of tears and coming to blows. They asked me to moderate while they went over the arguments in their paper on trade credit one more time (see Brechling and Lipsey 1963). I was honoured at being asked to perform this delicate task with its mixture of calming the situation and suggesting a few economic insights. It was nice to be able to give something back.

During our joint time at LSE, I was associated with the much maligned and misunderstood psychiatrist Ronald (R.D.) Laing. He was a great figure, and like many great figures, not all, he had a silly side. Critics make too much of that. Ronald asked me if I could bring some economists to an evening session at Kingsley Hall. This former headquarters of Gandhi's movement in London had been taken over by Ronald, along with Aaron Esterson and David Cooper, as a new way of addressing severe mental conditions. Part of the approach was no labels or uniforms. There was no outward way of distinguishing a patient from a perhaps distinguished psychiatrist. I brought Dick and Mark Blaug to the evening with Laing. I rather bungled chairing a sort of seminar that evening. Hopefully, that did not matter too much. The important thing was being there. Dick was right at home. Like the Kingsley Hall philosophy, Lipsey is not concerned with labels. He got on well with the people at Kingsley and brought warmth and understanding to them and to the occasion. Mark was, however, uneasy throughout. Laing embarrassed his colleagues by rounding out the evening with a stirring declaration that this was the beginning of a movement that would change economics, politics and psychiatry forever.

¹⁰A list of Lipsey's collaborators would include: Kelvin Lancaster, Chris Archibald, Max Steuer, Frank Brechling, Peter Steiner, Douglas Purvis, Gordon Sparks, Paul Courant, Robert Clower, Michael Parkin, Frank Flatters, Gideon Rosenbluth, Curtis Eaton, Wendy Dobson, Murray Smith, Bruce Ewin, Robert York, John de la Mothe, Paul Dufour, Colin Harbury, Daniel Schwanen, Edward Safarian, Paul Wonnacott, Clifford Bekar, Kenneth Carlaw, Robin Mansell, Morley Lipsett, Adam Holbrook, Russell Wills, Patricio Meller, Leonard Henrikson, Alec Chrystal, William Scarth.

In the taxi ride back, I had the feeling that Dick not only enjoyed the evening but could see something encouraging in it. On another occasion, I brought Laing and Harry Johnson together at a large party at my flat. Harry kept rifling through nearby draws looking for more booze. Laing was impressed with Harry's coherence given the booze. He took Harry's point about the human condition. We are ants in an ant heap with sharp limits on what a single person, or even a group of like-minded people, can do. As I will explain, Lipsey is a counterexample. He has engaged forcefully and successfully in public policy.

In some ways equally off the wall with the Laing encounter was the establishment of the NEDC, the British flirtation with French indicative planning. The cornerstone was a growth target. In order to capture public attention, the growth target had to be an integer. With the then growth rate believed to be 2.5%, 3.0% seemed too unambitious. A figure of 5.0% seemed unlikely, so 4.0% was chosen. Lipsey was taken on and, along with Alec Cairncross, was among the more senior economists at NEDC. A lot of interesting things went on other than economics, such as meeting with various sectors. Guesses were made about the future growth of electricity generation, and frequently revised, up and down. I seriously doubt if this exercise had the least effect on UK growth. I do not recall any of the elements at the time of Lipsey's later great work on economic growth. It is possible that this experience might have contributed to his eventual focusing on the role of technical change in economic growth, but more likely was his undergraduate exposure to Schumpeter.

Dick brought me into NEDC, which made me feel appreciated and had the additional feature of adding to my income. It was generally agreed that investment had a lot to do with growth. I do not recall much discussion of technical change. I was asked to do a study of the determinants of investment. As I prepared for this assignment, Dick took me aside and pointed out that there were roughly one hundred papers in the literature on the determinants of total investment. If I succeeded in my study there would be a hundred and one. It would make more sense to see what could be learned from the research undertaken so far. As Dick wrote of himself years later: 'In line with my original motivation [with respect to his book on growth] to know rather than to discover' (Lipsey 1997: xxxi–xxxii). This fatherly advice from someone only two years older than me had a very beneficial impact. The idea was not just to do a survey of the literature, but rather to treat what had been found as a great body of evidence and decide what could be validly inferred.

3 Moving to the University of Essex

Dick is above all a realist. While at the NEDC I remember us taking an American visitor to lunch. He suffered from gout. Nevertheless, he decided to order steak, and commented aside, 'I'll tell my wife I had lobster'. Dick's response was, 'What are you going to tell your foot?' Dick's decision to leave LSE and to go to the newly founded University of Essex was a rational one, it seems to me. Archibald followed him. For those of us left behind, or at least for me, Lipsey's leaving was a blow, not that I wanted to go to Essex, or anywhere else. It was also a blow when Peston and Corry left for Queen Mary College at Mile End, but that is another story. There was nothing vindictive in Lipsey's departure. That was not true of Archibald. He asserted that LSE was more or less finished, ignoring in my view the power of momentum of reputation and failing to anticipate the remarkable achievements of Ely Devons, the great Convener of the Economics Department. At a later date when I told Archibald that LSE was going to go for Harry Johnson and Frank Hahn he was sceptical, putting it politely and very mildly.

I remember very well when Ely came into the SCR at LSE flanked by Harry Johnson and Frank Hahn, with Terence Gorman as an added extra. It was not the same without Lipsey, but for me at least, it was a great boost in another way having Harry as a colleague. His joint appointment at Chicago was not apparent in his work at LSE. I remember an unnecessarily rough time Harry gave Richard Titmuss over the notion of blood donation as having benefits from being outside the market. But, like Lipsey, Harry had the same deep affection for and skill in economics, though with more acceptance of current practices compared to Lipsey. I once asked Harry if there was anything an economist needed to know other than economics. He took a long time and then answered, 'No, there is nothing'. What about history, I asked? You always give an historical introduction to your lecture topics. 'I make that up' was his reply. Maybe all this was because he thought I was too interested in things outside economics. However, Harry did appear on the stage of the National Film Theatre defending my film *The Committee*.

Lipsey probably left LSE out of a combination of frustration at not being able to establish graduate teaching, the challenge of being part of something dramatically new, and the appeal of Albert Sloman, the first vice-chancellor at the University of Essex. Both Essex and LSE were innovators in graduate teaching in economics. Under the pioneering leadership of Devons, and just after Essex introduced the first taught Master's economics degree in the UK, LSE replaced its own Master's degree by dissertation with a course-work MSc. I was asked to devise the empirical course. I called it 'Methods of

Economic Investigation’. Econometrics was about a third, or possibly half, of the course. Important evidence, relevant facts for understanding the economy, come from other sources as well as from econometrics. In taking this position, I was putting into practice ideas heavily influenced by Lipsey. The title ‘Methods of Economic Investigation’ has lasted, but the course became entirely one on econometrics. So it goes. Later I developed, administrated and for twenty years taught in a structured PhD degree in economics at LSE. That has been replaced with a strictly American-style doctoral degree, starting directly from undergraduate training and with the balance tipped towards coursework and away from teaching how to do research.

I doubt if Lipsey would approve of the growing homogenisation of higher education. For example, he introduced a course at Essex called ‘The Novel as a Force in Social Change’, where books such as those by Ralph Belamy and Charles Dickens were studied as exemplars of social impact. Dick’s idea was to expose students of economics to sources of change other than purely intellectual ones—no doubt an important lesson for students of economics. It is interesting to note that Thomas Piketty in his book on inequality draws on information from French novels (Piketty 2014: *passim*).

4 Returning to Canada

Textbook writing has been a factor in Lipsey’s very broad grasp of economics. Such work forces one to keep up with what is going on all over the subject, but I think he is inclined that way in any case. The economy is a complex and highly interdependent system. No doubt specialism pays off and has an important role. But as a realist wanting to understand any one aspect of the economy, Lipsey is sensitive to aspects outside a narrow confine.

Unlike Robert Lucas and more in line with Paul Krugman, I see the general failure of the economics profession to be aware of the financial crisis of 2007–2008 before anyone else as a problem. I put this failure down largely to specialisation, but that may not be enough. Macroeconomists paid little attention to the possible implications of the financial world for economic stability. Specialists in finance on the whole directed their attention to matters other than macro-stability. Lipsey was not among the few economists who had a good grasp on what was going on. This is not a criticism. He was working on other things. What it does suggest to me is that having a wide range is not sufficient. A specific empirical interest is needed in addition if one is to spot the current causes of future problems. Lipsey’s recent interest

in the implications for labour markets of extensive use of robots is a good example of combining wide knowledge with specific empirical information.

In spite of his wide range, finance does not seem to figure prominently in Lipsey's work. This is a little unexpected given his interest in, or better, his high regard for, Schumpeter and Schumpeterian ideas in economics. Schumpeter was not a modest man. He said he wanted to be the world's greatest economist, the greatest lover and the greatest horseman. When asked how it was going, he replied, 'Not too well with the horses'. Like Lipsey, Schumpeter had a wide range but with a prominent place for finance. Keynes's *General Theory* came out in 1936 and explained macro-stability largely in terms of real expenditure on consumption, investment, international trade, government expenditure and so on. Thus, in Keynes's opinion, the role of financial markets had little to do with the events of 1929. Schumpeter's two-volume work on the *Business Cycles* came out in 1939, three years after Keynes's, and to his great disappointment, had little impact. He must be partly to blame as *Business Cycles* is tortured reading. The argument is torn between historical description and formal analysis without in any way resolving or explicating the relation between the two. But it does offer to explain 1929 largely in terms of finance, which I think is correct. However, in most UK academic institutions, including LSE, the Keynesian view was overwhelming accepted compared to that of Schumpeter.

Inequality is another area where Dick has devoted relatively less attention. However, drawing on standard theory, he did point out in the late 1990s in several publications that globalisation would hurt low-paid blue-collar workers in advanced economies. As it happens, finance and inequality are particular interests of mine (see Steuer 2012, 2015; Steuer et al. 2015). Something remarkable has happened over the past thirty years especially in the growing degree of inequality in income and wealth. Of course, no one can cover anything approaching the whole of economics. Nevertheless, what has been achieved by Dick is pretty amazing. Quantity and quality often do not go well together, but this is not the case for Lipsey. There may be increasing returns for research. The more you do, the better you get at doing it and the more productive you can be, or so it seems in his case.

5 Active in Public Policy

Along with research, Lipsey has had a distinguished career in economic journalism. This is no mean achievement, and it has been recognised. Jointly with Douglas Purvis he received a National Business Writing Award in 1982

‘for distinguished financial writing by Canadians who are not primarily journalists’. While this is important as an economist with public concerns, more remarkable is Lipsey’s success in public policy. This success is a counter case to Harry Johnson’s view that individuals can do little. One focus has been Canadian-US free trade. I had a shot at this but it was half-hearted and scruffy (see Steuer 1969). Dick has given his all to public policy both as a researcher—three influential policy books—and as a dedicated campaigner and lobbyist, including several dozen longish pamphlets on the Canadian-American free trade debate. I believe it is fair to say that he has had a significant impact on Canadian trade policy and more widely.

In Lipsey’s view, more important than financial mismanagement and inequality is climate change. There are a number of interesting features to Dick’s research in this area. Part of it takes the existence of climate change as a given and addresses the issue as to what to do about it. His home territory of Vancouver is physically vulnerable on two counts. An important part of it is less than five feet above sea level. Moreover, the surrounding cliffs are subject to collapse if there is very heavy rainfall. Dick took a leading role in setting up what is now known as the Adaptation to Climate Change Team (ACT) at Simon Fraser University which works with local authorities with direct responsibility for anticipating the local consequences of climate change. On the preventative front, Lipsey is a founding commissioner on the privately funded Canadian Ecofiscal Commission. This body works with local and provincial governments to change policy on taxation and subsidy in ways that reduce pollution and encourage greener activities. It has had considerable success in both carbon pricing regimes and congestion pricing regimes in the provinces.

6 The Lasting Legacy

Will Lipsey be remembered long into the future? This is a tricky one. It amazes me today that quite often good graduate students are not familiar with the name Harry Johnson. I do not accept the test of time. History is not a good filter of merit. In literature, for example, some things last which are inferior to things forgotten. Being clearly and specifically relevant and hugely beneficial to a particular time is a great thing. Often it does not lead to lasting appreciation. Being able to help and give a lot as a colleague is invaluable. I certainly benefited from that, but it does not normally lead to lasting influence. The plain fact is that though ideas like the second best and modelling technology are likely to last, it is too soon to even take a guess as

to Dick's long-term impact. His way of doing economics will last, whether his name is attached to it or not, and we have not heard the last from Lipsey. It is highly likely that more great work will come forward.

Dick is an economic theorist, but a theorist with a difference. This is not theory for its own sake. The focus is firmly on issues of policy. Recently published work is characteristic of this objective (see Lipsey 2017). He continues to add to our understanding of the circumstances where economics can contribute to the development of better policy interventions. Going further, he has much to contribute to how to go about formulating useful policy.

In 2005, Lipsey received the Canadian Social Sciences and Humanities Research Council's Gold Medal for services to economics. The Council noted Lipsey's research foremost but also his work as an editor (*Review of Economic Studies*), textbook writer, journalist, activist in public policy and so on. The nomination notes for the award drawn up by Simon Fraser University conclude: 'This is a range of activities not commonly found among academics' (quoted in Lipsey 2005). Nested among the small group who do a lot of things is the even smaller group who do them well.

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30

Richard Layard (1934–)

Richard Jackman

1 Introduction

Richard Layard was born in 1934. His father, John, a distinguished anthropologist and psychologist, had led an unconventional and in some ways troubled life. But Richard nonetheless benefitted from a conventional, indeed traditional, education, following his father to Eton (where he was a King's Scholar) and King's College, Cambridge, where he read history graduating with First Class Honours in 1957. He worked initially as a secondary school history teacher in London, but in 1961 was appointed Senior Research Officer for the Robbins Committee on Higher Education, appointed to review policy on university education in the UK. Following the publication of the Robbins Report in 1964 Richard was appointed Deputy Director of the Higher Education Research Unit at LSE, and while in this position he studied also for the MSc in Economics, which he was awarded with Distinction in 1967. He joined the LSE Economics Department in 1968, as a Lecturer, becoming Reader in the Economics of Labour in 1975 and Professor of Economics in 1980. Richard remained in the Economics Department until reaching the (then mandatory) retirement age in 1999.

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1.1 Layard at LSE

While at LSE Richard was a major figure in research, not only through his own work but also setting up and leading research centres and through creating links with outstanding academics from all over the world. In 1974, he set up the Centre for Labour Economics (CLE) which investigated many aspects of labour market behaviour and which, from the early 1980s, concerned itself particularly with the causes and possible cures for the scourge of mass unemployment, then plaguing Western economies. This work was influential both academically (the Layard-Nickell model) and in terms of developing policies to reduce unemployment, in particular 'active labour market policies', which aimed to provide training and work experience for unemployed people. In 1990, the scope of his research leadership was expanded with the creation of the Centre for Economic Performance (CEP). Its various programmes investigated key policy issues, such as productivity and skills, the impact of globalisation, inequality, unemployment and labour market institutions, education reforms, macroeconomic policy, and the process of post-communist reform in Eastern Europe and the former Soviet Union. Richard continued as Director of CEP until 2003, again running into stipulations concerning retirement. He was however able to stay within the Centre as the Director of its newly created Programme on Wellbeing, which has been the main focus of his research over the past 15 years.

These research centres generated a vast amount of research involving many of Richard's LSE colleagues, most notably Steve Nickell, but many LSE academics working on macroeconomics or labour economics undertook their research in these centres. They included Chris Pissarides, Charlie Bean, David Metcalf, Richard Jackman, and latterly Tony Venables and John van Reenan among many others. The research covered a huge range of issues in macroeconomics and labour economics, and a succession of high-profile international conferences brought outstanding academics from all over the world to LSE. Many came to visit the centres for longer periods, including Olivier Blanchard, Rudiger Dornbusch, Stanley Fischer, Paul Krugman, Jeff Sachs and Larry Summers, and (more specifically on labour economics) Orley Ashenfelter and Richard Freeman. Many of Richard's publications on macroeconomics and labour economics were co-authored with these leading international experts during their visits to LSE.

While Richard's greatest achievements at LSE were in research he was also committed to teaching which he saw as complementary to research in that it required a clear focus on purpose combined with a systematic framework

for investigation. Initially, he taught the MSc Microeconomics course with Alan Walters—something of a maverick economist initially best known for a paper on congestion charging, but subsequently more famous as personal economic adviser to then Prime Minister Margaret Thatcher—bringing to it a focus on welfare economics. This course formed the basis of a textbook on *Microeconomic Theory*, co-authored with Walters. From the mid-1970s however Richard's main focus in teaching was on the MSc Labour Economics option, which he taught until his retirement.

Between 1984 and 1987 Richard was Head (at that time called 'Convenor') of the Economics Department. The 1980s were a difficult time for universities with severe cutbacks in government funding. While fees for home undergraduates remained fixed, universities were allowed to choose the fees they would charge international students. LSE, which already had by UK standards a high proportion of overseas students, was able to maintain a strong income flow while becoming (slightly) more exposed to commercial pressures. Despite Richard's early specialisation in the economics of higher education, he did not play a leading official role in LSE policy at this time. He was however a reformist Convenor, engaging academics more in departmental policy, for example in the design of degree structure. Additionally, Richard encouraged the setting up of a Summer School in Economics, initially to assist recruitment to the MSc programmes through the provision of feeder courses. The Summer School has grown and is now the largest in Europe. Though Richard never sought a senior management position within LSE, the School nonetheless recognised his many contributions to the School and to public life more generally and he was made an Honorary Fellow in 2000.

However, already by this time Richard had been pursuing a parallel career as a public figure. In the 1980s, he organised street demonstrations and the like to put pressure on the government to do more to combat unemployment, urging analysis and prescriptions derived from work at the CLE. After the collapse of communism, he was directly involved with both research and policy concerning the transition in Eastern Europe and Russia. Also, with the New Labour government in Britain after 1997, he was centrally involved with many policies, most notably with the campaign for joining the euro. (This campaign being perhaps the most detached from his academic work in economics.) His current work, still based at LSE, on happiness, well-being and the alleviation of depression and mental illness, brings together social and medical research with an effective political campaign to raise awareness of the issues and what can be done about them.

2 Economics

2.1 Education

The Higher Education Research Unit was set up at LSE to continue the research initiated by the Robbins Committee. Its Director was Claus Moser, then a Professor of Social Statistics at LSE, and who had been a statistical consultant to Robbins (while Richard was the Senior Research Officer). Richard's first article, 'Planning the Scale of Higher Education in Great Britain' (Moser and Layard 1964), provided an account of the analysis underlying the Robbins recommendation for the expansion of university education.

Moser left LSE in 1967 but the Higher Education Research Unit continued (albeit under various names) with Richard as Director until 1974. Richard's work on education could be grouped into three main areas. First, the determination of the scale of educational provision within the economy, overall and by sector, a topic at that time discussed in terms of the merits of 'manpower planning' as against more market-based criteria (the expected returns on education in relation to its costs). Richard's even-handed summary of these issues (in his paper 'Economic Theories of Educational Planning' (Layard 1972)), while generally in support of rate of return criteria, draws attention to the absence of futures markets in wages. This creates an information void which manpower planning might help to fill. A major issue here is the impact of capital accumulation on the demand for skilled as against unskilled labour. Richard's 1975 paper on this subject (with Peter Fallon) provided some general support for the 'capital-skill complementarity hypothesis', that capital accumulation raised the relative demand for skilled labour and hence would help maintain the employment prospects of the educated. Also in this subject area, Richard published a critique of the 'screening hypothesis' (that students do well because universities are good at selecting bright and able people, not because they teach them anything) pointing to a range of contrary evidence (Layard and Psacharopoulos 1974).

Second, Richard's approach to the technology of teaching, particularly at university level, was marked by an enthusiasm for the adoption of the new media because of the enormous economies of scale it offered. Richard was one of the first to argue for the widespread introduction of video and television and in his 1974 paper (with Michael Oatey) provided detailed calculations of its potential cost savings. Even in the 1970s, though, the main puzzle seemed to be why universities seemed so reluctant to take

advantage of these opportunities. Forty years on, following the most enormous expansion of new technologies and the outstanding quality of some of the products, the puzzle remains, in that the use of the new media in most universities remains limited.

Related to this, the third area of research concerned how decisions were actually made within the then government-planned world of the universities. Though constrained on many matters, such as tuition fees and numbers and staff salaries, by government policy, remaining decisions were typically made by staff, particularly academic staff, rather in the manner of labour-managed firms. Thus individual academics might seek fame and fortune through research while maintaining a baseload of routine teaching to justify their university salary (and therefore not wishing to be displaced by the new media). A number of implications of this type were explored in a 1973 paper, 'University Efficiency and University Finance', with Richard Jackman.

2.2 Labour Economics

The CLE was created in 1974 to investigate issues affecting incomes and employment, such as wage distribution, labour force participation and unemployment. Although the subjects of labour economics are central to people's lives, the study of labour economics has never been as central to academic economics as say, industrial organisation or finance. The LSE MSc course in labour economics, which Richard first taught in 1975, for example would not normally attract more than eight or ten students from an MSc cohort of around 120. Though the issues discussed in labour economics, with their direct bearing on the availability of work and the distribution of income, are of paramount importance to the well-being of a society, research into these issues was relatively undeveloped.

Even so, education was not forgotten. Much of Richard's work in the second half of the 1970s was concerned with the relationship between education and inequality. The immediate problem is that measuring inequality in terms of the distribution of incomes across individuals within a society at any point in time does not take account of the fact that the incomes of many people vary significantly over their lifetimes. At any point in time there will be in a society people at different stages of their lives, so that even if everyone has the same lifetime income there can be significant measured inequality. Part of the function of cash transfers such as tax/benefit systems which tax people when they are earning to pay benefits when they are ill,

or unemployed, or old, is to smooth out individuals' incomes over their lifetimes rather than redistributing incomes from one person to another. Likewise, higher earnings of graduates may to some extent compensate for years without income while studying or acquiring professional qualifications. Inequality between people is different from any one person having an uneven income over his or her lifetime. What matters is the income an individual receives in total over his or her lifetime.

However, while this seems clear in principle, it is obviously a complicated matter to establish an individual's lifetime income, particularly as most people live most of their lives within families which typically practise some degree of income sharing (Layard and Zabalza 1979). It is not even clear how lifetime income can be measured given not only differences in family circumstances but also in the lengths of people's lives and economic factors, such as discount rates. These problems were discussed in Richard's first paper in this area (Layard 1977) which ended with an illustrative example of how one might think of the impact of various educational policies on the distribution of lifetime incomes (Layard 1979). It seems fair to conclude that lifetime income is too complex a concept to be operationally useful in policy formation, but it does stress the need to assess the longer term, rather than current year, impact of policies.

The CLE also investigated some of the main labour market issues of the time, in particular the impact of collective bargaining on wages in the UK (see Layard et al. 1978), which was found to be large and increasing for manual workers (in the early 1970s) and the increased labour force participation of women (see Layard et al. 1980) for which higher wages appear to provide only a partial explanation.

2.3 Unemployment

After more than 30 years of unprecedentedly low unemployment throughout the Western world, the sharp recession of the early 1980s created unemployment rates not seen since the Great Depression of the 1930s. Moreover, it was unemployment which turned out to be remarkably persistent. Though the sharp rise in unemployment was clearly macroeconomic in origin (the response of Western governments to high and rising inflation), it seemed that its persistence could be explained only by labour market factors.

Hence, it became a major task of the CLE to explain the origins and causes of persistent high unemployment, and to design policies which, within the framework of these explanations, might be expected to reduce it.

The work of the Centre became best known through the lengthy and comprehensive book *Unemployment: Macroeconomic Performance and the Labour Market* (Layard et al. 1991) which describes a great deal of the Centre's research.

Initially though the task was to explain why unemployment had risen so sharply and remained so high in response to the oil price shock of 1979 and the adoption at the same time of restrictive macroeconomic policies by governments scarred by the inflationary turmoil of the 1970s. The perceived problem was that wages continued to grow due to concerns about inflation and resistance to cuts in real living standards. Early papers (such as Grubb et al. 1982) focused on the role of import prices and productivity shocks in wage and price setting, and the real and nominal lags impeding adjustment. Unemployment was assumed to hold down wages and prices, and the unemployment rate was whatever was required to restrain wages and prices in order to meet the inflation target. The magnitude and persistence of unemployment thus depended on the magnitude of shocks hitting an economy and the speed with which wages and prices adjusted. This approach could then be used to explain differences in the persistence of unemployment across countries (as in Grubb et al. 1983). The most significant finding of these papers was that countries where wage bargaining was centralised (or at least coordinated), such as in Scandinavia, seemed better able to adjust than those with strong unions but decentralised bargaining.

The framework developed to explain unemployment and its relationship to inflation, generally known as the Layard-Nickell model, was first unveiled in a 1986 paper in the *Economica* Supplement on Unemployment, and constitutes the backbone of Layard et al. (1991). The purpose of the model was to provide a rigorous microeconomic model of the Phillips curve relationship between inflation and unemployment, based on wage bargaining within firms. Workers and their union representatives are assumed to be rational so their wage claims are constrained by an awareness that wage increases feed into price increases which reduce the firm's sales and hence their demand for labour which can lead to job losses. Job losses matter more if unemployment is high and there are few alternative jobs than if it is low and other work is readily available. The upshot is that unemployment discourages large wage claims at the level of the firm, thus leading to lower wage and price settlements. If governments are committed to holding down inflation, in the absence of other measures they will need to allow whatever level of unemployment is sufficient to achieve their inflation target. This conclusion is of course no different from the familiar Phillips curve. What is distinctive is

the detailing of the underlying model, which enables the effects of different institutional arrangements and policies to be analysed.

For example, the model is characterised by a ‘decentralisation externality’ rather akin to the Prisoners’ Dilemma. Workers in each firm may correctly perceive that, by raising their money wages, they can increase their real wages (since their decision has a negligible effect on the general price level). But if all workers act on this basis, prices will go up, there will be no real wage gains and most likely more unemployment. If workers or their representatives could act collectively (or at least cooperatively), they could achieve an outcome in which all would be better off involving lower money wage increases but no loss of real wages and less unemployment. This appears consistent with the empirical finding noted above of the more corporatist countries coping better with the recession of the early 1980s.

The benign outcome, that restraint by everybody could benefit everybody, could also be used as a justification for incomes policies. Incomes policies fix through legislation the wage increase allowed in any firm, and thereby, if fully effective, ensure that the wage increase within each firm is equal to that in the economy as a whole. In the UK, incomes policies had been used successfully but only as an emergency measure. They are a blunt instrument since they prevent adjustment of relative wages to market forces. In principle, a similar outcome could be achieved by the use of taxation rather than legislative control. If a tax were levied on firms increasing wages by more than a stipulated amount, firms would become more resistant to wage increases. The tax on wage increases, also known as a ‘tax-based incomes policy’, could be presented as a sustainable and ‘market-friendly’ policy, since it avoided any form of price control.

Richard had in fact argued the case for a tax-based incomes policy earlier, notably in his Inaugural Lecture in October 1981. The Lecture, entitled ‘Is Incomes Policy the Answer to Unemployment?’ (Layard 1982), made the case for what was then described a ‘wage-inflation tax’, that is a tax paid by employers on increases in average hourly earnings above a stipulated norm. The economic effects of this proposal were very fully discussed (e.g. by Jackman and Layard 1986, 1990) and also in critical comments, e.g. by Minford (1987). While this discussion left the basic argument for the tax intact, it also uncovered numerous possible complications, for example, the effects on productivity growth, which somewhat weakened the case particularly when taken together with the significant administrative costs the tax would impose.

A crucial feature of the model is the mechanism through which unemployment discourages wage increases. If unemployment is high, but the

unemployed are not looking actively for work, they will not compete effectively with newly unemployed workers for jobs, so such unemployment would not discourage wage claims. It had been noted early on that the rise in unemployment in the early 1980s was not accompanied, after the initial shock, by a commensurate fall in vacancies, suggesting that the unemployed were not chasing jobs as actively as before. A possible explanation was that many of those made unemployed in the recession had become unemployed for a long time and had become, at least in the eyes of employers, less suited for work.

If vacancies are an important indicator of the state of the labour market, it is necessary to investigate how their number is determined and relates to the number of unemployed. This requires an analysis of firms' hiring practices and of the flows of workers within the labour market. A study of these issues had been prepared in the CLE (see Jackman et al. 1989) which demonstrated that there had been a significant shift in the relationship between unemployment and vacancies (the UV curve or Beveridge curve) in the early 1980s. The paper considered some possible causes, such as social security policy (the level of and eligibility for unemployment benefits), and policies to make it easier for unemployed people to find work or acquire new skills.

Though there was never completely conclusive evidence that long-term unemployment was in and of itself a major impediment to successful job search, the development of policies to assist long-term unemployed people find work, and latterly to avoid the emergence of long-term unemployment, may be seen as one of the most lasting direct policy legacies of Richard's work. Many of Richard's (and other) CLE papers during the 1980s stressed the need to focus on long-term unemployment (see Layard et al. 1986). While many others worked on unemployment duration issues, and the linked advocacy of 'active labour market policies', Richard returned to this as a central concern only with the return of a Labour government in 1997, as noted above.

The Layard-Nickell model was influential primarily because it was rooted in institutional reality, or at least the reality of the time within Europe, namely the central importance of unions in wage determination, and likewise the central importance of wage determination in inflation and hence in macroeconomic policy. Although the model was based on wage bargaining, rather oddly the determinants of the power of the two sides in the wage bargain were not itself central to the analysis. Though, during the 1980s, the government in the UK and in some other countries was preoccupied with undermining the position of the unions, the purpose of this was seen largely in terms of freeing companies from restrictive practices rather than of reducing

unemployment (although some economists did highlight this implication, as in the ‘insider-outsider’ model (see Lindbeck and Snower 1988)). The lower rates of inflation and unemployment which have followed seem to be entirely consistent with the model, even if not among the policy recommendations that Richard and his colleagues derived from it.

Likewise, unemployment can only be reduced if more jobs can be created. This can be achieved through more ‘flexible’ labour markets, with the unemployed being encouraged to take unattractive jobs by stricter benefit enforcement, etc. But this cure could be worse than the disease in that the life experience of people with irregular, insecure and poorly paid jobs might be worse than unemployment and possibly more difficult to escape from. In 1989, Richard published a paper with Steve Nickell on ‘The Thatcher Miracle?’, outlining some doubts, which were more fully articulated in a 1996 OECD paper ‘Combatting Unemployment: Is Flexibility Enough?’ (Jackman et al. 1996). Though unemployment may be a labour market problem, its long-term solution may have more to do with the creation of stable and worthwhile jobs matching the skills and capabilities of the workforce.

2.4 The Fall of Communism

Political developments in Britain by the late 1980s were overshadowed by the extraordinary and sudden collapse of the communist system, symbolised by the fall of the Berlin Wall in November 1989. The fall of communism was completely unexpected, and its speed and peacefulness were equally remarkable. Though unprepared, there was a feeling among Western economists, perhaps over-optimistic, that familiarity with how market economies worked could be helpful in guiding countries without that experience or professional expertise in adjusting to the challenges of the transition to market economies.

Richard was immediately involved, setting up a group with Rudiger Dornbusch to study the problems, and to recommend policies, for countries seeking to introduce market economics in place of central planning. This group reported as soon as 1991 in the form of a short book, *Reform in Eastern Europe* (Blanchard et al. 1991) which analysed the main immediate policy questions concerning price liberalisation, stabilisation, privatisation and economic restructuring. The book stressed the need for major reforms in all these areas simultaneously, while putting in place safety nets to reduce the pain created by the resulting dislocation. This became the standard approach

in guiding policy during the transition, which was adopted by the international agencies, such as the IMF and the OECD.

Centrally planned economies lack many essential features of market economies such as private ownership and the legal system surrounding it. For example, how might privatised firms, where wages and employment had previously been set by the State, in the absence of a clear ownership structure decide how many workers to employ and how much to pay them? Or how to price the goods they produce? How might the ownership of formerly collectively owned assets be assigned to new owners or managers to provide incentives for good governance (see Blanchard and Layard 1992a)?

Meanwhile, from a macroeconomic perspective, the collapse of the planned economies had led to unexpected problems. In most countries, there was an immediate sharp rise in unemployment as uncommercial operations shut down with nothing to take their place. In addition, many economies suffered rapid inflation, close to hyperinflation in some countries, including Russia and Poland. Stabilisation policies had to be specifically tailored to the circumstances of individual countries. In Poland, for example, wages were indexed, and prices marked up on wages which meant that adverse shocks (like say, a depreciation of the exchange rate) could lead to an inflationary spiral. On the basis of Polish experience, Richard argued for the continuation of incomes policy since firms had otherwise little reason to restrain wage increases (see Blanchard and Layard 1992b).

One area though in which the experience of advanced market economies did appear helpful was that of restructuring. The idea, popular at the time, that transition economies needed high rates of unemployment to make it easier for growing firms to recruit workers did not survive exposure to the evidence, which showed that restructuring proceeded as rapidly in low unemployment as in high unemployment countries and that growing firms recruited mainly from other firms rather than from the pool of unemployed. Unemployment rates could be better understood as resulting from the same factors as in Western economies, such as the stance of macroeconomic policy or the availability of benefits, rather than transition-specific factors (see Layard and Richter 1995).

3 Layard as a Public Figure

Alongside his work at LSE, Richard was always active in public life, though up until the 1990s academic work was the priority. During the 1980s, however, research at the CLE highlighting the harm done by unemployment and

focusing on policies to reduce it spilled over into public campaigns. Richard set up the Employment Institute to organise practical research on proposals to reduce unemployment and a pressure group, the 'Campaign for Jobs', to coordinate protest demonstrations and the like to put pressure on the government to do more about it. While he at the same time maintained a dialogue with officials and politicians on these matters it was as an outsider.

By 1991, the collapse of communism had spread from Eastern Europe into the Soviet Union. With the break-up of the Soviet Union, and the end of COMECON (the economic arrangements governing trade between the former Soviet Republics), Russia became not just the largest of the economies in transition but also the country most entrenched, historically and ideologically as well as economically, in the ways of its communist past. But in 1991 Russia had a new government of committed reformers appointed by President Yeltsin and led by Yegor Gaidar (Finance Minister in 1991 and Acting Prime Minister during 1992). Richard was appointed as an economic consultant. These were turbulent times in Russia with the 'hardline' reforms introduced by the government leading to the collapse of much economic activity, severe hardship and political unrest. In addition to advising the government, Richard set up an LSE office in Moscow to monitor developments and to provide advice. The data it collected were published in *Russian Economic Trends*, which, for ten years, was one of the main sources of information and analysis of the impact of transitional reforms. He also set up a college in Moscow, with academic direction from LSE, to teach internationally recognised principles of economics and finance to Russian undergraduates.

Meanwhile, in the UK, the accession of Tony Blair to the leadership of the Labour Party in July 1994 led to a radical transformation of its political stance encapsulated in its 'New Labour' slogan. This was designed to break the connection in the public mind between the Labour Party, the trade unions, high taxation and economic failure, and replace it with a greater commitment to markets and individual freedoms but combined with a continuing concern with the alleviation of poverty and other social objectives. Though this approach had been adopted in many European countries for many years, perhaps most explicitly in Sweden, it was regarded as novel in Britain and, it was suggested, may have been inspired by the concept of the 'Third Way', popularised by sociologist (and LSE Director) Anthony Giddens.

Richard had always supported a 'centrist' position in politics, believing that well-judged fiscal or legislative interventions could ameliorate many of the problems of society without seriously undermining the workings of the market. During the 1980s, he had been an enthusiastic supporter of the

newly formed Social Democratic Party, but New Labour adopted many of the same policies while becoming a much more effective political force. New Labour policies focused on ameliorative social reform but, importantly in the present context, also on policies designed on the basis of evidence and reasoned argument rather than of ideology or sectoral advantage. It was not just Blair, but, as it appeared, the values of the Enlightenment which moved into Downing Street on the morning of 2 May 1997. Moreover, the CEP was in a position to offer evidence and reasoning to assist in policy formation. Richard had already prepared proposals and ideas over a broad range and published a popular work, *What Labour Can Do* (Layard 1997a), and it was perhaps at this time that his close association with the government of Tony Blair marked the peak of his involvement as a public figure.

By 1997, the UK economy had been enjoying several years of expansion, sustained by a recovery of aggregate demand following the ‘Black Wednesday’ devaluation of the pound in 1992, together with reduced wage pressure linked to the weakening of the unions. But unemployment, and in particular long-term unemployment, was still high and its reduction was a major concern of the new government. As already noted, Richard was much involved, primarily with the implementation of policies based on earlier CLE or CEP work. Most notable were policies for tackling long-term unemployment through active labour market policies (the ‘New Deal’) and problems of low skills. With the policies was a commitment to the evaluation of their effectiveness (as, for example, in Layard 1997b).

Richard was himself brought directly into government at this stage, initially as a consultant at the Department of Education and Skills and as a member of various advisory committees. In 2000, he became a member of the House of Lords as a ‘working Peer’ (taking the Labour whip), and from then until 2008 was a member of various economic committees.

3.1 New Labour and the Euro

The major political issue at this time though was the debate concerning Britain’s membership of the euro, the single European currency, which was launched in 1999. While the former Conservative government had been cautious, as were many officials and economists, and indeed the incoming Labour Chancellor, Gordon Brown, the new Prime Minister, Tony Blair, was committed to joining and Richard became something of a cheerleader for this cause. The Labour government postponed the decision, opting out of initial membership in 1999 while investigating five ‘tests’ of the potential

benefits to Britain of joining. It decided that the tests had not been satisfied in 2003. But between 2000 and 2003, Richard published five (mostly co-authored) pieces on the euro which were unequivocal in their titles, and impassioned in their argument, for example, 'The Euro as an Engine of Prosperity' (Layard 2001) and a contribution to the short volume entitled *Why Britain Should Join the Euro* (Buiter et al. 2002).

The basic argument stressed in these papers, in terms of the substantial efficiency gains of large markets, such as the European Single Market, was not at issue. The extent to which realisation of such gains depends on adopting a single currency throughout the single market though is not self-evident. There are clearly savings in terms of transaction costs but their magnitude is not large while the wider economic effects, for example, in enabling trading partners to adjust to 'asymmetric shocks', depend on a range of assumptions and could go either way. With the benefit of hindsight though it is clear that claims such as 'One market requires one money' (Buiter et al. 2002: 8) were overstated.

The wholly unanticipated financial crisis of 2008 and the Great Recession which followed it had dire consequences for the Eurozone. The 'Stability and Growth Pact' (an agreement by governments to limit borrowing to 3% of GDP), which was designed to prevent excessive borrowing by member State governments, ruled out the use of fiscal policy to sustain demand while monetary policy was constrained by the 'zero lower bound' on interest rates. Worse, in the most depressed economies, with minimal inflation, the adjustment of wage costs could be achieved only by reductions in money wages, a slow process which unnecessarily prolonged the economic distress. Of course, fiscal contraction is needed to deal with large deficits but standard procedure, adopted, for example, by the IMF and which had served a variety of economies well, is to accompany it by exchange rate depreciation to sustain demand. This was of course not possible with a single currency.

While this particular set of circumstances was not foreseen, the underlying analysis of, for example, the exchange rate as an adjustment mechanism and the constraints on demand management policy imposed by a single interest rate, were well known. That so little was said about them (in Richard's papers among others), could perhaps be attributed to the 'neoliberal triumphalism' of the time according to which the collapse of communism was judged to have demonstrated that there was sufficient price flexibility in market economies to overcome any macroeconomic problems. Or indeed, any problems at all. In retrospect, it is remarkable that the case for the euro was made entirely in terms of unleashing the forces of greater competition, without much regard to the well-being of those whose jobs would be lost.

4 Happiness, Well-Being and Depression

From 2000, the main focus of Richard's research was on issues to do with happiness, well-being, mental health and illness. It was first highlighted in the Lionel Robbins Memorial Lectures, delivered at LSE in March 2003 ('Happiness: Has Social Science a Clue?' (Layard 2003)) and subsequently published as *Happiness: Lessons from a New Science* (Layard 2005a).

Richard had been interested in these issues but for many years but they had taken second place to his involvement in more urgent matters, like unemployment and the fall of communism. He has one early article on the subject (see Layard 1980). The starting point, known as the 'Easterlin Paradox', is that for all the efforts governments put into supporting economic efficiency and encouraging growth, the citizens of rich countries on average appear to get no happier over time (in terms of self-reported happiness) and the citizens of rich countries no happier than those in countries less well off. Yet individuals strive to increase their incomes and within any society rich people appear to be happier than the poor.

In the 1980 article, Richard simply assumed that people were primarily interested in their relative income (or status). This creates an 'isolation fallacy' where each person believes that by increasing their own income they could improve their relative income, but it is not possible for everyone to increase their relative income and efforts to do so will be self-defeating. One person can only succeed in raising his or her relative income if someone else's relative income is reduced. In a race there can be only one winner however fast people run. The focus of the article was on public finance: the isolation fallacy would lead to a misallocation of resources which could be reduced by suitable taxation. It did not probe either the evidence for the relative income assumption (described as a 'basic fact' (ibid.: 737)) or discuss its possible causes.

Though a number of influential economists took this issue seriously it did not generate much research, mainly because, while plausible in general terms, it would be difficult to design policies without a robust quantitative basis. Moreover, the basic measure, how people reported their feelings of happiness, seemed too subjective. (Did the word 'happiness' mean the same to different people? In different countries? Did people see their objectives summed up in this word? Did putting their feelings on a scale of 1–10 make sense to them?)

However, by the 2000s analysis of the problem had been transformed by developments in neuroscience and the discovery that there was a clear link between objective and measurable neural impulses and reported happiness. Neuroscience was able to establish that, far from being shaky and subjective,

the survey evidence on reported happiness correlated closely with neural activity within particular areas of the brain. On this basis, it became reasonable to treat survey data on reported happiness just like any other data, and hence as a sound basis for scientific enquiry. It was, as the book title indicated, a 'new science'.

Of course, the fact that reported happiness has an objective neurological measure does not mean that it is self-evidently the appropriate objective of public policy. This raises various largely philosophical issues to which we will return. More immediate is the question as to how policy should be designed if its objective is to create happiness rather than wealth. Over the last 10–15 years, there has been an enormous amount of research on this in the UK, the USA and in many other countries, supported by international organisations, such as the UN and OECD. Notably, the OECD has committed itself to 'Redefining the growth narrative to put the well-being of people at the centre of our efforts' (OECD 2015: 1). Developments have been reported annually since 2012 in the *World Happiness Report* (edited by Helliwell, Layard and Sachs) published at Columbia University, New York. This report not only ranks countries in terms of the reported happiness of their people (Norway was top in 2017) and more significantly records the main factors affecting people's well-being. According to its assessment of the research findings, based on aggregate national data, income matters but it is not the only or even the main factor. As important are life expectancy and social arrangements, including freedom and trust. Of the top ten countries in 2017, seven were (small, mostly Northern) European countries, together with Canada, Australia and New Zealand. While there are always ambiguities in the interpretation of data, the overall message from this analysis does not seem to support the Easterlin Paradox: People appear to be happier in high-income countries, particularly those which use their wealth to support social objectives, such as health and education.

Richard campaigned for the adoption of happiness as the guiding principle for public policy in both popular (see Layard 2005b) and academic publications (e.g. Layard 2009) but was also directly involved in many econometric studies of the factors affecting individual happiness. Much of this work was summarised in one of the chapters in the first issue of the *World Happiness Report* (see Layard et al. 2012) and, more recently, in Richard's report to the OECD Well-Being Conference (Layard 2016). These latest findings suggest that income is almost as important as health or family circumstances, and more important than employment status.

Many of the studies on individuals confirmed the notion that happiness is characterised by 'mean reversion', that is to say that most people appear to

have some base level of happiness depending mostly on their personality, and if they experience say, an increase in income, their happiness would initially increase but then, with no further change in income, would fall back to the base level. Happiness it seemed depended not on the absolute level of income but on current income relative to what was expected or what they were used to. This gave rise to the metaphorical picture of the ‘hedonic treadmill’, of people trying to improve their happiness by earning more, but as their expectations and living arrangements adjusted their happiness would fall back to its previous level. This would help to explain why happiness is correlated with income in cross section, but not over time: once people become accustomed to a higher standard of living, they do not feel any happier.

Nonetheless, it has to be said that the happiness principle has not caught on in public or political discussion, possibly because happiness sounds to many a somewhat secondary, if not frivolous, objective when the majority of people see their lives as something of a struggle. Moreover, people need a purpose in their lives other than just enjoying themselves. Obviously, people do value material comforts, such as warmth and shelter and protection from hunger and disease, but would not describe themselves as happy because they have achieved these objectives. It seems better to use a term such as well-being, contentment or life satisfaction which appear less subjective and less volatile than happiness.

In any event, while ambiguity may surround the word happiness, there is no such ambiguity about its opposite, misery, and it seems natural to ask within this framework what characteristics define those reporting least happiness. Most reported misery is determined by the same factors as reported happiness but with the opposite signs. However, one clear cause of misery is severe depression, and this led Richard to a sustained and effective campaign to raise awareness of mental health issues and of what might be done about them. He set up a Mental Health Policy Group at the CEP which published a *The Depression Report: A New Deal for Depression and Anxiety Disorders* (Centre for Economic Performance, Mental Health Policy Group 2006) and a further paper arguing that mental illness is inadequately funded in the NHS (Centre for Economic Performance, Mental Health Policy Group 2012). In a further series of papers, Richard showed that mental illness and depression were not only the main cause of unhappiness but that they could be treated or at least ameliorated and at a small cost compared to other policies which might be expected to improve the national well-being. Much of this work was carried out with leading psychologists and other medical professionals and has had considerable success in raising awareness of mental health issues in the UK (see, for example, Layard 2014).

4.1 Layard and Philosophy

It is finally worth asking whether happiness, or well-being, is the most sensible basis of policy. Throughout all his work, Richard has had a clear and consistent philosophy. It is based explicitly on Benthamite utilitarianism, summed up in the phrase: ‘the greatest happiness of the greatest number’. This Benthamite principle is made to some extent operational for the first time by the new science of happiness. ‘The Greatest Happiness Principle: Its Time has Come’, as Richard put it in his 2009 paper. With a better understanding of what factors affect happiness it becomes possible to design social and economic arrangements which put people’s well-being first. Conventional welfare economics defines well-being in terms of the consumption of goods and services which is only part of the story, but more seriously it may be perverse if arrangements to encourage efficiency and growth adversely affect other dimensions of welfare. Competition may lead to greater efficiency, but cooperation to greater happiness. People living in Sweden and Denmark are happier than people in the USA though their incomes are on average lower. But, while the science of happiness frees Benthamite utilitarianism from its association with economic materialism, that does not establish that it is the best principle for social organisation.

Along with most other economists, Richard has never questioned the central importance of free markets in economic matters, both in creating material well-being and in channelling individual self-interest into serving the common good. But happiness research has stressed the importance of social arrangements to individual well-being, such as individual freedoms, the rule of law, social justice, the absence of corruption and integrity in public administration. Whether or not the maximisation of happiness is the best principle of social organisation, it seems logically better than the maximisation of GDP, and provides a more rational basis for policy decisions.

5 Conclusion

Richard’s career has been exceptionally varied, wide-ranging and productive. He has published close on 50 books and 150 articles, has advised governments in Britain and abroad and has guided public policy on a wide range of issues. His work on happiness and mental health is perhaps most important and has the greatest potential. But all his work has been motivated by an extraordinary commitment to tackle important problems, however intractable

they appear, and to employ the most appropriate scientific methods to their solution. It has also been characterised by a strong dose of idealism and optimism about people, which though sometimes a bit impractical, has made him the most inspirational colleague.

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31

Charles Goodhart (1936–)

Donald Kohn

1 Introduction

Charles Goodhart is an engaged political economist who has contributed to and applied several strands of economic analysis to improve policy making, especially policy making by central banks. He has been a pioneer in linking macroeconomics and finance, the fields that come together in the monetary and regulatory policies of central banks. He has insisted that policy analysis has a coherent theoretical basis, but that the resulting models be tested against empirical evidence if they are to produce useful policy suggestions. To make those models useful, they also must accurately reflect the institutional setting in which policy is made—the legal, political and economic framework of society and the resulting incentives and constraints faced by private economic agents and policy makers. Goodhart has thought deeply about the structure and functions of central banks and the linkages between central banks and the financial system and the government. He has served as an academic at the London School of Economics (LSE) but also inside the Bank of England as staff supporting policy makers and as a policy maker himself on the Monetary Policy Committee (MPC), with his service in each setting enriching his contributions in the other.

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Goodhart's output has been enormous and diverse. Moreover, along with Mervyn King, he founded the Financial Markets Group (FMG) at LSE in 1987 to foster the study of financial systems and to link that study to the actual functioning of the financial markets nearby in the City of London. In the first discussion paper of the FMG, he wrote: '[W]e should surely base our study and research about the functioning of financial markets on the practical experience and actual current problems of the City, to provide a union between practitioners' knowledge and academic analysis' (Goodhart 1987: 2). Goodhart has been a prolific author of books and articles about the financial system, the economy, central banks and central banking policies. At the end of 2017, Google Scholar listed 539 books and articles of which he is the author or co-author. The three most cited works illustrate the focus of Goodhart's interests: *The Fundamental Principles of Financial Regulation*; *The Evolution of Central Banks*; and *Money, Information and Uncertainty* (Brunnermeier et al. 2009; Goodhart 1988, 1989). He has also been a mentor and exemplar for many colleagues in academia and central banks, including the author of this chapter.

This chapter will begin with Goodhart's biography, emphasising the origins and influences on his body of work and how that body developed over time, through his service on the MPC. The second section will cover his work in the period since then, focusing on the lead up to the global financial crisis (GFC) and analysis of the responses to that crisis, in regulation, in monetary policy and in the structure of central banks and their relation to governments. The extraordinary developments of the GFC—including the build-up of vulnerabilities in the financial system, the unfolding of the crisis itself and the responses of the authorities to counter the crisis and to build a more stable system for the future—have, however unfortunately for the many affected, given Goodhart an opportunity to demonstrate his considerable experience, wisdom, knowledge and skills as an engaged political economist.

2 Biography and Contributions Through 2000¹

Charles Albert Eric Goodhart was born on 23 October 1936. Goodhart came by his interest in financial markets and in public service naturally; his father, an American, was the grandson of one of the Lehman Brothers who

¹This section draws heavily on 'Whither Now?', an autobiographical essay that Goodhart wrote for the *BNL Quarterly Review* (Goodhart 1997).

started the investment bank, and the extended Lehman family included Herbert Lehman, a Governor and Senator from New York, and (by marriage) Henry Morgenthau Jr., Franklin Roosevelt's Secretary of the Treasury. His appreciation of the importance of legal and political institutions also has a family foundation. Goodhart's father, Arthur Lehman Goodhart, was a renowned Professor of Jurisprudence at Oxford and Master of University College, his brother Phillip was a Conservative Member of Parliament, and his other brother William was a lawyer, frequent political candidate (as was his wife) and a member of the House of Lords.²

Goodhart went to Cambridge in 1957. There he found economics to be a subject at which he excelled and which he enjoyed:

Moreover, economics was fun and a challenge because it seemed so unsure of itself (so bad). Despite the formal models, no one really knew, or knows now, what determines the level or rate of growth of most of the key economic variables ... To find a subject wherein one's teachers admitted that there were several possible answers, and that none of those yet developed might be correct was profoundly liberating. I believed as a result that I might be able to contribute, and to do so in a socially worthwhile manner (Goodhart 1997: 388–389).

His paper as an undergraduate to the Marshall Society dealt with learning and decision-making under uncertainty, themes that would recur in his later work.

Goodhart went to Harvard for his PhD. His thesis showed both his emerging interest in banks as key elements in understanding business cycles and his insistence on confronting theory with data. He utilised detailed high-frequency US banking data from 1900 to 1913 to test hypotheses about the behaviour of financial markets and economies when banking systems were subject to both seasonal and cyclical and other shocks under the gold standard in the absence of a central bank (see Goodhart 1962). Back at Cambridge, England, he continued to bolster the profession's ability to formulate and test theories by collecting, checking and assembling monthly reports of the British joint-stock banks from the late nineteenth and early twentieth centuries and publishing the results for others to use (see

²From a lecture by brother William on the life of their father: 'I must explain that the Lehman family, for many years past has had no connection with the management or ownership of Lehman Brothers. I disclaim any personal responsibility for the financial crisis. Of course, had the family remained in charge my brother Charles would have been running Lehman Brothers and there would have been no crisis' (W. Goodhart 2010: 2).

Goodhart 1972). After a brief stint at the Department of Economic Affairs, in 1966 he was attracted to LSE by its emphasis on monetary economics.

In 1968, Goodhart went to the Bank of England on a two-year assignment that in fact lasted until 1984. With the expertise and interests he brought with him from the academy, he was the only economist in the Bank familiar with modern monetary theory and policy analysis. Importantly, he had greater knowledge of the ideas of Milton Friedman and the monetarists than did others at the Bank. As Bretton Woods was breaking down and inflation rising in the 1960s, attention turned to alternative anchors for the price level and to monetary policy as potentially supplying that anchor—for the monetarists through establishing and achieving reasonably stable targets for growth in the monetary aggregates. This contrasted with the prevailing Keynesian analysis in the Radcliffe Report, which emphasised the primacy of fiscal policy.

In this environment, Goodhart found himself translating the monetarists in academia and the government to the Bank and the Bank's thinking to the monetarists. A key to the monetarist prescription was stability in the demand for money, and Goodhart, along with Andrew Crockett, did pioneering work in trying to determine whether the required stability existed in the UK (see Goodhart and Crockett 1970). They found a stable demand for sterling M3 (and also M1), which had the added advantage that sterling M3 could be linked to domestic credit expansion. Stable money demand meant that the relationship of money growth to income growth was predictable, so money was a viable intermediate target for achieving nominal income and inflation objectives, and that money growth could be controlled over time by the central bank adjusting interest rates in money markets.

The usefulness of targets for sterling M3 proved short-lived, however, as the stability of demand broke down within a few years. Goodhart observed that this experience was not unique to the UK, and that in fact the stability of money demand had eroded in several jurisdictions—and often that breakdown was most pronounced for the definition of money relied upon as a guide or target by the central bank. That observation led to a wry footnote in a paper for a conference at the Reserve Bank of Australia in 1974 that stated that 'whenever a government seeks to rely on a previously observed statistical regularity for control purposes, that regularity will collapse', which became known as Goodhart's Law (Goodhart 1984: 96, aside). The experience of the early 1970s did not lead to the abandonment of monetary aggregates targets, but Goodhart's Law came back into play in the early 1980s when such targets again proved to be poor intermediate indicators of current or future income growth as shifts in money demand re-emerged.

Although the comment was partly jocular, the underlying thinking was serious and consequential and is often linked (including by Goodhart himself) to the Lucas Critique of policy modelling and evaluation that emerged around the same time (see Lucas 1976). Lucas noted that the response of private agents to economic variables was affected by agents' expectations of future policies, and that econometric estimates of responses under one policy regime might not be indicative of responses were policies to shift and along with them the public's expectations of future policies. Goodhart's Law was focused on one particular policy—monetary targeting—but the regime shift to focus on actions to achieve those targets, along with other policy changes in the financial sector, greatly affected the demand for money in ways that had not been anticipated. Deregulation in the early 1970s led to a surge in bank credit and broad money; that was followed by the 'corset' which was designed (by Goodhart among others) to rein in money and credit growth. In the early 1980s, the elimination of exchange controls and the corset, along with other financial innovations, meant that the relationship among interest rates, incomes and money growth was further disturbed. In brief, the public's reaction to shifting government policies had undermined the utility of the aggregates as intermediate targets for monetary policy.³

Although monetary aggregates turned out not to be useful as intermediate targets for policy, Goodhart did not abandon the quantity aspects of monetary policy—the growth of money and credit—as indicators of policy efficacy; he did not follow the economic herd in the pre-crisis years into focusing exclusively on prices and rates in financial markets to characterise the channels of monetary policy. Informed by his work on US and UK bank balance sheets, his grounding in institutions, his experience in the Bank of England and his knowledge of financial markets, he has continued to emphasise, including after the GFC, that transmission through banks is critical and that awareness of the quantity of bank assets and liabilities is key to understanding how policy is affecting economic activity and inflation.

In 1983, towards the end of his tenure as a staff member at the Bank, Goodhart became involved in establishing a new currency anchor for Hong Kong. He helped to formulate the peg of the Hong Kong dollar to the US dollar, thereby durably stabilising a currency and financial system that had been subject to considerable economic and political shocks as the relationship of Hong Kong with the UK and China was evolving. Goodhart went

³The foregoing paragraph draws in part from Chrystal and Mizen (2003).

on to play important roles in the formulation and implementation of inflation targeting regimes in New Zealand and, as policy maker, in the UK, with both regimes serving as examples that other countries have drawn on.

Goodhart has been a strong advocate of central bank independence in the implementation of monetary policy, but in pursuit of targets set by the government. The requirement for elected representatives to set the broad framework, including the inflation objective, is critical to maintaining the primacy of government in a democratic society. At the same time, an independent central bank is best positioned to implement policy within the framework to achieve the inflation objective on a consistent basis. The reason for this, in his view, is not that politicians would be engaging in complex games with the public to entice extra effort and income, as posited by the formal time consistency literature. Rather, politicians periodically facing the electorate would not want to take the sometimes painful steps of raising rates in the here and now to avoid the uncertain future problem of higher inflation. The natural consequence is that they would be hesitant to raise rates, putting off the necessary actions until the problem was clear and present; the net result of this inclination is that inflation would be higher and more variable—further from the ideal societal value set by the politicians themselves—if the tools of policy were directly controlled by elected representatives. Central banks with a high degree of operational autonomy can adjust the level of interest rates today based on a forecast of future inflation, thereby taking into account the lags inherent in the transmission of monetary policy (see Goodhart 1997).

This careful parsing of the roles and connections of elected governments and of independent central banks and regulatory authorities, with a clearly superior position for elected representatives in setting the framework and incurring any financial obligations, is characteristic of Goodhart's approach to policy and institutional design. For example, in his analysis of optimal currency areas and its application to Europe, he contrasts a market theory of money, in which money arises as basically a private sector effort to reduce transaction costs, to the cartel theory that links money to the State as issuing authority (see Goodhart 1998). The optimal currency area is built on a market view, so it is not necessary to have money tied to a government with fiscal authority. Historically, however, money creation and sovereignty have been closely linked, and Goodhart sees money as a government function, with money acceptance tied to fiscal policy and to the requirement to pay taxes in a particular currency. An optimal currency area, like the eurozone, consisting of several countries, breaks the tie between fiscal policy in sovereign nations and money creation in the currency area. The European Central Bank (ECB) is largely independent of any sovereign while fiscal powers

within the eurozone reside at the State level. As early as 1998, Goodhart saw the severing of that link in the euro area as a serious weakness.

In 1984, Goodhart left the Bank and returned to LSE as the Norman Sosnow Professor of Banking and Finance.⁴ In 1986–1987, he teamed up with Mervyn King to found the FMG at LSE to concentrate on research in money and finance. FMG was supported by contributions from City firms, whose interest in research had been elevated by uncertainty about the effects of recent deregulation and who saw the benefits of having a source for training bright recruits. FMG would maintain an arms-length relationship with the City and protect its reputation for objective analysis by not carrying out direct consultancy, and they insisted on doing research in-house, rather than serving as a collection point for research elsewhere. FMG has been an important and influential addition to the study of financial markets and related policies. By November 2017, FMG economists had published 766 discussion papers, frontier economics research that typically goes on to be published in professional journals, and had issued 247 special papers, which are generally less technical and more policy focused. Goodhart contributed considerably to the special papers, where he developed recommendations for monetary and regulatory policies.

Through the FMG and his own research and writing, Goodhart played a key role in bringing finance into mainstream economics. His study and experience told him that central banks were operating at the intersection of finance and economics as they conducted both monetary and regulatory policies where actions to achieve macroeconomic and financial stability objectives were transmitted through banking and financial systems. In addition, he saw that the shift to a more financial focus was how economists could make a greater contribution to understanding economic systems, managing risk in the private sector and improving the design of regulatory policies in the public sector, given recent developments in macroeconomics. Ten years after the founding of FMG, Goodhart wrote:

Despite greater formalism of economics now and the effects of the IT revolution in enabling us to access and analyze mountains of data, we do not really understand much more about, or feel any better able to predict, the macro-economy than in the early 1960s ... What has, instead, developed with great success has been the study of finance and the analysis of the relationship between risk and

⁴Teaching at a university was a natural place for Goodhart to land after the Bank. While still at the Bank he had written *Money, Information and Uncertainty* (Goodhart 1989), which had become a standard text for teaching intermediate level money and banking courses.

asset prices and the determination of risk ... Where economists can really help is in the analysis of risk. There are much more systematic and predictable fluctuations in the variance than in the level of asset prices (Goodhart 1997: 411).

For several years after returning to LSE, Goodhart's own work on financial markets centred on the foreign exchange markets. His observations of market behaviour while he was at the Bank suggested that price movements in reaction to news did not always conform to some of the major hypotheses about efficient markets. In particular, he saw reason to question whether markets overshoot and then were drawn back to equilibriums determined by fundamentals, as theory predicted. He recognised that testing these hypotheses required high-frequency data from an actively traded market, which suggested the forex market as a proving ground. His initial findings were presented in the Inaugural Lecture at FMG and its first discussion paper, 'The Foreign Exchange Market: A Random Walk with a Dragging Anchor' (Goodhart 1987). In that paper, Goodhart reported that markets tended to underreact to news, rather than overshoot, and that the pull of fundamentals was weaker than theory anticipated. Drawing on his knowledge of how financial market participants talked and acted, he attributed this to the existence of separate groups of speculators—one focused on fundamentals, and the other on random walks—both with limited resources and risk averse (anticipating the later 'limits to arbitrage' literature). The study of forex markets, like his early studies of banking in the USA and UK, illustrated Goodhart's insistence on exposing theory to data, on doing the painstaking work to find and collect the detailed observations that might best test the theory, and on linking the academy to the 'real world' of financial markets and institutions, to the benefit of both.

3 Goodhart on the Global Financial Crisis

The GFC, which began in late 2007 and gathered considerable momentum in the fall of 2008 after the bankruptcy of Lehman Brothers, provided an opportunity and a challenge to Charles Goodhart, engaged political economist. The crisis itself was an extraordinary episode of financial instability occurring at the juncture of the financial markets and the macroeconomy, where Goodhart had pioneered analysis; it revealed weaknesses in market functioning and regulatory oversight, areas that had long been the subject of his studies, and the response in dealing with the immediate effects of the crisis in reforming regulation and engaging in unconventional monetary policies mostly involved central banks, where he had his policy experience, and

which he had studied extensively. Well before the crisis, Goodhart's work at the FMG had come to focus on financial stability.

Seeing the Vulnerabilities: Goodhart's Per Jacobsson Lecture of June 2004, 'Some New Directions for Financial Stability?', was remarkably prescient about the tsunami that was to engulf the global economy four years later and the required regulatory and supervisory responses (Goodhart 2004). He noted that much progress had been made in designing policy regimes and supporting models for central banks to achieve their inflation objectives, but little had been done on the financial stability side, and dangerous gaps remained in the modelling of financial systems and in the design of regulatory tools required to achieve and maintain financial stability.

One design deficiency was in the very structure of decision-making on financial stability and the responses to financial crises. Supervision in many countries was being hived off from central banks into separate agencies, and crisis response would require considerable coordination between central banks with their lender of last resort function, the supervisory agencies, who presumably had information on the underlying viability of individual banks, and the fiscal authorities, who would need to be brought into provide capital in a severe bout of financial instability. More generally, designing and implementing policies to maintain financial stability were challenged by the lack of clear metrics for gauging financial stability and of well-defined instruments to build stability.

Another deficiency was in the research agenda. Successful financial stability policy would require economic models that encompassed the possibility of instability; this would require incomplete markets, heterogeneous lenders and borrowers, and most importantly the possibility of default; none of these characterised the models most in use at the time, though Goodhart and co-authors were working in this direction.

As noted, crisis management likely would require a fiscal backstop to stabilise banking systems. But for cross-border banks, it will be difficult to align the fiscal backstop with the source of the problems in globalised financial markets. This fiscal disconnect seems especially troublesome in the euro-zone where neither bank supervision nor fiscal responsibility was centralised at the time of the Lecture. Goodhart saw the ECB acting as an arbiter and organiser of cross-border rescues, a role it did indeed play in several instances during the crisis, but for which it came under criticism after the fact.

Most of the energy around international standards for banks had focused on capital. Insufficient attention had been paid to other aspects of risk, most especially liquidity. Minimum standards for liquid asset holdings had in the past been an aspect of oversight, but the pendulum had swung too far

towards capital, when the main threat to the system was runs and fire sales in which forced liquidation of one bank's assets drove down prices of assets held by other banks: '[M]aintenance of sufficient liquid assets by the banks protects the system as a whole from damaging fluctuations in asset prices when adverse conditions force banks to shrink their books' (Goodhart 2004: 10). Liquid asset requirements are a major addition to regulation after the crisis.

Importantly, the Lecture also featured Goodhart's advocacy of countercyclical movements in capital and liquidity requirements. He recognised the tendencies to procyclicality in the financial system, and he saw those tendencies as being accentuated by the shift to mark-to-market accounting. (Later, he wrote that regulatory risk weighting together with mark-to-market accounting was a '[d]oomsday machine for exacerbating leverage cycles' (Goodhart 2011a: 116).) His recommendation was to counter these tendencies by raising capital requirements in good times and making them high enough so that they could be safely released when boom turned to bust. This idea is now embodied in the countercyclical capital buffer of Basel III and the new emphasis on macroprudential policies in developed economies.

Goodhart ended the Lecture with: 'I recommend these new directions to you, without, I fear, much confidence that they will be followed' (Goodhart 2004: 14). Indeed, they were not before financial instability resulted in severe and prolonged loss of output and employment, which exposed for all to see the vulnerabilities that Goodhart already had identified and focused the authorities on many of the remedies he already had suggested.

The Causes of the Crisis: Once the crisis had occurred, Goodhart addressed its causes in many different writings. A common thread was the importance of real estate cycles intersecting with cycles in leverage and maturity mismatch in the financial sector (e.g. Goodhart 2013). The GFC was caused most fundamentally by the expansion of credit to finance a property bubble in commercial and residential real estate. The Achilles' heel of the financial system is the financing of long-term mortgages with short-term credit backed by very little capital. Dependence on short-term debt to finance mortgage lending increases in good times when optimism about real estate values abounds. Once the real estate cycle turns, lenders to financial institutions with mortgage exposures become more cautious; ultimately fears about potential losses at the financial intermediaries lead their short-term creditors to run to safety, sparking fire sales of assets by the intermediaries and contagion across lending institutions. This inherent procyclicality in the psychology of market participants is accentuated by the use by banks of mark-to-market accounting conventions and risk-weighting systems for assessing capital adequacy noted previously.

Although much of the questionable lending originated in retail banking in the USA, securitisation spread the US bubble into Europe, induced in part by capital arbitrage to European banks, which faced less severe restrictions on leverage than did American banks. Investment banks played a key role in all this, not only by fostering securitisation, but also by gambling themselves with wholesale-financed mortgage holdings on highly leveraged balance sheets. Of course, it was the failure of one of these investment banks, Lehman Brothers, that deepened and spread the crisis globally. Fire sale dynamics were not confined to banks; those dynamics were evident in securities markets, especially where loans for carrying securities were collateralised and the declining value of the collateral sparked margin calls that added to liquidity pressures on intermediaries and forced asset sales that drove prices down further (see Kashyap et al. 2011).

Moral hazard applying to the creditors of financial institutions—‘too big to fail’—did not cause the crisis; rather, it was undue optimism about real estate values. But market discipline could be enhanced by requiring more equity capital; equity was clearly at risk in any resolution regime, and added equity would bolster resiliency without risking added moral hazard. Moreover, fury at banker remuneration had driven the narrative too much. Incentives were important and needed to be better aligned with risk-taking, but skewed incentives did not cause the crisis—the real estate and financial cycles did.

The Regulatory Response to the Crisis: Much of Goodhart’s work from 2008 on has involved making suggestions for fixing regulation to prevent a reoccurrence of financial instability and evaluating the steps that were being taken by the authorities. He applauded much of what had been done but raised questions about the premises behind some of the actions.

In ‘How Should we Regulate Bank Capital and Financial Products?’, Goodhart identifies the reasons for regulation and lays out some general principles that should guide the response to the crisis. Initial approaches to regulation in the Basel I and Basel II bank capital regimes focused mostly on setting minimum standards and bringing lagging banks up to best practices (see Goodhart 2010). Those efforts had two underlying weaknesses: the methods for assessing capital adequacy might be fine for normal circumstances, but the techniques did not address tail risk—and that is where instability was generated, and the focus was on the individual institution, not the system, and in this regard, individual institutions looked healthy in early 2007 as reflected, for example, in minimal spreads on their credit default swaps (CDS).

Regulation is justified by two broad rationales, namely consumer protection and externalities. The need for consumer protection in the form of

deposit insurance arises from asymmetric information—the difficulty of assessing the true state of the bank, especially by unsophisticated depositors. Externalities arise from numerous amplification channels in financial markets, and amplification tends to be quite pronounced when firms fail. Bankruptcy is an especially costly form of failure with major externalities: resources are wasted, markets are dislocated, the expertise of the failed institution is lost, creditors face uncertainty about their losses, and borrowers lose access to credit. These externalities extend beyond the banking system, as was seen clearly with the failure of Lehman.

The limited liability of the owners of the institution is one source of externalities. Owners face asymmetric and distorted risks in that they cannot lose more than they have put up. The conversion from the partnership model of investment banking to the limited liability corporation in the 1990s changed the risk-taking culture of these businesses, opening them up to taking much riskier bets now that the downside was limited. Mitigating the distorting effects of limited liability could involve clawback of previously granted pay, requirements for additional capital to take care of stress, further regulation to control risk-taking and pricing of too-big-to-fail insurance.

Added capital should be required before stress materialises; the benefits of imposing higher capital requirements once stress hits could well be outweighed by the costs, which might entail forced deleveraging in an already weak business environment. More attention should be on liquidity backstops, including buffers that could be drawn down in stress.

Here and in other writings, Goodhart stresses two boundary problems facing the regulators as they try to protect against systemic risks. One is the boundary between banks and non-banking sectors. Just taking care of banks will not solve stability problems; tighter regulation on banks will encourage flows to less regulated sectors; crises can start in unregulated sectors and spill over to banks via fire sale dynamics. Attention must be paid to the whole financial system and where risks migrate.

The second boundary that challenges financial stability is that between sovereign States in the presence of cross-border banking and capital flows. Among other dangers, competition for business in different jurisdictions can induce a regulatory race to the bottom to keep the playing field level. Cross-border flows may constrain countercyclical policies aimed at growing imbalances in one jurisdiction. Moreover, without internationally agreed approaches, resolution of globally active intermediaries can be especially difficult and entail substantial spillovers that threaten financial stability in host as well as home countries.

Overall, regulation needs to shift from a focus on individual institutions to dealing with the externalities of the whole system emerging mainly from cycles in leverage and maturity mismatching. Capital and liquidity requirements need to be high enough to encompass the externalities and to include buffers that can be utilised as risks crystallise, with graduated sanctions imposed as the buffers are drawn down. Markets and regulators would benefit from greater transparency with the existence of a central repository for derivative data. If creditors cannot be reliably bailed in, the system needs more equity capital. Also, living wills to guide any resolution are critical to having a chance for orderly wind down outside of bankruptcy.

In ‘The Emerging New Architecture of Financial Regulation’, Goodhart evaluates the actions taken post-crisis against this analysis and criteria. This evaluation was made at the end of 2010, when the basic shape of the regulatory response had been established, but the details of implementation were still being worked out: ‘A touchstone for assessing whether the planned reforms to financial regulation are desirable is whether they will diminish the extent and volatility of the credit and leverage cycles’ (Goodhart 2011b: 5). His conclusion is that, evaluated at the end of 2010, ‘proposed reforms are incomplete and/or partially misdirected’ (*ibid.*: 2).

The capital and liquidity rules emerging from the Basel Committee on Bank Supervision were a major improvement, but still had some concerning limitations. Those rules were raising the levels and quality of bank capital required substantially, though whether they were high enough is difficult to determine. The introduction of countercyclical capital requirements was an important step forward in the direction of macroprudential policy designed to lean against leverage cycles, but were largely discretionary in the hands of national regulators, and would meet political resistance were they to be raised in good times. Capital buffers were part of the new regime, but not a ladder of increasingly tough sanctions as the buffers were utilised. Moreover, the capital requirements were deficient in not extending much beyond banks. The leverage cycle was the product of actions across the financial sector, and maintaining financial stability would require capital requirements wherever this cycle was manifest; although some systemically important non-banks might be subject to more regulation, broader requirements had not been imposed. New liquidity requirements are welcome, but as with capital, deficiencies exist in the treatment of buffers and the lack of increasingly severe sanctions as institutions drew down liquidity buffers.

As an economist, Goodhart would prefer to address financial stability risks through Pigovian taxes—pricing the externalities—instead of the complex array of much of the capital and liquidity regulation. The taxes

could escalate as risk increased, meeting Goodhart's desire for a ladder of sanctions as buffers were utilised and for a countercyclical thrust to policy. Unfortunately, although some taxes had been proposed on banks and financial transactions, none of them had been designed to damp the highly procyclical leverage cycle or to penalise especially risky activities. Indeed, by allowing the deduction of interest payments in the calculation of income taxes, the tax system itself encouraged leverage.

Prohibitions on banks exceeding a certain size or on their engaging in certain activities are equivalent to a 100% Pigovian tax on the prohibited size or activity. This approach—prohibitions—is not consistent with an economist's view of varying degrees of risk and externalities with taxes or sanctions keyed to those externalities. Moreover, such draconian 'taxes' will encourage the activity to shift to a more lightly regulated 'nontaxed' market or entity; it will not reduce the activity itself to a more socially optimal level. Narrow banking, for example, which has picked up advocates in the wake of the crisis and limits deposit-taking banks to very safe and low-yielding assets and activities, only moves the location of the leverage and credit cycles. It is those cycles, encouraged by widespread misperceptions of risk in the boom and bust, that are the source of systemic risk, not just activities that happen to be carried out inside something called a 'bank'.

The limited liability of financial companies—the constraint on downside costs to the owners—was one factor behind the excessive risk-taking that led to the crisis, leading Goodhart to be sympathetic to suggestions for reform of remuneration structures; too often, those structures are tied to shareholder returns and share the skewed incentives of ownership. Paying bonuses in subordinated debt or having unlimited clawback on bonuses would be steps in the right direction to mitigate the effects of limited liability. However, this is a collective action problem: no one firm can adopt more symmetric pay structures for fear of losing staff. Goodhart is skeptical that anything substantive will be accomplished.⁵

Bilateral over-the-counter (OTC) derivative trading was a source of systemic risk leading up to the crisis. It created complex and opaque interconnections, often backed by little or no collateral, until the viability of one party was threatened, in which event demands for collateral from that party exacerbated developing problems. The requirement that standardised trades

⁵Since the end of 2010, regulators in the EU, USA and other places have issued remuneration guidelines that generally include requirements that pay structures reflect safe and sound banking and appropriate risk-taking incentives and provide for clawback of bonuses over time if decisions later result in losses.

settle through central counterparties is a major step forward. A further step that would increase transparency and enhance efficiency would be to have those trades be undertaken in public marketplaces.

The regulatory response has focused on increasing the resilience of the financial system, but leverage among end-users—businesses, households and governments—is also a source of systemic risk. Mortgage debt has been a particular problem in repeated financial crises. In the boom part of the cycle, the terms of lending are eased: loan-to-value (LTV) ratios and loan-to-income (LTI) values climb, and requirements on borrower creditworthiness fall. The authorities need to be able to limit LTV and LTI ratios when conditions loosen excessively. It may well be difficult politically to do this—to ‘take away the punch bowl’ as the credit party is getting into full swing. To resist the political pressures under such circumstances, the authorities should have a plan announced in advance to act under specified conditions, a highly systematic approach that would carry a presumption that might be difficult to overturn.

Goodhart expresses his greatest reservations about emerging plans for crisis management. Crises are inevitable, given waves of optimism and pessimism applied to a financial system in which maturity transformation exposes intermediaries to a loss of confidence and access to funding. The resulting failure of a bank or other intermediary can entail considerable externalities by disrupting the credit and other functions of the financial system. Some aspects of the new approaches to resolution of a systemically important institution should be beneficial. Crises and failures are likely to occur suddenly, and living wills will help the authorities plan and manage the situation. International agreements on how to approach the failure of a cross-border firm will help to avoid asset grabs and other disruptive actions by national authorities. Best that any common approach to resolution be embodied in law, but the approach agreed internationally after Goodhart wrote this evaluation should go some way towards accomplishing the goal of common understanding and cooperation in a crisis.

Still, Goodhart has serious reservations about some characteristics of the special resolution regimes.⁶ First, resolution is likely to be triggered late, well after the value of the institution has disappeared, given the lags in information to indicate approaching insolvency and concerns about the possible systemic effects of failure. As a result of this delay, the losses in an institution are likely to exceed the value of shareholder equity—more than just the owners will need to absorb losses. In the past, in a crisis, taxpayers have

⁶The following discussion of creditor bail-in resolution draws on Avgouleas and Goodhart (2015).

footed the bill by injecting capital and protecting creditors, but the new regimes would avoid that by shifting the cost to some creditors or to other healthier banks, protecting the creditors of systemically important pieces of the banking organisation. 'Bailing in' creditors has at least two advantages: it would remove the too-big-to-fail subsidy and thereby reduce moral hazard and enhance market discipline, and it would mitigate the 'doom loop' in which doubts about bank solvency come to affect the perceived creditworthiness of governments that might be called on to support the banks.

However, these burden-sharing devices ignore the effects on the other banks in the system. If bank creditors are bailed in, contagion and amplified procyclicality are likely under systemic circumstances when confidence is impaired by more than an idiosyncratic event at a single bank. Other banks are likely to find it very expensive or impossible to roll over their own bail-inable debt as it comes due. Lenders to banks that are resolved or at risk of being resolved may have difficulty in differentiating the obligations at risk from those of the surviving newly capitalised operating companies, requiring considerable liquidity support until confidence can be restored. Lawsuits and their resulting uncertainty are sure to proliferate. Pension funds and insurance companies will bear the losses, which itself may have systemic implications. Systemically important institutions often operate across borders, presenting another set of complications for resolution by bail-in of creditors. Among other questions will be: how to determine which subsidiaries are solvent or systemic and which can be allowed to fail along with the holding company; where the bailed-in debt is held relative to protected deposits; and where the liquidity should be provided. Considerable planning and understanding *ex ante* and close coordination among authorities in several jurisdictions *ex post* will be required. Assessing other banks after the fact to repay any taxpayer funds used will weaken the stronger banks at the wrong time.

In sum, Goodhart doubts that sufficient attention has been paid to the disadvantages of bailing-in private creditors in a systemic event, and he wonders whether such resolutions will or should be attempted under those circumstances. Efforts to make the banking system and its creditors bear the burden are self-defeating—they will make the situation worse. In a true crisis of confidence, support from the sovereign and its taxpayers will likely be necessary to limit the damage and begin the recovery in a timely way.

Goodhart was also critical of some of the recommendations made in 2011 by the Independent Commission on Banking in the UK, in particular that deposit-taking and associated lending to British households and businesses be ring-fenced in a separate subsidiary from investment banking. He saw this recommendation as related to others made in the wake of the GFC to

move towards narrow banking in the spirit of the Currency School, which focuses on protecting current (transaction) accounts issued by banks as the key to financial stability (see Goodhart and Jensen 2015). Goodhart, by contrast, emphasises the critical function of the broad intermediation process, wherever it occurs. Separating money from near money and letting issuers of the latter fail will not protect financial stability, as was seen so clearly in the case of Lehman Brothers. The problem is financing mortgages with short-term liabilities. We need to make important intermediaries safer by requiring them to match more closely the maturity and liquidity of assets and liabilities, not draw artificial distinctions around narrow banking.

The administrative structure for implementing the new regulatory architecture has tilted in favour of more authority for central banks—and this seems appropriate, up to a point (see Goodhart 2011b). Central banks often have explicit or implicit responsibility for financial stability because this is closely aligned with the objectives of monetary policy for price and economic stability and because central banks have the lender-of-last-resort powers essential to stopping runs.⁷ Moreover, central banks are largely staffed by economists who have the professional expertise to monitor market interconnections and their potential macroeconomic consequences, and central banks usually have a high degree of independence from short-term political pressures, which is likely to be necessary to implement countercyclical macroprudential policy.

Whether central banks should have the authority to actually supervise systemically important or other institutions is a more open question. Most macroprudential actions utilise and build on microprudential regulation, and the macroprudential authority needs to have a detailed knowledge and good working relationship with the microprudential authority, but not every financial regulatory power needs to be inside the central bank. Goodhart suggests a microprudential supervisor housed within the central bank but separate from the monetary and macroprudential committees of the bank, and an entirely separate conduct regulator. In fact, this exact structure ended up being implemented in the UK in 2012. He also raises questions about where the resolution authority should be; here, he leans in favour of a

⁷In ‘The Changing Role of Central Banks’, Goodhart points out that the essence of central banks is ‘banking’, i.e. the use of their balance sheets to add and absorb liquidity (Goodhart 2011c). This they do with lender of last resort and open market operations. Central banks were institutions of financial stability long before they were monetary policy makers and, with the payment of interest on deposits at the central bank, liquidity and monetary policy interest rate setting can in theory be separated, though it would not be a good idea to do so.

separate authority outside the central bank because of the required coordination with the Treasury in a crisis, the difficult legal and accounting issues that would accompany resolution which would not be normal areas of expertise of the central bank and the high reputational and legal risks. In this area, however, the UK chose to lodge the resolution authority inside the Bank of England.

The more intrusive regulatory regime ushered in by the response to the GFC has consequences for the relationship of the central bank to the government (see Goodhart 2011c). The operational independence of the central bank with respect to monetary policy should be maintained, and monetary policy should continue to focus on achieving the inflation target, while macroprudential policies are used to foster financial stability. In maintaining financial stability, however, both the government and the central bank are likely to find more areas in which interests overlap and intersect and more areas for cooperation than they do for monetary policy. Tax structures (especially for banks), debt management, crisis management and resolution of systemic institutions are all subjects in which governments will take the lead and can have important bearing on financial stability and call for consultation and collaboration between the central bank and the government. There is no reason that the central bank cannot continue to be independent for the purpose of operating monetary policy as it works more closely with the government on financial stability.

Modeling with Default: An important aspect to Goodhart's approach to economics has been his insistence on basing his analysis in part on models that could be expressed in mathematical form so as to reveal underlying premises and assumptions and interactions, having those models reflect key aspects of the institutional and legal environment, and then bringing the models to the data to test their validity and usefulness. Well before the crisis, as early as 2004 (Goodhart et al. 2004), Goodhart and Dimitrios Tsomocos and others began a strain of modelling in which financial sectors played a key role, financial instability was endogenous, and approaches to detecting and dealing with financial instability could be derived and tested.

They were trying to correct some of the flaws in the dynamic stochastic general equilibrium (DSGE) models widely in use in central banks and academia at the time. As they point out in several articles (see, for example, Goodhart et al. 2013), those models assume no default and a representative agent; one consequence is that there is no diversity in tastes or creditworthiness with everyone borrowing and lending at the same rate. These models have no money, no credit and no financial sector—finance is a veil; they cannot address many questions about the effects of monetary policy, or the origin or effects of financial cycles and

their feedback on the real economy; and they give no guidance to policy makers as to the types of policies that might be followed to mitigate financial cycles.

Goodhart and his co-authors bring a more realistic institutional set-up to economic modeling. Echoing the founding philosophy of the FMG at LSE, these models reunite finance and macroeconomics; they reintroduce financial frictions into a model of the macroeconomy. The essential element of the model is default; agents may not meet their obligations—indeed, they will weigh the costs and benefits of defaulting (see, for example, Goodhart and Crockett 1970; Goodhart and Tsomocos 2012; Goodhart et al. 2013). In addition, markets are incomplete in the model so future states of the world cannot be hedged. Default and incomplete markets imply a need for money, credit and banks, and roles for liquidity and collateral. Borrowers and banks are heterogeneous: agents differ in risk appetite and probability of default. Among other things, this gives rise to an interbank market and interconnected institutions as channels of contagion: ‘It is only by constructing a mathematical institutional economics that one can study the economic system in a rigorous and analytical manner’ (Goodhart et al. 2013: 7), and these models should ‘provide a top-down approach for assessing stresses in financial markets and in helping to design regulatory reform’ (ibid.: 31).

Agents play a two-period game in which the distribution of possible outcomes in period two is known, but not the actual result. Cycles are generated, and instability is endogenous. Banks are required to hold capital and will be penalised for falling short of requirements, so, like borrowers contemplating default, they will weigh the costs and benefits of meeting the capital requirements. In setting the requirements, authorities are faced with trade-offs between stability (control over risk through capital requirements) and efficiency and growth. Both regulatory and monetary policies are non-neutral and affect the distribution of income and wealth among heterogeneous households.

The model is very complex and does not settle down to equilibrium without further constraints being imposed. However, such constraints can be fashioned to fit the particular problem to be interrogated. Goodhart and his co-authors used this approach to illustrate and address several issues.

For example, in ‘Modeling a Housing and Mortgage Crisis’ (Goodhart et al. 2010), they incorporate housing and mortgage markets and first-time home buyers into their model to replicate some of the aspects of the 2007–2008 crisis. Their results largely are aligned with intuition: when banks become less risk averse, default in a subsequent bad state of the world (decline in housing prices) is higher and the odds on a crisis greater; liquidity

assistance by the central bank reduces default and helps the real sector by easing credit conditions for households and first-time home buyers.

In ‘The Optimal Monetary Instrument for Prudential Purposes’, they use the model to explore the interactions of monetary policy and financial stability (Goodhart et al. 2011). They calibrate the model to UK banking data and ask which monetary policy tool—interest rates or base control—best protects financial stability against a variety of shocks. It is interest rates that tend to absorb these shocks, while base control tends to amplify their effects on the financial sector and the economy.

In ‘A Model to Analyse Financial Fragility: Applications’, easier monetary policy in the form of an increase in the money supply can increase financial fragility because the associated increase in lending reduces capital ratios, more than offsetting the positive effect of lower interest rates on default (Goodhart et al. 2004). With heterogeneous borrowers and lenders, the effects of tighter regulation—an increase in capital ratios—very much depend on who is affected. Agents with alternative sources of credit to banks can push the costs onto counterparties; banks and agents without alternatives cut back lending and borrowing. Unlike in a DSGE model with representative agents, the effect of shocks depends very much on the characteristics of who is being shocked.

Macroprudential policy suffers from not having an observable measurement of financial stability, unlike the inflation measures that give feedback on the success of monetary policy. In a model with default and instability, the key metrics are related to the probability of default and the ability of the banking system to absorb those defaults (see Goodhart 2011b). Goodhart looks across seven industrial countries with a two-factor model to predict the effects of instability on GDP; one factor is the probability of default derived from an IMF measure of distance to default, and the other is the profitability of banks as captured by changes in bank equity prices. Empirically, the probability of default factor is most important, and both show important non-linearities—threshold effects with little effect below that threshold.

In 2008, a good part of the instability originated outside the banking system, in the so-called shadow banking system where long-term lending also was financed by short-term borrowing. Goodhart and his co-authors extend the model to address fire sales, credit crunches and defaults across securities markets (Kashyap et al. 2011). A channel for these externalities is the repurchase agreement (RP) market, where short-term credit is often extended based on the value of long-term collateral. When house prices fall, the value of the collateral declines, the RP can be defaulted on, and the underlying

mortgage asset sold in a fire sale. Capital and liquidity regulation of banks is not enough to avoid credit crunches, fire sales and defaults. Bank regulation must be supplemented by controls on RP margins to create cushions that prevent fire sales by non-bank financial institutions.

Monetary Policy at the Zero Lower Bound: Although much of Goodhart's focus after the GFC was on the weaknesses in financial systems and the efforts of authorities to remedy those weaknesses, he also addressed the effectiveness—or lack thereof—of the unconventional monetary policies undertaken when short-term policy rates approached zero. His evaluation drew on several themes of his long-standing approaches to macroeconomic analysis: It was focused on the intermediation functions of banks and financial markets; it emphasised that policy effectiveness would need to be judged by its effect on growth in the quantities of money and credit, not just on interest rates and asset prices; and standard DSGE models were not useful for evaluating the effects of unconventional policies.

Empirical modelling of the effects of quantitative easing (QE), using vector autoregressions (VARs) and data on portfolio allocation, suggested that the initial round of bond purchases had some, albeit not large, effects in terms of narrowing spreads of private debt over government bond yields and putting downward pressure on the foreign exchange value of the currency, providing a boost to the economy and inflation (see Goodhart and Ashworth 2012). Subsequent rounds, however, appeared to have had little net positive effect, perhaps because there were offsetting negatives. Very low rates were increasing pension deficits, possibly discouraging investment as firms directed cash into addressing these deficits, and for some households—especially those nearing retirement—the adverse income effect of low rates was probably more than offset by any positive effects of low rates on wealth or on the incentive to substitute current consumption for saving and future consumption.

But the central perspective of Goodhart's comments on unconventional monetary policies has been to ask why the huge increase in the monetary base from QE was not reflected in a significant increase in money or bank credit—why the money multiplier had collapsed (see, for example, Goodhart 2015).

Goodhart advanced several reasons why banks might not be making loans and competing for deposits despite holding large volumes of reserves at central banks, i.e. why the transmission channels were broken. Importantly, incentives to make loans had been held back by central bank actions—paying interest on deposits at the central bank and buying longer-term debt to flatten the yield curve. Returns on central bank deposits, even at very low interest rates, compared favourably to loans to households and businesses,

which looked especially risky in a recession or slow-growth economy. Flatter yield curves reduced incentives to maturity transformation at the heart of commercial banking. Moreover, central banks and other authorities had greatly increased capital requirements, raising the cost of funding loans. The banks reacted by reducing cross-border loans as they were under pressure to increase lending at home. But, as a result, increases from home-country lenders were offset by decreases from overseas institutions.

The remedies to make policy more effective flowed from the diagnosis. Reduce the rate paid on deposits at the central bank, perhaps to below zero, but only on marginal deposits to insulate bank profits. Banks that would not or could not raise new capital to bolster ratios without deleveraging should be forced to take government capital with restrictions on dividends and compensation to avoid moral hazard. To increase incentives to lend to private parties, the government should consider programmes to reduce the risk of such lending, like the Help to Buy programme in the UK that guaranteed mortgages for first-time home buyers. Also, central banks and governments should think about subsidised lending to banks keyed to the volume of their private loans, like the Bank of England's Funding for Lending Scheme. More broadly, economic expansion needed to be supported by fiscal and structural policies, with less reliance on monetary policy when intermediation was impaired and interest rates were close to zero.

4 Conclusion

Through his studies and analysis, Charles Goodhart has greatly advanced our understanding of financial markets and the macroeconomy, their interactions and the effects of these interactions on financial and economic stability. The application of this analysis by Goodhart and countless others has improved the practice of central banking and financial regulation, enhancing the ability of central bankers and other authorities to dampen cycles and increase public welfare. He has more than fulfilled the promise that he saw at Cambridge at the outset of his engagement with economics that he 'might be able to contribute, and...do so in a socially worthwhile manner' (Goodhart 1997: 389).

But, as he noted at the same time, 'economics was fun and a challenge because it seemed so unsure of itself (so bad). Despite the formal models, no one really knew what determines the level or rate of growth of most of the key economic variables' (ibid.: 388). Although Goodhart's work has pushed out the frontiers of knowledge, it has also pointed to questions arising out of

the experience in and after the GFC in which understanding is incomplete and further study holds the potential for greater advances in public welfare. They include:

1. What are the causes of financial instability—the deep underlying causes embedded in human behaviour and institutions? How can we further develop the theoretical and empirical models of this behaviour and these institutions to identify usable, forward-looking, metrics of risks to financial stability and to evaluate the costs and benefits of policy approaches to mitigating these risks? How do we need to adjust current policies and institutional structures in a cost-effective way to minimise the odds on future crises?
2. What do governments and central banks need to do to minimise the adverse effects of the next financial crisis when, inevitably, it does occur? How can resolution regimes with creditor bail-in for systemically important institutions be structured to meet a cost–benefit test under the widest set of circumstances? What policies should be put in place to anticipate the possibility that under some circumstances in a crisis the costs of resolution with bail-in will exceed the benefits? How can resolution and any back-up policies be structured to deal with the boundary problems of systemically important activities crossing institutional, market and national boundaries?
3. Why have the unconventional monetary policies put in place when policy interest rates approached zero not been more effective in promoting growth and achieving inflation targets? What steps should be taken to foster greater and more certain transmission of these policy impulses to financial markets and spending?
4. How should central banks be designed to best fulfil their responsibilities for financial and price stability? How should responsibilities be apportioned between elected governments and independent central banks to both promote welfare-enhancing outcomes and preserve democratic legitimacy? Should the parameters of this apportionment vary by category of responsibility and, if so, how can this be designed?

In all these areas, Goodhart's body of work has provided enormously helpful advances but, as he would be the first to acknowledge, the work programme he has done so much to establish is far from complete and will benefit from further advances by him and the many other economists and policy makers building on the foundations he has laid.

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32

Meghnad Desai (1940–)

P. N. (Raja) Junankar

1 Introduction^{1, 2}

Meghnad Desai has had a distinguished career as an economist, a political scientist and a politician: he is a Renaissance man. He has published numerous books and academic papers on a variety of topics, including Marxian economics, agricultural economics, economic history, economic theory, development economics, macroeconomics, etc. Besides his academic interests, he has been involved in movies and theatre. Meghnad has always been concerned with social issues. He was born in India and did his undergraduate education in Bombay, his doctorate at the University of Pennsylvania, and

¹A declaration: I first met Meghnad in 1965 soon after he had joined LSE. I was a graduate student at the School, and we spent many an hour in the Three Tuns student bar. He had suggested that I should work on technical change in Indian agriculture for a PhD. However, I did not take up his suggestion. Many years later, he was an external examiner for my doctorate entitled 'Essays on Indian Agriculture'.

²I am grateful to Meghnad for sending me draft chapters of his (incomplete) autobiography. Thanks to him for making some minor corrections to an earlier draft of this chapter. Unfortunately, he did not provide me with his curriculum vitae with a list of his publications. I am also grateful to Jim Thomas (LSE), David Hendry (Oxford) and Brian Henry (Cambridge, Oxford, LBS, NIESR) for providing me with information about Meghnad that helped in writing this chapter. Thanks to Geoff Harcourt and Peter Kriesler for comments on an earlier draft and to my wife, Susie, for helping correct my English.

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began his academic career in the Agricultural Department at the University of California, Berkeley, before moving to the London School of Economics (LSE). After retiring from LSE as Professor Emeritus, in 2015 he collaborated with some of his former students to set up an academic institution in Mumbai, India, the Meghnad Desai Academy of Economics (MDAE).

In what was a relatively conservative place (in terms of dress and demeanour), this irreverent academic, Meghnad, stood out at LSE with his huge bird's nest of curly hair.³ Unlike many of the academics at that time, he mixed with students and staff in the bar as well as at parties. He also invited us to his home for drinking parties, and he came to our home to our parties.

Although an academic, Meghnad became an active member of the Labour Party and was Chair of the Islington South and Finsbury Constituency Party from 1986 to 1992. In 1991, he was made a Life Peer: Lord Desai of St. Clement Danes.

For many years, he retained his Indian citizenship, but finally in 1977 became a British citizen. In 1970, he married his first wife, Gail Wilson, and had three children with her. He married his second wife, Kishwar Ahluwalia, in 2004. In 2008, Meghnad was awarded the *Padma Bhushan* (the third highest civilian award) by the Republic of India. He now spends several months every year in India and keeps an eye on his academy.

Meghnad has published several books (not only on economics) and numerous papers on applied econometrics, economic theory, economic history, development economics, economic history and Marxian economics. His economic life began as a Keynesian economist, was influenced by Marx's economics and in later years appears to have moved into supporting the neoliberal camp in proposing 'austerity' policies for the economies that were savaged by the Great Recession. However, his support of austerity policies is apparently based on the idea that the economy goes through a Kondratiev wave.

Meghnad is Indian by birth and British by naturalisation. However, his many years in the USA have left him with an American accent!

2 Early Years

Meghnad Desai was born in Baroda (Vadodara), Gujarat, India, on 10 July 1940, the youngest child of his parents (mother) Mandakini and (father) Jagishchandra. His family were Brahmins, upper-caste Hindus, a small

³It is surprising that Meghnad declares that he never faced racial discrimination in the UK. As I am also an Indian who lived in London, Durham and Colchester, I faced discrimination in housing and on the streets in Britain.

land-holding landlord family with limited means. Sometime later, the family moved to Bombay (Mumbai). His father became a civil (public) servant in the Bombay Government Service. Meghnad's family taught him English even before he started school and he commenced university at a lesser known place called Ramnarain Ruia College to study arts at the precocious age of fourteen. He was a voracious reader and used the college library 'religiously' (Desai unpublished),⁴ while also becoming involved with the theatre crowd at university and producing some plays. Eventually, Meghnad decided to study economics for his BA degree. After graduation, he did some part-time tutoring in economics, then studied for an MA degree and registered for a PhD at Bombay University.

While at university in Bombay, he became involved in politics and was impressed by J.P. Narayan, who launched the Inaugural Meeting of the Swatantra Party. The Party was against the dominant Congress Party led by Jawaharlal Nehru that was setting up a socialist society. However, once he went to America, Meghnad was affected by the Free Speech Movement and became involved in the anti-Vietnam War protests at Berkeley, moving significantly to the political left.

Thanks to an American Visiting Professor (Charles Whittlesey) from the University of Pennsylvania who encouraged him to travel to the USA to do his doctorate, Meghnad sat the (American) Graduate Record Examinations (GRE). With a Ford Foundation Scholarship, he was admitted to the PhD programme at the Economics Department of the University of Pennsylvania with Professor Lawrence Klein as one of his supervisors. That was his introduction to econometrics which was to be his forte for the next few years. His thesis was on the world tin economy, some of the results of which were subsequently published in *Econometrica* (Desai 1966). After leaving Penn, he was appointed to a research position in the Department of Agricultural Economics, University of California, Berkeley.

Meghnad made many friends during his stay in the USA, including Dale Jorgenson at Berkeley and Amartya Sen, then a visitor to Berkeley. These connections proved to be important for his future academic development.

3 LSE: A New Beginning

In 1965, Meghnad joined LSE as a Lecturer on the princely salary of £1400 per annum (plus a London Allowance of £60), having given up a position in the USA that paid \$9000!⁵ He was promoted to a Senior Lectureship in

⁴Meghnad had been areligious for some time as a young man, but subsequently became an atheist.

⁵Had he stayed in the USA, he would have obtained a Green Card but then been eligible for the draft—he decided that he would rather go to the UK!

1977 and in 1980 to a Readership. Finally, in 1984 he was promoted to a Professorship.⁶ He believes that the delay in being promoted to a Professor was because he was 'doing too many things' (Desai, unpublished) and not specialising in a particular field. Given that he was working in different fields and in areas like Marxian economics, he did not have a 'godfather' figure at LSE. However, he did have Lawrence Klein writing references for him.

Meghnad's entry into LSE in 1965 was helped by his forthcoming publication of an econometric model of the world tin economy in the prestigious journal, *Econometrica*, and with the support of a future Nobel Laureate, Lawrence Klein, from Pennsylvania. He was attracted to move to the School in order to join the outstanding professor of econometrics, Denis Sargan. He became part of the recently formed group at LSE that had started a master's degree in mathematical economics and econometrics, including with Frank Hahn, Terence Gorman, Bill Phillips and Sargan. Along with Sargan, Jan Tymes and Phillips, Meghnad helped to run the Quantitative Economics Seminar for graduate students. Meghnad is a many-faceted economist: although he began his career as an applied econometrician, he moved on to extend his research interests into different fields of economics: macroeconomics, development economics, economic history and Marxian economics.

It is difficult to categorise Meghnad as belonging to a particular camp at LSE. Although he had joined the School as an applied econometrician, his subsequent adventures into what might be regarded by some as dangerous areas, like Marxian economics, as well as his sympathy for the students during the 'troubles at LSE' during the late 1960s, must have led to some of the conservative professors into thinking that Meghnad had entered LSE under false pretences. Perhaps, that was one reason why he had to wait a long time to get promoted.⁷ However, Meghnad was always a likeable colleague and happily mixed with the more conservative academics, like Professor Peter Bauer. Indeed, he collaborated with a close colleague of Bauer's, Dipak Mazumdar (also not a left-wing economist), publishing a paper on disguised unemployment in developing countries (Desai and Mazumdar 1970).

⁶In the 1960s and 1970s, most British academics did not publish many papers. When Harry Johnson joined LSE (spending six months of each year at LSE and six months at Chicago), he began the 'Americanisation' of LSE academics with the 'publish or perish' theme. Meghnad, compared to his colleagues, was a prodigious publisher.

⁷However, he did get tenure at the end of his three years' probation.

4 Meghnad as an Applied Econometrician

As already mentioned, Meghnad had achieved academic success with his 1966 publication in the leading journal, *Econometrica*. In the Quantitative Economics Seminar for graduate students at LSE, he was involved in the debate between Phillips and Sargan about the Phillips curve and the alternative concept of ‘real wage resistance’ that Sargan had introduced in 1964 in his path-breaking paper (Sargan 1964): ‘They had erudite arguments about autoregressive and moving-average representations, matching Denis’s and Bill’s respective interests’ (David Hendry, private correspondence, 5 September 2016).

He collaborated with Brian Henry working on a model of the UK economy to study the problems caused by so-called stop-go policies. This was related to the pioneering work done by Phillips on optimal control of a dynamic economy where he had used his knowledge of electrical engineering. In studying whether fiscal policies could be destabilising, Desai and Henry (1970) and Henry and Desai (1975) build a model with single lags and calibrated the parameters governing the fiscal policy responses. Apparently, Sargan was critical of this modelling approach and suggested that they should use a continuous time modelling approach (Brian Henry, private correspondence, 9 September 2016).

Along with these illustrious econometricians, Meghnad collaborated on winning a five-year grant from the UK’s Social Science Research Council (the forerunner to the Economic and Social Research Council) for research into macroeconometrics. At this stage, Meghnad was comparing Keynesian and monetarist models of inflation. He published several papers that criticised the monetarists and subsequently published a book in 1981, *Testing Monetarism*. He also published an interesting, if idiosyncratic, interpretation of the Phillips curve in 1975, which was criticised by Chris Gilbert (1976). Thanks to Meghnad’s interest in cricket, he met up with Malcolm Falkus, an economic historian at LSE, and became involved in research linked to a debate with Michael Postan at Cambridge about the role of population growth and famines. This led to a publication in the *Bulletin of Economic Research* in 1991. Although this was an ‘invasion’ into another field, it was really the use of econometric methods in economic history, or what is now called cliometrics.⁸

⁸David Hendry, in private correspondence (5 September 2016), says that although Meghnad used the Hendry PC-GIVE econometrics computer program, the diagnostics always rejected the empirical specifications of most researchers and he [Meghnad] called it a ‘model destruction program’!

As well as his other work in applied econometrics, Meghnad was a co-editor of the *Journal of Applied Econometrics* from 1984 to 1991, and he published a textbook on *Applied Econometrics* (Desai 1976a). He also edited a series of lectures delivered by Sargan (1988).

5 Meghnad and Marx

While Meghnad was at university in Bombay, he was more inclined to be on the conservative side of politics, but he had read Schumpeter, Marx and Hegel. While at Berkeley, he became involved with radical students protesting against the Vietnam War and began reading more of Marx. He states that he was 'Marxist in my thinking on the long run dynamics of Capitalism but wary of State control of the economy' (Desai, unpublished: 65). He was clearly against the Leninists among the left-wing economists.

Meghnad's reputation spread with his important contributions to the Marxist literature, first with his book *Marxian Economic Theory* (Desai 1974) followed by several papers on Marx's value theory and on cycles. He made important contributions to an understanding of Marx's economics and developed his ideas in his extension of the Goodwin 'predator-prey' model of business cycles in his paper in the *Journal of Economic Theory* in 1973 (a purely theoretical paper in what was a move away from his usual applied econometrics). It is interesting that Meghnad did not get caught up in the Sraffa debate where many Marxist economists became strong supporters of the Italian. As Meghnad was not a Sraffa advocate, he was not popular with many of the Cambridge economists. However, he got on well with Geoff Harcourt and even wrote a Foreword for one of his books.

Meghnad's reputation as a left-wing academic thrust him into the student revolt at LSE. In October 1968, following on from the anti-Vietnam War demonstrations, the students decided to occupy the School. As Honorary President of the Students' Union, Meghnad chaired the Union Meeting which went on throughout the night to decide whether to occupy LSE. Over the Christmas break, Walter Adams, the then Director, installed security gates, supposedly to prevent a further occupation by students. In January 1969, some students removed the gates, which led to the police being called on to campus (at that time at least, the police could not enter university premises in Britain without the authority of the university itself). This led to a further massive revolt. LSE was closed for some time, and poor Meghnad had to move his computer to his home to continue his research with Henry. The student revolts in fact led to an increase in demand for

the teaching of Marx, and Meghnad decided to provide a set of lectures on Marxian economics, which in turn provided the basis of *Marxian Economic Theory*.⁹

In 2002, Meghnad published *Marx's Revenge*, a controversial and provocative book, which received praise from some notable quarters, including Amartya Sen and Robert Skidelsky.¹⁰ The volume takes a sweeping look at Marx's theories and the growth and demise of the Soviet economies of the USSR and Eastern Europe. It is critical of central planning and State control in these economies. Meghnad argues forcefully that Marx would have rejected Soviet-style economies, was an advocate of free trade and also a supporter of free markets and thus against State control of industries and central planning. However, a serious student of Marx knows that he was studying capitalist society as it existed: he did not propose policies to mitigate the evils of capitalism.¹¹ While he was carrying out his research for *Capital* (remember that Volumes II and III were published after his death with the collaboration of Friedrich Engels, his financial supporter and political colleague), he was a social scientist, not a political activist. Marx certainly did not recommend that capitalist countries *should* move towards free trade and free markets! Meghnad would like us to believe that Marx was a neoliberal! The political activism was to be seen in Marx and Engels's *Communist Manifesto*. Meghnad argues that the collapse of the Soviet system does not refute Marx but vindicates him because Marxists had tried to end capitalism prematurely before it had reached its productive potential. It is reported that Marx once said, 'I am not a Marxist'. He was rejecting some of the writings of the so-called Marxists. Recently, Gareth Stedman Jones has argued that Marx was a theorist while it was Engels who in fact rewrote Marx to be more interventionist in the economic sphere. As Marx scholars know, Marx argued that the capitalist system would go through crises (business cycles, in today's language), and the Global Recession of 2008 and subsequent years could be thought of as the realisation of one of Marx's predictions.

What can one make of Meghnad's statement that capitalism 'is the best arrangement for the alleviation of poverty and misery' (Desai 1974: 304) when there are millions of people living in poverty in capitalist India while communist China has made significant strides in reducing poverty using

⁹As a political commentator, Meghnad also wrote regularly for the British radical weekly, *Tribune*.

¹⁰For a critical review, see Varoufakis (2005); see also Junankar (2012).

¹¹For a critical analysis of Marx's writings, see Junankar (1982).

a managed market system? Again, he argues that ‘global financial markets act as a powerful discipline on individual states, yet are volatile and fragile’ (ibid.: 304). As the world is still struggling to recover from the financial crisis, we can hardly argue that the financial markets acted as a powerful discipline on the USA or the other OECD economies!

6 Meghnad, Development Studies and Global Governance

From 1991 to 1995, Meghnad was the Director of the Development Studies Institute (DESTIN) at LSE. The Institute’s mission was to promote interdisciplinary postgraduate teaching and research on processes of social, political and economic development and change. The Institute was dedicated to understanding problems of poverty and late development within local communities, national political and economic systems, and in the international system. It was set up to attract students to their MSc programme. Besides his work at DESTIN, Meghnad had been publishing several papers on development economics using econometric methods. He published a paper with Mazumdar on testing for the existence of ‘surplus labour’ in Indian agriculture published in *Economica* (1970). A paper on the measurement of poverty (Desai and Shah 1988) extended and formalised Peter Townsend’s (1962) measure of relative deprivation and was highly cited. Meghnad also made an interesting contribution to the United Nations *Human Development Report 1990* and the creation of the Human Development Index.

LSE Global Governance (initially called the Centre for the Study of Global Governance) was set up in 1992 to increase understanding and knowledge of global issues, to encourage interaction between academics, policy makers, journalists and activists, and to propose solutions. Meghnad was the first Director of the Centre, resigning this post in 2003. Subsequently, Mary Kaldor and David Held were joint Directors. LSE Global Governance closed in 2011 after an inquiry into the School’s links to Libya. A large donation of £1.5 million from the Gaddafi International Charity and Development Foundation was made to LSE in 2009.¹²

¹²The Woolf Inquiry in 2011 reported: ‘I [Woolf] have set out a number of failings in this Report, but would like to make clear that I am satisfied that the evidence does not show that any of the academics or staff at the LSE acted other than in what they perceived to be the best interests of the School’ (Woolf Inquiry 2011: 129).

Nevertheless, Meghnad published various papers on globalisation during the period of operation of LSE Global Governance.

In a celebration of Meghnad's seventieth birthday in 2010, LSE Global Governance invited a panel made up of Amartya Sen, Charles Goodhart, Clare Short (former Secretary of State for International Development in the Blair government) and Purna Sen, a former student of Meghnad's, to discuss his many and varied contributions. All of them gave glowing tributes. Mary Kaldor, who presided over the panel, praised Meghnad, saying: 'He is a real globaliser: for free trade, free capital movements, free labour movements, and human rights'. Nicholas Stern was unable to attend but sent a moving tribute to Meghnad (see LSE 2010).

7 Meghnad and India

Meghnad has always had a close relationship with India and the Indian economy. He was an academic Visitor to the Delhi School of Economics (DSE) while Professor Sukhamoy Chakravarty was, I believe, Head of the DSE. Sukhamoy encouraged Meghnad to teach a course on dynamic economics which led to his research into Goodwin cyclical models of growth. Meghnad's collaboration with Dipak Mazumdar led to his studying Indian agricultural problems. He studied the politics of India, a multilingual and multicultural nation, publishing his book *The Rediscovery of India* in 2011.¹³ He published an article about the role of the various Marxist parties in helping the poor and underprivileged in their political struggle against the Congress Party. In concluding, Meghnad writes: 'There are new opportunities for revolutionary socialism in India now, because of the fluidity created by the creeping collapse of the old political order' (Desai 1970: 60). I imagine he would now reject his earlier views even though India now has a campaigning free market Hindu government led by Narendra Modi.

Along with Dharma Kumar, Meghnad edited the second volume of *The Cambridge Economic History of India* (Kumar and Desai 1983). It is widely accepted as a standard work of reference on the subject of economic development in India from company rule to the 1970s.

While in India, Meghnad renewed his friendship with Amartya Sen and Professor I.G. Patel (a fellow Gujarati from Baroda, a neighbour known to his family) who was subsequently to become Director of LSE.

¹³This is obviously a play on words on Jawaharlal Nehru's classic *The Discovery of India*.

Meghnad was offered a position as Secretary of the Agricultural Prices Commission by Sen,¹⁴ but decided he wanted to remain an academic. He was awarded the *Padma Bhushan* in 2007, the *Pravasi Bharatiya Puraskar* (Distinguished Overseas Indian Award) in 2004 and the *Bharat Gaurav* by the Indian Merchants' Chambers in 2002.

After he retired from LSE as an Emeritus Professor, Meghnad set up, as already noted, the MDAE in Mumbai, India. As the Academy's website notes:

The Post Graduate Program in Economics and Finance at the MDAE is a carefully crafted course with real world applications under the supervision of renowned experts in the field. This course will move students from the traditional way of thinking to critical thinking in the MDAE way (MDAE 2015).

During his annual stays in India, Meghnad is a regular columnist for the *Business Standard*, *The Indian Express* and *The Financial Express*.

8 Meghnad and the British Labour Party

Meghnad has been an active member of the British Labour Party and was a member of the General Management Committee of the constituency Labour Party. He was Treasurer, Secretary and finally Chair of the Islington South and Finsbury Constituency Party from 1986 to 1992. In the 1980s, Neil Kinnock asked Nicky Kaldor to convene a group of economists with left-wing sympathies to formulate a programme for the Labour Party and had Tony Atkinson, Nick Stern and Christopher Allsopp to help. Meghnad was also invited to help.¹⁵ At some point when Michael Foot was the leader of the Labour Party, Meghnad was co-opted onto the Constitution Sub-Committee of the National Executive Committee of the (national) Labour Party. Unlike Michael Foot (who wanted to abolish the House of Lords), Meghnad was interested in reforming the Lords and eventually was made a Life Peer. While in the Lords, he has been an active member of various committees.

Although Meghnad was a member of the frontbench of the Labour opposition in the Lords, he has been sacked three times! He obviously maintains his position as someone who thinks independently and loves to annoy and provoke.

¹⁴This is mentioned in Meghnad's draft autobiography.

¹⁵This is based on Meghnad's draft autobiography.

9 Meghnad, Theatre and Movies

Meghnad has had a long-standing interest in the arts. As a young student in Mumbai, he took part in producing plays. While at LSE, Max Steuer found out about Meghnad's theatrical interests and persuaded him to direct a Kafka play. The production was presented at Max's friend's house with a decent audience of about fifty people.¹⁶ In more recent times, Meghnad has published a book about his favourite Indian actor, Dilip Kumar. Meghnad has always been keen on films and is particularly enthusiastic about the French New Wave (*La Nouvelle Vague*). Recently, he played a small part in a Bollywood English–Bengali movie, *Life Goes On*.

10 Meghnad the *Provocateur*

No story about Meghnad can be complete without mentioning that he began his academic life as a Keynesian, but after the Great Recession went out of his way to argue against his erstwhile colleagues about the proper way of tackling the crisis. He argued strenuously against the Keynesians and for strict austerity measures in the middle of the crisis. Meghnad claims that his policies are based on Marx, Kondratieff, Schumpeter, Hayek and Goodwin.¹⁷ Since he is unable to provide any clear theoretical or econometric model for his policies, one has to take it on trust (or not). I, for one, find his arguments unconvincing.

Perhaps, he is just trying to provoke his colleagues. If he was true to Marx, he would have to argue that since a capitalist system is inherently unstable, there is no cure for crises. They are just inevitable. If he believes in Kondratieff's long cycle, then all we have to do is wait for the upturn which would come about eventually. However, in the meantime, millions of people will be unemployed, suffer declining living standards and despair.

11 Conclusion

Meghnad is an incredibly well-read and well-rounded man. He has a wide set of interests and a very creative mind. He has a wonderful way of making friends, even with people who disagree with him. He has opened many windows of thought at LSE and brought much excitement to a relatively quiet establishment.

¹⁶I am grateful to Jim Thomas for providing me with this information.

¹⁷See his recent book, Desai (2015).

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33

Nicholas Adrian Barr (1943–)

Stuart Astill

1 Introduction

This chapter sets out what I have chosen to present as the ‘parallel Barrs’: Nick Barr’s contributions as an academic, his policy work and its influence, his collaborations both orthodox and less so, the dimension of being a good LSE citizen, and the very human aspect of Nick himself, including a happy family life, that runs through these. In the UK and across the world, his work encompasses early adventures at the cutting edge of the new economics of information to create an at-the-time undefined economics of the welfare state; cutting through myths about pensions and reconstructing the space with Peter Diamond; and bringing clarity amidst controversy to the economics of higher education finance.

Nick Barr is not the most conventional of academics: motivated by the usefulness of applying analysis and allowing an interesting policy question to lead him where it will, he is refreshingly unconcerned by academic or public limelight and un-careerist. His most read work, *The Economics of the Welfare State*, is also the most wide-ranging, but it would be unduly restrictive to consider it as a full representation of his output. Nonetheless, it reveals Nick as a fully functioning political economist and gives an immediate flavour

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of his *modus operandi*. The political chapters sandwich some powerful economics: the distinction between aims and methods—separating the what and the how (Barr 1987: 337)—is at the core. Nick offers context and theory as vehicles to uncover efficiency and sustainable social justice. ‘Pursuing goals of equity informed by the answer to the efficiency question’¹—if we waste our energy and resources on a badly thought through pursuit of more equitable outcomes, we are doing no good.

1.1 Early Life

Nicholas Adrian Barr, widely known as ‘Nick’, was born in London on 23 November 1943. He had a happy childhood and youth in London and kept his base there, in recent years dividing his time between London and Somerset, where high-quality grand-parenting takes place.

Nick’s home, school and social life during his early years all shaped his future. At primary school, Nick won a scholarship to University College School, where he found himself surrounded by other good scholars and so ‘clearly no longer the brightest kid on the block’. From his school days comes Nick’s love of the great choral works, one of which the School put on for public performance each Easter.

His parents were always supportive—encouraging, but never forcing his hand. From a typical story, we see, with one deft act of parental support, Nick starting on a balanced path towards his twin anchors—LSE at the core of his professional life and his wife Gill at the core of his personal life and also, I sense, a shaping of his empathetic attitude towards generations of his future students:

When I was 13, we moved house. A new neighbour said, “Nick should go to the local tennis club—meet some people”. This was a great group who become lifelong friends—one of whom is Gill. While studying for A-levels, I wanted to go to the tennis club every night. There were titanic rows about how often I could go, so my parents grounded me from Monday to Thursday: they said “You say you want to go to university, so you need to study. We can’t make you study but we can keep you in!” I knew they were right.

¹Any quotes not attributed to other sources are from private discussion with Nick Barr in November 2016 and subsequent correspondence.

1.2 Finding Economics

Aged 18, with a vaguely expressed aspiration towards ‘industry’, Nick chose, or as he has suggested, ended up with ‘by accident’, economics: a ‘generally useful...broad-ranging degree’. Although his aims were vague, the young Barr had set his sights on attending a good university which, to him, meant LSE—simply the place that excited him most. Eventually, after a frustrating postal strike, an envelope arrived offering him a place. As befits a billet-doux from a great love, Nick has this letter in his desk drawer: ‘It’s very sentimental ... I still have it...the original LSE acceptance’.

The ‘conversion’ to economics as an explicit, rather than accidental, direction came from inspirational teaching by Richard Lipsey who gave the lectures for the first-year economics course. Nick recalls an ‘outstanding teacher – already a professor in his early 30s which was unheard of in those days!’ Nick became captivated by economics.

Following his undergraduate years, Nick undertook a Master’s at LSE, accepted on condition that he could postpone for a year to travel overland to India (‘very much the fashion at the time’). The year travelling gave him time to realise that he ‘enjoyed economics and enjoyed the LSE’. The decisions around his PhD were, by contrast, more strategic. A doctorate at Berkeley was the way to a job at LSE: ‘It made no sense to go anywhere less good than LSE, so it was Harvard, Chicago or Berkeley. Harvard rejected me, Chicago lost the papers and Berkeley offered a four-year fellowship! The decision was not difficult’.

Nick recalls the great influences at Berkeley that became one and two on his thesis committee: Robert Hall, now at Stanford, ‘still a young Turk’, and Aaron Gordon. Nick recalls that as good friends, they would attend each other’s lectures and snipe from the back: this recollection mirrors Nick’s appreciation of LSE as an environment where it is not only the quality of colleagues and students that make the place, but the genuine academic freedom—‘what matters is the quality of the argument, not the conclusion’. Even when colleagues disagreed strongly with Nick, he recalls ‘lively conversations but no hostility’. When Hall moved to MIT, Nick moved with him for his final PhD year.

In 1971, Nick was appointed to a Lectureship in Economics at LSE. Once in post he single-mindedly worked on ‘methodically maximising’ the likelihood of getting tenure. After this, his main motivation was curiosity and usefulness, rather than a planned body of work for career advancement. Over time, he settled into ‘following [his] nose on interesting policy questions’.

1.3 Academic and Policy Work

Nick's academic work covers the economics of the welfare state (including post-communist transition), higher education finance and pensions, drawing on the economics of information, behavioural economics, incomplete contracts and optimal taxation.

At the very beginning, two particular LSE mentors and friends in the Economics Department were instrumental to Nick's launch. Alan Prest invited him to co-author a book advocating the introduction of self-assessment for income tax (see Barr et al. 1977), and subsequently to be a co-author of his well-regarded text on public finance (see Prest and Barr 1979, 1985). The other key person, Alan Day (then Professor of Economics and later Ralf Dahrendorf's very successful Pro-Director), frequently ended up at the same table as Nick for lunch in the days when most academics had lunch together most days, and over the years they became firm friends. Not always talking shop, they frequently discussed shared interests in electronic gadgets and flight routes and, indeed, Day got Nick involved in two trips to the USA in the early 1980s to recruit students as part of the School's response to the loss of taxpayer support for overseas students. A slightly later, but important, influence was Tony Atkinson, who helped Nick to shape his writing, with generous and valuable comments on draft papers, and as a supportive colleague concerning promotion.

Other than temporary absences, Nick has been based at LSE throughout his career. In 2002, he was appointed Professor of Public Economics, cementing a varied, productive and fascinating career. Nick's summary of this relationship: 'Nothing that was ever a question in my career could have been solved by going elsewhere'.

This is far from a purely academic story however; Nick Barr stands with one foot firmly in academia and the other in the real world, with all the conflict, messiness of policy and the joy of debate which that entails. Alongside his academic work, Nick is an engaged actor in public policy across the deeply connected worlds of economics, social policy and politics.

Work away from LSE encompassed two spells at the World Bank, a shorter time at the IMF, and work with the OECD, ILO and World Economic Forum. In addition to his work in the UK, he has had a significant presence in Central and Eastern Europe, the EU, Chile and China. His written and oral evidence to public bodies and membership of committees and enquiries is extensive, and he has been an influential press commentator. He is perhaps best known publicly in the UK for his work on the finance of higher education.

1.4 Teacher

Just as significantly, Nick is known to many as an author, as a highly talented, unstuffy and empathetic teacher, as a collaborator on many projects, and as a good friend to many he has met in all these roles. In all these things, he sees his role as an ‘academic midwife’. In many ways, his pedagogical side is the place from where his strengths in other roles spring. Indeed, in the Preface to *The Economics of the Welfare State* he acknowledges that: ‘I owe a continuing debt to my students. They ask awkward questions (all the time), see things in clearer ways (often), or provoke me into seeing things in clearer ways (sometimes)’ (Barr 2012a: ix).

Nick is a great learner as well as a teacher; his productive partnership with Nobel Laureate Peter Diamond was at a time of Nick’s rediscovery and reassessment of his own work. He describes Peter’s influence on the fifth edition of *The Economics of the Welfare State* in terms of ‘a great intellectual debt’ and as ‘the ultimate masterclass’ (ibid.). Similarly, he speaks of his experiences at the World Bank as a time when he learned a lot about policy, notably the central importance of implementation.

2 The Economics of the Welfare State

Doing his PhD at Berkeley, Nick was looking for a thesis topic when Adam Ridley, a fellow student,² knowing Nick’s interest in the subject, suggested that he should write about the US welfare system. To supplement a large data set on labour Nick requested access to a survey of welfare recipients conducted by the US Department of Social Security, ‘knowing they would say no. But, they said “yes”’. From this unexpected break emerged one of the first threads of his future work. To give a period flavour, a condition was that Berkeley would cover the cost of ‘five big boxes of punch cards’.

2.1 Academic Writing

The Economics of the Welfare State is Nick’s academic, policy and pedagogical fulcrum. Its intellectual core was the first application to the welfare state of the then new theoretical literature on the economics of information, for

²Later to be advisor to Geoffrey Howe when he was Chancellor of the Exchequer in the UK, but at that time a Harkness Fellow at Berkeley.

which Akerlof (one of Nick's teachers at Berkeley), Spence and Stiglitz were awarded the 2001 Nobel Prize (see Nobel Prize Committee 2001). Nick was one of the first to work out the implications of that literature for the role of the State more generally. The introduction of information failures into the analysis is part of the efficiency case for public production, thus justifying the 'state' within 'the welfare state'.

The Preface to the first edition laid out the core ideas:

There is a large literature on different aspects of the welfare state and a substantial body of economic theory which bears on the issues involved. One of the main purposes of this book is to draw together these diverse sources into a unified whole. Two general conclusions emerge. First, the issues raised by the welfare state fit very naturally into the conventional theoretical framework used by economists. Second, public involvement in institutions of the general sort which comprise the welfare state...can, for the most part, be justified...in efficiency terms, quite independent of debates about social justice. To the extent that this is so, it is no longer public involvement per se which is controversial but only its precise form and the choice of its distributional objectives ... [T]he theory set out in this book assigns a prominent role to technical problems with markets, with particular emphasis on information problems ... These, more than any other theoretical consideration, are crucial to establishing the important efficiency role of the welfare state (Barr 1987: xiii).

Though the crucial content at the heart of the 1987 edition has stood the test of time, the changes, edition by edition, reflect the evolution of Nick's work on the welfare state. The second edition (Barr 1992) amplified discussion of public choice and government failure and of the problems caused by asymmetric information in insurance markets. The third edition (Barr 1998) introduced as a central theme the distinction between risk and uncertainty.³ The fourth edition (Barr 2004a) reshaped the book round the core themes of insurance, lifetime consumption smoothing and poverty relief (this aligned the analysis more closely with Nick's 2001 book, *The Welfare State as Piggy Bank*).

The 2012 edition introduced two important sets of changes mapping out the future. The economics of information, which had always been at the heart of earlier editions, was expanded to include other deviations from first-best, including the findings of behavioural economics, missing markets

³The distinction between risk and uncertainty was first made by Frank Knight (1921).

and incomplete contracts. Significantly, given Nick's profile, the centre of gravity of policy discussion shifted towards an international perspective rather than the UK.

Nick's other key book, *The Welfare State as Piggy Bank*, sets its stall out clearly upfront:

Of the many purposes of the welfare state, two stand out: as a series of institutions that provide poverty relief, redistribute income and wealth, and reduce social exclusion (the "Robin Hood" function); [and] as a series of institutions that provide insurance and offer a mechanism for redistribution over the life cycle (the "piggy-bank" function) (Barr 2001: 1).

This book goes beyond *The Economics of the Welfare State* in its exploration of the role of the welfare state as an efficiency device: notably, quite separate from its role in relieving poverty, the welfare state enables people to redistribute over their life cycle (the 'piggy bank' function). This is set in the context of the pervasive information failures which preclude efficient private institutions. From this arises the generalised point that the welfare state has a *technical* function which sits independently of any *values* about redistribution.

Between them, these two books made significant contributions to analysis:

A unified economic theory of the welfare state: Nick's work fully integrated the welfare state into the core social welfare maximisation framework. This established a coherence for the microeconomic foundations of the welfare state by incorporating previously separate literatures on the economics of education, health and pensions. The first edition of *The Economics of the Welfare State* received uniformly favourable reviews in this vein ('Economists seeking a unified treatment of the economics of the welfare state can stop searching' (Plotnick 1988: 1750)).

The welfare state as an efficiency device: Previously, it was accepted that traditional market failures such as externalities could justify regulation and subsidy but offered no efficiency argument for public production. The introduction of imperfect information in product and insurance markets was the key analytical element in demonstrating the efficiency role of the State in terms of public production. Building on this analysis came an explicit distinction between finance and delivery since under this conception the case for each needs to be considered separately. With health care, for example, information failures in the market for medical insurance create an efficiency

argument for reliance mainly on public finance while its delivery, in contrast, can operate successfully with publicly produced health care—or privately produced—or a mix.

When first articulated, the efficiency role of the welfare state was a completely new result and its implications are important for several reasons. In economic terms, it shows that a welfare state would be necessary even if all poverty had been resolved—its existence is important and in (almost) everyone's interest: not just 'the poor'. In political economy terms, it explains the durability of the welfare state in the face of often extreme ideological hostility, for example during the 1980s in Britain and the USA. It also sheds new light on middle-class capture suggesting that the existence of middle-class recipients of welfare state benefits is not necessarily a perverse, or adverse, outcome.

Nick insists that he is not someone who produces new theory, but instead applies existing theory to something new. This is an understatement. Nick recounts that when he first started working on the economics of the welfare state people would ask, 'Is that really economics?' It is impossible to imagine anyone asking that now. The impact on ideas of his work around the welfare state has been substantial, both within economics and in adjacent fields, and has in fact appeared recently on a list of social policy's 'greatest hits' (see Powell 2017). As a result, the welfare state is no longer a subject apart, but has been satisfyingly integrated into the mainstream.

The Welfare State as Piggy Bank has also had a wide impact on the practitioner community—a deliberate outcome on Nick's part. The start of the book quotes A.C. Pigou, the father of welfare economics:

When a man sets out upon any course of inquiry, the object of his search may be either light or fruit—either knowledge for its own sake or knowledge for the sake of good things to which it leads ... There will, I think, be general agreement that in the sciences of human society...it is the promise of fruit and not of light that chiefly merits our regard (Pigou quoted in Barr 2001: No page number).

While acknowledging the importance of knowledge for its own sake, Nick regards his main purpose as producing fruit.

While I [Astill] was working at the sharp end of the Blair administration's welfare state reform as a government economist during the late 1990s, the only two books consistently on our desks that absolutely had to be consulted were the Child Poverty Action Group's *Welfare Benefits Handbook, 2000/2001* (CPAG 2000) (the only manageable and up-to-date description of the UK's labyrinthine social security system) and Barr's *Economics of the Welfare State* (Barr 1998), which offered the basic principles against which

any reform had to be tested for robustness and ‘sense’, in a digestible format, that was easy to explain to ministers.

In bringing insights from economic theory to bear on social policy, Nick has always been clear that the approach is not mechanistic but requires judgement. He makes the point right at the start of the fifth edition of *The Economics of the Welfare State* (Barr 2012a), which also quotes Peter Diamond on the use of models:

As its title makes clear, this book uses economic theory to analyse the principles of the welfare state and as the main basis for assessing welfare-state institutions in different countries. However, it is important to be clear that economic theory, though offering powerful insights, should not be applied mechanically. Peter Diamond makes the point cogently in his Nobel Prize Lecture: “The complexity of the economy calls for the use of multiple models that address different aspects ... I am concerned that...too many economists take the findings of individual studies literally as a basis for policy thinking, rather than drawing inferences from an individual study, and combining them with inferences from other studies that consider other aspects of a policy question, as well as with intuitions about aspects of policy that have not been formally modeled. Assumptions that are satisfactory for basic research, for clarifying an issue by isolating it from other effects, should not play a central role in policy recommendations if those assumptions do not apply to the world. To me, taking a model literally is not taking a model seriously. *It is worth remembering that models are incomplete—indeed, that is what it means to be a model*” (Diamond 2011: 1045–1046; italics added [by Barr]).

A large part of the book’s wide reach and influence—for students, academics and practitioners—is the voice in which it is written. A review of the first edition that particularly pleases Nick said, ‘A measure of the book’s success in equipping the reader to understand and use analytical tools is that those readers not sharing Barr’s philosophical position will be provoked into using those tools in order to formulate counter-arguments!’ (Heald 1988: 871).

2.2 Impact on Policy and Teaching

In parallel with the maturing academic writing on the welfare state, the real world was fascinating and engaging, and Nick’s flagship book acted as a de facto calling card to the world of practice, notably the World Bank in the late 1980s, when a considerable part of his attention was drawn to the countries in Central and Eastern Europe making the transition from planned to market economies.

Those activities started with an invitation from the Bank to spend two weeks in Poland looking at their proposed new law on unemployment benefit. Nick went to Poland in November 1989 and again in January 1990, turning out to be a ‘round peg in a round hole’. When the Bank asked for a full year of his time, which eventually became two, LSE granted him leave. He returned to LSE in 1993, with a second stint at the World Bank in 1995–1996 as a principal author of their flagship *World Development Report 1996: From Plan to Market*.

Nick’s work in the transition countries led to two further developments. First, at LSE, he established the MSc in the Political Economy of Transition in Europe (PETE).⁴ Since effective reform requires both the economics and politics to work, ‘it was an article of faith that the MSc would be in political economy, not economics’. However, a course in political economy taught in the Economics Department was likely to meet an unhappy response from the Government Department, and vice versa, and for that reason, the course was located in LSE’s European Institute, and thus, it was that Nick came to divide his time between the Economics Department and the European Institute. PETE was an environment in which Nick’s teaching approach could flourish in an area where he had expertise and could explore the material with a vibrant diversity of students from different countries, perspectives, disciplines and backgrounds.

A second consequence was a book that Nick masterminded and edited under the aegis of the World Bank: *Labor Markets and Social Policy in Central and Eastern Europe: The Transition and Beyond* (Barr 1994). The book built on the analytical framework in *The Economics of the Welfare State* at a time when little had been written about those countries. As its Preface states:

The book is aimed at policymakers. The intention is not to write an instruction manual—the problems are far too complex for *anyone* to attempt such a task—but to offer an analytical tool kit which policymakers can apply to their own country. There is a strong emphasis on practicality because of the growing realisation during the writing that the success of the reforms depends at least as much on administrative and political skills as on policy design (ibid.: xvi; italics in original).

In a Foreword to the book, Václav Havel, the first post-communist President of the Czech Republic, noted that the topics of the book ‘are important to the material aspects of the reforms. More important, they are central to

⁴It was when I was studying this Master’s course that I first encountered Nick.

giving the citizens of our reforming countries genuine freedom and some control over their own lives’ (Havel in *ibid.*: xiii). The book was well received, including praise from a long-time critic of the World Bank for being ‘devoted to the human dimension’ (Patterson 1995: 22).

2.3 Collaboration and Friendship

Nick is clear that the credit for his work on the welfare state is far from all his own. LSE gave him time to write the first edition of *The Economics of the Welfare State* (way back when technology was such that the first draft was written in pencil on the back of dead computer printout) and also agreed to his spell of leave at the World Bank. Recognition is also given to one of the origins of the book being a course on the economics of the welfare state for third-year economics undergraduates that Nick and Julian Le Grand designed; Nick giving the lectures on cash benefits and Julian on benefits in kind. Howard Glennerster represented for Nick collegiality at its best, offering detailed comments on large chunks of the first edition of the book, the start of collaboration in which Nick and Howard swapped draft typescripts of their separate but complementary books on the welfare state. That collaboration ripened into friendship during overlapping periods in Washington, D.C., particularly aided by their shared enjoyment of cricket. Nick tells the story of the two of them sitting on the balcony of the pavilion at Lord’s, when Nick’s mobile pinged with a text message from his wife. Reminding them of what is also important, it simply said, ‘Stop talking shop’.

If Nick’s work on the welfare state was planned, his involvement in the economics of higher education finance had large elements of serendipity, and as we shall see, the same is true of much of his work on pensions. In order to explain this tendency, he likes to quote Isaiah Berlin, who reputedly often said, ‘I am like a taxi: I have to be hailed’.

3 Higher Education Finance

3.1 Academic Writing

Writing on higher education had its genesis in the mid-1980s when Alan Day (mentioned earlier as one of Nick’s mentors) pointed out that a book on the welfare state should include discussion of student loans. Approaching

this new (for him) topic, Nick recalls ‘one of those occasions where the logic turned me round 180 degrees’. Initially regarding student loans as a ‘nasty right-wing plot’, his reading included an article (Farmer and Barrell 1982) about income-contingent loans—these are loans with repayments in the form of $x\%$ of the borrower’s subsequent income until the loan is repaid—this reading made him realise that such loans could be an important instrument of progressive social policy.

The analysis of higher education in the first edition of *The Economics of the Welfare State* was the basis of his subsequent work. Lord Nicholas Stern, then Chair of STICERD,⁵ recognises Nick’s work at this time as ‘pioneering...laying the foundations for everything that followed’.⁶

The notion of social insurance, which can cover risks that private insurance handles badly or not at all, is a critical element in locating student loans within the wider economics of the welfare state. It is not immediately obvious that pensions and higher education finance both involve transferring consumption across a person’s life cycle—from one’s younger to one’s older self in the case of pensions, or vice versa. Thus, the first loan proposal (see Barnes and Barr 1988; Barr 1989) was for repayments as an add-on to national insurance contributions.

At a later stage, Nick tracked down the original expositions of income-contingent repayments by Friedman and Kuznets (1945) and Friedman (1955) who found that the return to human capital was higher than to physical capital with a diagnosis that because loans to finance investment in human capital had no collateral, they were risky. Furthermore, he saw that the resulting risk premium led to underinvestment in human capital. Friedman noted that the private response to risky lending is equity finance; hence, he proposed a loan (investment) from government in return for repayments of $x\%$ of the graduate’s earnings, i.e. a ‘dividend’.

Originally, Nick says, he was not aware of the Friedman angle, but when he came to it, he saw that he could deepen the theoretical argument by anchoring the analysis in the economics of information (which post-dates Friedman’s writing) and by demonstrating a convergence between the benefit principle, the ability-to-pay principle and the social insurance principle. A key result was that income-contingent loans are supported by both efficiency and equity arguments.⁷

⁵LSE’s Suntory and Toyota International Centres for Economics and Related Disciplines.

⁶Private correspondence with the author.

⁷For a detailed discussion of Friedman’s work on higher education finance, see Barr (2016a).

Nick's work in this area puts great emphasis on access. The answer is not 'free' higher education. In England, at a time when people from low-income backgrounds paid no fees, 81% of young people whose parents were professionals went to university; the comparable figure for young people from manual backgrounds was 15%. An important part of the explanation is that the problem starts long before the age of 18: controlling for school attainment, the socio-economic gradient in participation largely disappears:

The flawed argument is that people from poor backgrounds do not go to university because they are debt averse; thus resources to widen participation should finance grants for university students. What the evidence suggests is that poor people do not go to university largely because of low attainment and, if that problem is fixed, are almost as likely to go to university as people from better-off backgrounds. Thus, resources to widen participation should be used mainly to raise school grades, prevent dropout and improve information. The mistake is to attribute to the credit constraint behaviour that is determined mainly by the prior attainment constraint (Barr 2017: 370).

Nick's work on higher education comes across not as some dry exercise but as a small work of art. Like the best novels, it is revelatory, tightly plotted and at times emotionally moving. The gradual building of knowledge and the revelation of unexpected and surprising truth effectively forces one to question one's own assumptions and expectations.

3.2 Collaboration and Friendship

The work on higher education finance was again very much a collaborative effort. However, this time it was not with a fellow academic, but with LSE's multi-talented former Head of Public Relations, Iain Crawford. As Nick wrote in Iain's obituary: 'I was leading a blameless existence as an academic when Iain led me astray' (Barr 2004b). Nick's work with Crawford was complementary and hugely productive. As colleagues and friends, they were soon recognised as an in-demand team who 'entrusted each other with their professional reputations'.

Iain worked wonderfully alongside Nick's character and modus operandi. He would polish Nick's far-from-naive understanding of political realities and would get his work exposure by taking him into the heart of the beast. Iain had a talent to get himself and Nick to 'be where we shouldn't be', such as the now closed Annie's Bar in the Palace of Westminster, where the press and politicians would mingle. In Parliament and at high-profile political functions, Iain

would steer Nick to the people he should talk to, allowing Nick to do what he did best—educating journalists to understand the thinking behind the student loan proposal, which they found attractive as a ‘countercultural story’.

Nick recalls Iain’s style:

As press officer Iain would prop up the coffee-bar end of the Senior Common Room at lunchtime. He knew what everyone was writing and he had seen my discussion paper [on student finance] ... I was at the STICERD 10th anniversary conference when Iain beckoned me out of the room, handed me a bit of paper with a phone number and said “You are on the *Today* programme⁸ tomorrow”, “What?” “Talking about student loans—I told them you are an expert”. So I did the programme with Brian Redhead...and one thing led to another.

Nick also recalls their very different styles: ‘Nick the academic: “We’ve got to win, we’ve got the argument so right”. Iain the politician: “What the hell does that have to do with it?”’ The professional and personal teamwork blossomed into a writing partnership and friendship. In Iain, Nick saw political skills of which he had been previously unaware. As we have seen, technically, Nick saw student loans as consumption smoothing from an individual in middle age to his or her younger self. Iain’s political insight was to realise that though true in analytical terms these two ‘characters’—students and graduates—were not in a *political* sense the same person, hence ‘the politically powerful dictum that “students get it free – it’s graduates who repay”’.

3.3 Impact on Policy

The first UK student loan was introduced in 1990. Having failed to persuade the government that it should have income-contingent repayments, Nick and Iain set about creating more fertile ground for the next reform, with clear success, since their work was a key element in the unanimous recommendation by the 1997 Dearing Committee that student loans should have income-contingent repayments (see Barr and Crawford 1998).

The 2006 reforms in England saw the zenith of the partnership. Throughout his career, Nick tended to avoid official positions, on the

⁸The BBC’s flagship radio morning news programme.

grounds that an independent voice was likely to have more influence, but this time was exceptional in that he was a ‘semi-insider’. Again, there was an element of serendipity. In late 2002, Nick ‘got invited to a social event’ with Charles Clarke, the newly appointed Secretary of State for Education. It turned out not to be ‘social’ but ‘business’, with several other movers and shakers in higher education. Nick had two minutes of Clarke’s undivided attention and recalls: ‘I knew he had got it. I’m a teacher and I know when people have got it’. Unofficial and uncredited advice followed and, as the outcomes show, Clarke took notice, and so did others (an article in *The Guardian* (MacLeod 2003) referred to Nick and Iain as the ‘architects of the reforms’).

The introduction of variable fees was highly controversial: with the Blair government in its pomp with a parliamentary majority of 160, the Second Reading of the Bill passed with a majority of just five. During multiple sessions giving evidence to the Education Select Committee and in long discussions with MPs over coffee, it transpires that Nick and Iain had ‘turned’ at least three MPs. Had they voted the other way, the Bill would have failed.⁹

The 2006 reforms are seen by Nick as much more strategically coherent, leading to the tuition fee income of universities rising by 87%, the number of grants and loans by 25%, and the number of students by 20% during the period 2006–2012. Strikingly—and an outcome which gives Nick particular pleasure—the number of applicants from the most disadvantaged background rose by 53%. In contrast, Nick is highly critical of the 2012 reforms. ‘Governments, when presented with strategies, have a horrible tendency towards cherry picking a good set of reforms and making a mess of them’ (Barr 2014).¹⁰

The body of work on higher education finance was also influential internationally. Early writing, including articles in *The Times* (Barr 1988a) and *Financial Times* (Barr 1988b), was influential in Australia’s introduction of income-contingent repayments in 1989, and Nick advised a later inquiry (the West Committee, 1998) on higher education finance more broadly. Nick and Iain were also heavily involved around 2000 in helping to establish a loan scheme in Hungary (see Berlinger 2009).

⁹One of Nick’s few regrets is that a global virus that day slowed the Internet, so that an invitation to the Blair team’s private celebration after the vote did not reach his inbox until the next morning. Alternative history does not record how the world may have changed.

¹⁰Nick’s critique of the 2012 reforms (see Barr 2012b) is titled, ‘The Higher Education White Paper: The Good, the Bad, the Unspeakable – and the Next White Paper’.

4 Pensions

4.1 Early Academic Writing

A third major area of Nick's work is old-age pensions. The starting point was a 1979 paper, 'Myths My Grandpa Taught Me' (Barr 1979). Instead of the conventional analysis of pensions in terms of finance, the paper framed the issue in terms of national output and how it is shared between workers and pensioners. It had always been understood that pensions financed on a pay-as-you-go (PAYG) basis (i.e. where this year's workers' contributions pay for this year's pensions) face problems when there is an ageing population, due to fewer workers and more pensioners. The framing in terms of output helped to clarify that funded pensions (i.e. where pensions are paid from an accumulated fund built up over a period of years) face very similar problems. In both cases, the underlying problem is pressure on national output:

[Conventional arguments] suggest that there are risks attached to PAYG finance; they also imply, without ever saying so, that funded schemes are somehow "safe" ... This paper argues that any such assertion is false, and that funded schemes are just as sensitive to population changes...as is the state scheme or any other PAYG scheme ... The reason why PAYG and funded schemes respond so similarly to a decline in the labour force is that both face the same realities. In both cases pensioners can consume only what workers do not consume; and in both cases the work effort to produce pensioners' consumption has to be supplied by the next generation (Barr 1979: 28, 54).

Alongside discussion of information failures, this line of argument became part of the analysis of pensions in *The Economics of the Welfare State*.

4.2 Opportunities

Nick's subsequent work on pensions falls into the now familiar category of serendipitous opportunities of which the first was his involvement with the World Bank. In Warsaw in 1991, Nick was presented with a proposal for radical pension privatisation at a time when the monthly inflation rate was 80% and there was no financial market regulation because as yet there were no such markets. His report to the Bank noted the proposal as a potential medium-term development, pointing to unemployment benefit and poverty relief as the urgent agenda. This prioritisation caused 'a ferocious row' with

powerful actors at the Bank, who regarded pension privatisation as desirable and urgent, following the example of Chile in 1981. That was only the first skirmish in Nick's ongoing battle over pensions.

Returning to LSE, Nick continued the debate in *Labor Markets and Social Policy in Central and Eastern Europe* (Barr 1994) and subsequent editions of *The Economics of the Welfare State*. A spell as Visiting Scholar at the IMF in 2000 created an opportunity to pull together his key themes of national output, risk and uncertainty and imperfect consumer information (see Barr 2002).

When Nick's colleague, Nick Stern became the World Bank's Chief Economist, Nick Barr recounts that 'the discussion was "if you were Romanian would you want your pension invested in the Romanian stock market!"'. Though Stern had no line authority, he was able to ask for a position paper from the people involved in the Bank's operational pensions work (subsequently published as Holzmann and Hinz 2005) and suggested an associated advisory panel which included Barr and Diamond. The budget included a meeting in Washington for the panel to discuss a draft of the paper, where 'no-one had compared notes but everyone [on the panel] criticised [the paper] for the same reasons'. Chatting afterwards over a drink, Nick and Peter came up with an idea of the Bank conducting a review of its own pension work across the piece. This led to Emily Andrew's satisfactorily hard-hitting report (World Bank 2006), published under the aegis of the World Bank Independent Evaluation Group that gave 'a crunchy critique of the Bank's pensions work'.

A further opportunity arose when Jim Mirrlees led a project of technical assistance to China with a committee comprising mainly former World Bank chief economists. When the topic of pensions came up, Stern (a member of the committee) proposed re-forming the advisory committee he had created at the World Bank. Nick (Barr) was sitting in his office in May 2004, when a phone call out of the blue from someone he did not know asked whether he could find ten days to work on pensions in China. Nick's instant thought was 'impossible'. Then, the caller mentioned that the small team included Mirrlees and Diamond, and the response instantly became, 'of course I can find ten days'. During a global conference call for the advisory group, Peter and Nick did most of the talking, after which they did most of the writing, producing a 100-page paper, their first co-authored work.

The two of them were part of a group that presented a shorter version of the report to Chinese Premier Wen Jiabao in November 2004. Standing on the platform at the Beijing metro the next day, either Peter or Nick (each blames the other) proposed converting the longer version of the report into

‘a quick short book’. That book, Barr and Diamond (2008), took four years to complete and, in the event, was so long that a shorter version (Barr and Diamond 2010a) was published as well.

4.3 Collaboration and Friendship

Nick and Peter’s approach in the two books is summarised in their 2009 paper:

This article, sets out a series of principles for pension design rooted in economic theory: pension systems have multiple objectives, analysis should consider the pension system as a whole, analysis should be framed in a second-best context, different systems share risks differently, and systems have different effects by generation and by gender ... That discussion is reinforced by identification of a series of widespread analytical errors—errors that appear in World Bank work, but by no means only in World Bank work: tunnel vision, improper use of first-best analysis, improper use of steady-state analysis, incomplete analysis of implicit pension debt, incomplete analysis of the impact of funding... and ignoring distributional effects ... The second part of the article considers implications for policy: there is no single best pension design, earlier retirement does little or nothing to reduce unemployment, unsustainable pension promises need to be addressed directly, a move from PAYG towards funding... may or may not be welfare improving, and implementation matters—policy design that exceeds a country’s capacity to implement it is bad policy design. We illustrate the ranges of designs of pension systems that fit the fiscal and institutional capacity constraints typical at different levels of economic development (Barr and Diamond 2009: 5).

The unfashionable warnings against pensions panacea are never far away. One of their central conclusions is that though there are sound principles of pension design, there is no single best pension system for all countries. Put succinctly: ‘If objectives and constraints differ, the optimum will generally differ, as well’ (Barr and Diamond 2010b: 5). Crucially, when seeking this optimum under the multitude of varying parameters (many unknown), Barr and Diamond emphasise the second-best context in which pensions are located:

In the theory of saving in a first-best world the individual is assumed to make choices about saving, borrowing, portfolio choice and annuitisation that maximize his or her lifetime utility from a complete array of competitively-priced market options. In those circumstances, consumer choice and competitive

markets maximize welfare, given lump sum redistribution. Pensions, however, face a number of serious deviations from such a theoretical world (Barr and Diamond 2009: 9).

Risk sharing is a major theme: ‘A central question for policymakers is how risks should be shared, a question with both efficiency and equity implications. As with redistribution, different answers are possible, but it is a major error to ignore the question’ (Barr and Diamond 2010b: 5).

A second major theme is the usefulness or otherwise of consumer choice. Evidence to an Australian inquiry (Barr and Diamond 2017: 1), drawing on findings from behavioural economics, starts with three propositions:

- The primary purpose of pensions is old-age economic security.
- People who wish to make choices about pensions and retirement should generally have room to do so. But some people will not make choices, choice can be costly, and some people may make bad choices. Thus:
- The pension system should work well also for people who make no choice—and making no choice should be an acceptable option.

Nick’s collaboration with Peter Diamond is both professionally fruitful and personally important. Peter has an exceptional analytical grasp,¹¹ with an ability to see economic problems in their multidimensional whole—this implies an approach that is inherently complex. On the other hand, anyone who has attended Nick’s lectures, or heard him speak on almost any subject, will recognise his way of taking things step by (very clear) step. As he says: ‘I have to take the bits apart and reassemble them; when people say I am clear it’s because I have to do that to understand’. Thus, Nick’s role is to set out Peter’s multidimensional analysis as a linear story suitable for a wide readership that opens up all the depth and extracts the value. He sees a crucial part of his task, symbolically and literally, to add ‘cryptic’ as a comment to text drafted by Peter, translated as, ‘Please expand so that the rest of us can understand’.

The caricature division of roles of Peter (Nobel Laureate) and Nick (accessibility) has been greatly successful and is clearly recognised by Peter:

[Our] books have been well received (the second is also available in Chinese, Polish, and Spanish) and seem to me considerably better than either of us could have done alone. Doing better, rather than merely dividing the work, is

¹¹The *American Economic Review* of February 2011 (Arrow et al. 2011) listed the 20 most important papers it had published over the previous 100 years; three were by Peter (including two jointly written with Mirrlees).

the sign of a really valuable collaboration. Along the way, we had a good time and have become fast friends. Indeed, we are at work on another book, going to issues not addressed in the earlier books.¹²

That new book is forthcoming at the time this chapter is being written in 2018 (Barr and Diamond, forthcoming) Nick's take on the durability of this partnership centres around the *point* of the work, which unites the pragmatism and the passion of both authors. As he puts it: 'The analytics aren't important for their own sake, but to improve people's lives: being sloppy and emotional shows you are kind but is not always very helpful'.

We have already seen how Nick translates this approach into his writing and policy work, and he sees Peter as having a similar motive, since much of Peter's groundbreaking theoretical work was motivated by policy issues that he regarded as important but for which he regarded existing theory as unsatisfactory.

4.4 Closing the Circle: Pension Reform in Chile

The root of Nick's disagreement with the World Bank's pensions analysis was that, in his view, the Bank overstated the potential benefits of introducing individual funded accounts along the lines of the reforms in Chile in 1981. Going some way to closing the circle have been Nick's activities advising the Chilean government. His writing as Visiting Scholar at the IMF (published as Barr 2002) was picked up in Chile, leading to invitations to take part in discussions in 2004 and again in 2006 as part of the Marcel Commission which recommended that individual accounts should be complemented by a tax-financed non-contributory pension. In 2014–2015, as a member of the Bravo Commission, Nick visited Chile six times, one of the trips with Diamond. Their paper, Barr and Diamond (2016), summarises the Commission's recommendations and some of its internal disagreements.

5 LSE Citizen

Nick believes that the part of university life that he refers to (more flatteringly than many academics) as 'citizenship' is important. With the now familiar blend of curiosity and pragmatism, he purposely selected elements

¹²Private correspondence with the author.

of citizenship that he saw as fun and interesting, so as to be impressively (and genuinely) busy when anyone came along with something worthy, but dull. That said, Nick describes the whole of his career as fun, apart from marking examination scripts, upon which he carried out a rapid but convincing cost–benefit analysis against lifetime earnings to conclude that, overall, he has a good deal.

In his role as good citizen, Nick has appeared on many committees at LSE, including Council, the Court of Governors, and the Finance and General Purposes Committee. Less visibly, he has sat on bodies ranging from the prosaic ‘Group on Copying Services’, through the ‘Working Party on Overseas Tuition Fees’ to the enigmatic ‘Futures Group’. He also contributed in his role as one of the co-ordinators for the Research Excellence Framework.

In unconnected side projects, as mentioned, he made two student recruiting trips to the USA. He also contributed to the heritage of the School in a project with its genesis in his undergraduate days when one of his teachers was A.W.H. (Bill) Phillips (see LSE 2014). Before Phillips developed his eponymous curve, he created the water-driven Phillips Machine, a physical hydraulic analogue of economic flows in the real world. In a project initially conceived by Tony Atkinson and later supported by Nick Stern as successive chairs of STICERD, Nick organised the restoration of the Phillips Machine and documented its history (see Barr 2000). The Machine is now on permanent display in the Mathematics Gallery in the Science Museum in London. It also gives rise to an image chosen by Stern, who offers a vision of Nick in shirtsleeves, somewhat damp, with a hammer working to join the two existing Phillips Machines in a unique experiment.¹³ Thereby we have a striking visual metaphor for the very practical nature of much of Nick’s academic work.

6 Conclusion

Though this chapter has been substantially about one of the pillars of Nick’s life—his work—its twin is his family life, particularly his happy marriage to Gill and, latterly, local grandchildren. In his younger years, he enjoyed the mountains, including trekking to Everest Base Camp with his brother where, never able to escape, he was spotted by an LSE student. His current entry in *Who’s Who* lists his recreations as cricket, computers, grandchildren and photography—it neglects to mention conviviality and good conversation.

¹³Private correspondence with the author.

Nick's students know most clearly what his readers, collaborators and policy 'customers' always discover—his main purpose is to empower whoever wishes to be a learner, and he looks to do that through his own work and enabling it in different ways. One of Nick's more famous students, the campaigning broadcaster and popular 'money saving expert', Martin Lewis, said that his chairing of the Independent Taskforce on Student Finance Information 'traces back to Nick and the LSE' and learning that 'theory must underpin the study' while retaining the crucial aspect of a 'practical education' (LSE 2016a).

To benefit from Barr, there are, however, no shortcuts or easy rides. You can know his method, but not anticipate the conclusions he may reach—in fact, you would be wiser to adopt the methods and reach the conclusions with him. In the Preface to the fourth edition of *The Economics of the Welfare State*, Nick says:

I have expanded the treatment of higher education finance by giving it a separate chapter and anchoring the discussion more explicitly in the economics of information ... [I]t strengthens the book's message to have two chapters on education, one on schools and one on post-compulsory education, the first sceptical about market forces, the second supportive—both sets of arguments entirely consistent with the core theoretical analysis of the book (Barr 2004a: viii).

This 'strengthening of the message' is central to Nick's academic writing and the credibility of his policy and public impact. Pervading his work is the complete acceptance, indeed celebration, of the fact that consistently strong analysis under different contexts may produce entirely opposite conclusions. Nonetheless, while never shaping analysis to a given end, he has objectives that he will fight for. He says that 'widening participation in higher education really matters', 'old age security matters', and he is personally involved in progressing these things. The motivation is to push forward analysis where existing work feels unsatisfactory to meet the challenge.

Although Nick is not 'an operator' or a career 'public intellectual', he admits to being 'a noisy public busybody'. He has been widely seen in the press around issues of higher education finance and, more formally, has provided evidence to parliamentary and other government committees at home and abroad, including the UK Department of Work and Pensions Working Party on Extending Working Lives and The Hutton Review (on pensions), the Dilnot Review (on long-term care) and, most recently, as specialist adviser to the Economic Affairs Committee of the House of Lord for its 2018 report on the economics of higher, further and technical education. For the most

part, he did not pursue such positions because, as mentioned, he felt more ‘useful and productive with no affiliations except as the official representative of Nick Barr’. In one of his most widely appreciated interventions, he proved the reach of sound analysis and clear evidence when LSE was proud to tweet, ‘So, Prof Nick Barr’s @lsebrexitvote blog post crashed the LSE servers this weekend! Over 190,000 views so far’.¹⁴ The blog (Barr 2016b) reached nearly half a million views by the time of the UK’s referendum on EU membership.

Nick is consistently modest about his contributions: ‘Not inventing the internal combustion engine but making sure it sits with a steering wheel and brakes so that it is safe and valuable to use’, and sees himself happily as an integrated member of the academic community. These are the parallel Barrs—academic writer, creator of impact in policy and in the world it affects, collaborator and, ultimately, teacher, mentor and friend to many. They have flourished within the environment of LSE that has been ‘home’ for over 40 years.

Finally, we should take the time to admire a man who says in a Preface (Barr 2012a: vii) that ‘Many people (including me) believe that it is an essential part of a civilized society that there should be a generous social safety net. That, however, is only part of the case for a welfare state’, and then proceeds to complement that deeply held belief (expressed in a dozen words) with a detailed and tightly argued case (across 348 pages), yet never allows his belief to colour or distort the analysis—only to motivate it.

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¹⁴@LSEnews on twitter.com (LSE 2016b).

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34

Stephen J. Nickell (1944–)

Jan C. van Ours

1 Introduction

It is an honour for me to write this chapter on Steve Nickell. I have known Steve for many years through conferences and workshops and his period as President of the European Association of Labour Economists (from 1999 to 2002). In addition to this, I know him as a co-author, which happened almost as a coincidence. In 1999, the editors of *Economic Policy* invited me to write a paper on the Netherlands and the UK about the reasons for the recent decline in unemployment in the two countries. I contacted Steve for advice on who could be my UK counterpart for the job. ‘Me’, he said, ‘I might as well do it myself to avoid wrong interpretations of recent events’. Our *Economic Policy* paper was published in 2000 (Nickell and van Ours 2000a), while a companion paper was published as the result of a dual presentation in Ottawa (Nickell and van Ours 2000b). Our main conclusions were that the reduction in unemployment in the two countries was due to the reduction of the equilibrium unemployment rate since the early 1980s. We concluded that combinations of supply-oriented policies were responsible for this, the main overlap being the popularity of part-time work and the reinforcement of financial incentives for work for unemployed workers collecting benefits.

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Over the years, Steve covered a wide range of research issues while also making a couple of interesting switches. At the start of his academic career, he did theoretical work, quickly moving on to empirical work. Coming from a mathematical background, it took Steve ‘two years to learn economics’. Initially, he worked on individual unemployment durations but he moved on to cross-country/time series analysis of unemployment. In his early years, he studied product market-related topics while later on he focused on labour market issues. In his early years, he also worked on investment before moving on to unemployment.

His top five publications in terms of number of citations (in EconPapers) show the wide variety of topics covered by Steve. He has almost 1800 citations of his 1981 *Econometrica* paper on biases in dynamic models with fixed effects, about 1300 citations of his 1991 book on unemployment with Richard Layard and Richard Jackman (reprinted in 2005), around 750 citations of his 1997 overview paper in *Journal of Economic Perspectives* on unemployment and labour market rigidities, roughly 650 citations of his 1996 paper in *Journal of Political Economy* on competition and corporate performance and nearly 400 citations of his 1999 chapter in the *Handbook of Labor Economics* with Richard Layard on labour market institutions and economic performance.

Section 2 of this chapter is a stylised overview of Steve’s interactions with colleagues and students, at both LSE and Oxford University. After all, Steve worked at LSE for 21 years but also in Oxford for 21 years (including Warden of Nuffield). A large part of his work on unemployment and labour market institutions, for example, was published during his Oxford years. Like most academics, economists are footloose and cooperation is not necessarily restricted to close colleagues. I group Steve’s wide range of research topics into three sections. Section 3 is very heterogeneous dealing with research on investment, competition, productivity, inflation and dynamic panel data. Section 4 is on his research on job search, employment and wages and collective bargaining while Section 5 highlights Steve’s work on unemployment. Some of his work can easily be allocated to one of the sections. His work on investment theory, for example, is clearly stand-alone. Other work, however, is more difficult to allocate. His papers on product markets, inflation and productivity are now in Section 3 but could also have been part of Section 5 where equilibrium unemployment is determined by wage setting and price setting. Although I cover a wide range of topics not all of Steve’s papers are included. I have tried to focus on his most important and influential papers but this is at least partly subjective. When presenting Steve’s work, the emphasis is on the empirical content, ignoring the fact that

each one of his empirical studies has a substantial theoretical part to it, often to help underpin the set-up of the subsequent empirical analysis.

2 Steve Nickell at LSE (and Oxford)

Stephen John Nickell was educated at Merchant Taylors' School, Northwood, and Pembroke College, Cambridge. He started his career by working for three years (1965–1968) as a mathematics teacher at Hendon County School. After that he did not want to be a school teacher any more so he started as a postgraduate student at LSE. When he was applying for a job in Southampton, during the interview, he learned that there might a job available for him at LSE where Frank Hahn appears to have said 'Why don't we give young Nickell a chance'. Steve's first job at LSE was from 1970 until 1984. During this period, he also spent time in Paris at the École Nationale de la Statistique et de l'Administration Économique (1974/1975) and was a visiting researcher at Princeton University. In the mid-1970s, Steve initially had many single-authored publications on investment focusing on the role of expectations and uncertainty. In the late 1970s, he published studies on unemployment durations using micro data, including his single-authored seminal paper in *Econometrica* (Nickell 1979a). This appeared approximately at the same time as Tony Lancaster's paper on unemployment durations (Lancaster 1979). Also in 1979, Steve and Tony read their joint paper before the Royal Statistical Society. Although overlapping in set-up there are also clear differences. Lancaster is a statistician who knew about the gamma distribution which he used to model the effect of unobserved characteristics. Steve was a mathematician and was not aware of the use of this type of distribution in this context. He found his own solution to deal with the issue. Later on, Steve published on unemployment durations with Wiji Narendranathan (now Arulampalam) and Jon Stern (Narendranathan et al. 1985). In the late 1970s, he also worked on unions and collective bargaining, again publishing some single-authored papers and some with Richard Layard and David Metcalf. In the 1980s, Steve worked on unions and wage determination with Sushil Wadhvani. From the early 1980s onwards, there appeared a string of papers on unemployment in Britain and unemployment in a cross-country setting. Many of these papers—especially review papers—are single authored but there are also many co-authored papers with a variety of colleagues, such as Martyn Andrews, Nicos Floros, Richard Jackman, Richard Layard, David Metcalf, Wiji Narendranathan, Nicolas Dimsdale and Nicolas Horsewood. Apart from the papers on investment,

unemployment and unions, Steve also published on inflation, single authored and with Glenda Quintini. He worked on product markets and firms with David Metcalf, Jari Vainiomaki and Sushil Wadhvani and on productivity with Daphne Nicolitsas, Sushil Wadhvani and Martin Wall.

From 1984 to 1998, Steve was Professorial Fellow of Nuffield College, Oxford, and Professor of Economics in the University of Oxford and Director of the University of Oxford Institute of Economics and Statistics. At the start of his Oxford period, he mainly worked on unemployment issues. A 1986 paper with his LSE colleagues Richard Layard and Charlie Bean on a multi-country study of unemployment has the interesting acknowledgement that it is 'largely the work of Charlie Bean who must therefore take the blame for errors' (Bean et al. 1986: S20). Later work was with Brian Bell, Richard Jackman and Richard Layard in the 1990s. In the 1990s, Steve also returned to work on unions and wage bargaining with Kevin Denny, Paul Kong and Richard Layard and in the current century with Brian Bell, Giulia Faggio and Glenda Quintini.

From 1998 to 2005, Steve had his second employment spell at LSE, where he worked on unemployment issues with Giulia Faggio, Richard Layard, Luca Nunziata, Wolfgang Ochel and Glenda Quintini. From 2000 to 2006, he was member of the Bank of England's Monetary Policy Committee. After that, he returned to Oxford as Warden of Nuffield College. In 2008, Steve and Richard Layard won the IZA Prize in Labour Economics, mostly for their work on unemployment and labour market institutions (see Layard and Nickell 2011). From 2011 to 2016, Steve worked at the UK's Office for Budget Responsibility, and in 2015, he was knighted for services to economics.

3 Investment, Competition, Productivity, Inflation and Dynamic Panel Data Models

3.1 Investment

In a series of papers in the mid-1970s, Steve presented theoretical work on the role of expectations and uncertainty in investment. Nickell (1974a) notes that neoclassical investment theory is essentially myopic with no role for expectations. A perfectly competitive firm decides on expanding or reducing capital such that the marginal product is equal to the real cost of capital. The paper introduces expectations and provides a theoretical analysis of how these might affect the optimal investment programme of a firm.

Nickell (1974b) follows up on this and considers the relationship between government policies and expectations of a firm when making investment decisions. The main conclusion from this theoretical exercise is that it is a ‘very tricky business’ (ibid.: 255) to try to influence investment such that it will always be difficult to assess the consequences of government policy in advance. Steve concludes that it is probably easier for the government to depress the level of investment than to stimulate an increase. Nickell (1975) considers the treatment of depreciation of capital and takes a closer look at replacement investment and maintenance costs. The model used is similar to the one in Nickell (1974a). As machines age, they become less productive, requiring more maintenance. Including maintenance costs results in an investment model of the vintage type, in turn enabling an analysis of scrapage rates and replacement investment. Nickell (1977a) again deals with the influence of uncertainty on firms’ investment decisions, now focusing on the role of the government in creating uncertainty. This uncertainty arises from attempts by the government to ‘manipulate the environment in which private firms operate’ (ibid.: 47), for example through fiscal policy changes or the threat of possible nationalisation. To the extent that government policy leads to more uncertainty, the level of investment will be reduced. An important issue is the role of expectations in the sense that firms might be influenced more by what they think the government can do rather than by what it actually does. Since the government cannot control what firms think one possible solution to reduce uncertainty is for the government to avoid, for example, macroeconomic stabilisation policy because of its unpredictable side effects. Nickell (1977b) is the last of the theoretical papers on investment decisions. This paper is once more on uncertainty and lags in the investment decisions of firms. Steve uses a dynamic demand model with a focus on the timing of future events. The idea is that firms might have a fairly good idea about what is going to happen but not about when events will take place. In this case, with an increase in demand, investments will only be made if this increase is expected to have some degree of permanence. In the early 1990s, Steve addressed the relationship between unions and investment in two empirical papers (see below).

3.2 Product Markets and Competition

Steve’s first paper on product markets and competition was Nickell and Metcalf (1978) which addresses the relationship between monopolistic industries and monopoly profits or, as the title of their paper asks,

'Are Kellogg's Cornflakes Overpriced?' In support of the theory of own brand prices in retailing, Nickell and Metcalf discuss the example of Sainsbury's cornflakes which come in an identical size as Kellogg's cornflakes and probably have the same quality. Yet Kellogg's cornflakes are more expensive than Sainsbury's cornflakes. They hypothesise that Sainsbury's cornflakes have a markup relative to Kellogg's that is positively related to the monopoly power as reflected in the price of Sainsbury's cornflakes and negatively related to the extent of advertising by Kellogg's. Nickell and Metcalf find empirical support for this phenomenon. About 15 years later, Steve returned to the issue of product market competition. Nickell et al. (1994) study the relationship between wages and product market power, using information from UK manufacturing firms. The main findings are that product market power has a positive impact on wages, while it reduces the negative effect of unemployment on wages. Nickell (1996) discusses the relationship between competition and productivity. This paper in the *Journal of Political Economy* is one of his most cited. It starts with the notion that most people believe that competition is a good thing. The general idea is that product market power reduces productivity, so competition raises the productivity of firms. The paper argues that neither theory nor empirical evidence provides overwhelming support for this general idea. Steve sets out to provide some empirical support based on an analysis of UK manufacturing firms, finding that market power induces reduced levels of productivity. Furthermore, competition is associated with higher rates of total factor productivity growth. Although providing evidence in support of the idea that competition is a good thing, Steve notes that 'it is worth entertaining the thought that we are barking up the wrong tree. Perhaps competition works not by forcing efficiency on individual firms but by letting many flowers bloom and ensuring that only the best survive' (ibid.: 741). In a companion empirical paper, Nickell et al. (1997) investigate three possible determinants of improved productivity performance in firms: product market competition, financial market pressure and shareholder control. The main conclusion based on panel data analysis of UK manufacturing firms is that all three possible determinants are associated 'with some degree of increased productivity growth' (ibid.: 793). Nickell (1999) studies whether monopoly power in the product market has a negative impact on the performance of the labour market. The focus of the paper is on the impact of monopoly rents on wage determination. Steve does not present new empirical evidence but discusses available data on the elasticity of firms' wages with respect to firms' revenue per employee. He concludes

that monopoly rents are likely to be shared through collective bargaining such that a rise in product market power leads to higher wages. However, this only holds for a rise in market power at the individual firm level. An overall rise of market power throughout the economy is likely to result in lower wages and higher unemployment because aggregate labour demand is reduced.

3.3 Productivity

Nickell et al. (1992) use a panel of UK manufacturing firms to study productivity growth in these firms in relation to their position in product, labour and financial markets. The main conclusions are that increases in product market power reduce productivity, productivity growth increases when unions become weaker, and higher levels of debt are associated with higher productivity growth rates. The latter phenomenon is attributed to the ‘discipline of debt’ which leads managers in situations of high debt to reduce slack and become more active. Nickell and Nicolitsas (1997) focus on restrictive work practices, i.e. rules about which sort of workers are allowed to undertake which sort of jobs. These restrictive practices are liked by workers and unions because they often reduce their pace of work and provide them with more control over their work environment. The restrictive practices are disliked by firms because they reduce productive efficiency. This difference in appreciation between workers and firms provides a scope for negotiations which is smaller if a firm faces a worsening of its financial or market position. Nickell and Nicolitsas analyse a panel of UK manufacturing firms to find that if market power declines or the financial health of firms becomes worse restrictive work practices are reduced, which causes productivity to increase. Nickell and Nicolitsas (1999) return to the issue of whether financial pressure affects firm behaviour. The analysis is again based on a panel of UK manufacturing firms. The ratio of interest payments to cash flow is found to have a large negative effect on employment. Nickell and Nicolitsas conclude that this represents a significant monetary policy channel from interest rates to unemployment as most of the lost employment materialises as an increase in unemployment. Nickell et al. (2001) discuss the issue of what firms do in bad economic times. They study a panel of British manufacturing firms from the early 1980s finding some evidence for the ‘pit-stop’ view of recessions in which firms in bad times introduce productivity-improving innovations.

3.4 Inflation

Inflation is another topic on which Steve has conducted research. Although interesting in itself, it is also part of the labour market theory that will be discussed in more detail below. Nickell (1987) addresses the issue of why wage inflation in the 1980s in Britain was so elevated despite the high level of unemployment. The starting point of the paper is the macroeconomic framework, presented in more detail below in Section 5.1, in which price setting and wage setting are influenced by the unemployment rate. When inflation is stable, unemployment is at its equilibrium value which is determined by wage pressure variables such as union power, labour taxes and unemployment benefits. Using annual data over the period 1956–1983 to estimate the parameters of the wage-setting and price-setting equations, Steve concludes that the increase in the share of long-term unemployed in total unemployment significantly reduced the responsiveness of wages to unemployment. Nickell (1990a) is also on inflation and equilibrium unemployment in Britain, announcing that in 1991 a book on unemployment will appear with more details (see Section 5.3, below). Nickell and Quintini (2003) discuss nominal wage rigidity in Britain, concluding on the basis of an analysis of micro data from the New Earnings Survey that, although there is some rigidity in nominal wage changes, the macroeconomic impact of this is very modest.

3.5 Methodology

Steve has a number of methodological papers that are not directly related to his main work on unemployment. Among these there is one which became his most cited paper. First there was Nickell and Tymes (1976) which ‘develops a simple iterative procedure for deriving linear decision rules which provide the optimal control policy for a stochastic dynamic linear system’ (ibid.: 323). However, Steve’s most cited paper, Nickell (1981), is 10 pages long and discusses the biases that occur in dynamic models with fixed effects. In static fixed effects models, the error term corresponding to the i th individual in the t th time period is usually assumed to consist of three components, one individual specific, one time specific and one that is both time and individual specific. In a simple static model in which the time specific fixed effect is ignored, this would look like

$$y_{it} = \beta x_{it} + f_i + \varepsilon_{it} \quad (1)$$

where y is the dependent variable, x is a vector of variables, and β is a vector of parameters including an intercept. In a typical panel data set, the number of individuals is large, while the number of time periods is small. An important question is whether f_i , the individual-specific fixed effects can be treated as a random variable since treating f_i as a parameter introduces an enormous number of additional parameters. The problem is that the individual fixed effects represent unobserved characteristics that are likely to be correlated with the other variables in the model. Since this may create biases in the estimation, the fixed effects need to be eliminated. The standard technique is to subtract the mean of (1) from (1) itself to get

$$y_{it} - y_i = \beta(x_{it} - x_i) + (\varepsilon_{it} - \varepsilon_i) \quad (2)$$

which removes the fixed effects and the constant and can be easily estimated using ordinary least squares (OLS). Nickell (1981) notes that this simple way out is not available if there is a lagged dependent variable such that

$$y_{it} = \rho y_{it-1} + \beta x_{it} + f_i + \varepsilon_{it} \quad (3)$$

where usually $|\rho| < 1$. In this case, subtracting the mean of (3) from (3) removes the fixed effects:

$$y_{it} - y_i = \rho(y_{it-1} - y_i) + \beta(x_{it} - x_i) + (\varepsilon_{it} - \varepsilon_i) \quad (4)$$

However, Eq. (4) cannot be estimated by OLS because the correlation between y_{it-1} and ε_i would introduce a bias. Nickell (ibid.) is devoted to an analysis of this dynamic panel data bias which in subsequent papers is sometimes referred to as ‘Nickell’s bias’. Steve did not follow up on this issue but his 1981 paper inspired others to develop a new generation of dynamic panel data models such as the one by Arellano and Bond (1991) which is now standardised in the statistical software package Stata.

4 Job Search, Employment and Wages

4.1 Job Search

Nickell (1979a) is a pioneering study on the analysis of individual unemployment duration data in which the exit rate from unemployment is modelled using a hazard rate model. The paper marks the start of Steve’s work on unemployment, initially on micro data, later on using cross-country time series data. Nickell (1979b) is a companion paper. The empirical part

of both papers is based on a sample of 426 unemployed males interviewed for the British General Household Survey of 1972. Of these unemployed it is known how long they were unemployed at the time of the interview but there is no information about their completed spells of unemployment. From the elapsed duration of unemployment at the interview, conditional probabilities to leave unemployment in a particular week are derived and from this expected durations can be calculated. The speed by which unemployed leave unemployment and find a job can be modelled as a search-theoretic phenomenon. The assumption is that the unemployed search by sampling wage offers from a wage distribution using a simple algorithm: if the wage offer exceeds a certain threshold value, the reservation wage, it will be accepted. If the wage offer is below the reservation wage, the offer will be rejected and the unemployed worker keeps searching for the next wage offer. The reservation wage is determined by the costs and expected benefits of the search. If unemployment benefits exist, the costs of search are lower and the search will take more time, i.e. the unemployment duration is longer. This theoretical concept has an empirical counterpart. Steve assumes that the conditional probability for individual i to leave unemployment during a particular week from time t to time $t+1$, conditional on having entered unemployment at time $t-s$ and on still being unemployed at t , has a logit distribution

$$p(x_i(t_i, s), s, v) = (1 + \exp(-(\beta x_i(t_i, s) + v)))^{-1} \quad (5)$$

where t_i is the date of the interview, x is a vector of personal characteristics and demand-side variables also including benefits and the mean of the wage offer distribution, β is a vector of parameters and v represents unobserved characteristics such as motivation to search for a job. To estimate the relevant parameters, various issues have to be taken into account. One issue is that there is only information about incomplete unemployment durations. Another issue is that there are unobserved characteristics that may affect the probability of leaving unemployment. Steve assumes that v comes from a discrete distribution with two points of support representing two types of individuals who differ for unknown reasons in their exit rate from unemployment. The difference in the exit rate can be estimated as well as the share of workers of each type. So, the assumption is that $v=v_1$ with probability ϕ and $v=v_2$ with probability $(1-\phi)$. The main findings are that the outflow from unemployment is higher for married men, lower for men with ill health, increasing with local labour demand and decreasing with age. The outflow from unemployment is also affected by the unemployment

benefit replacement rate in the first 20 weeks but not later on. Unobserved characteristics do not play a role.

Lancaster (1979) analyses a sample of 479 British male unskilled, unemployed workers for whom some information about their duration of unemployment was available. Lancaster uses a specification in which the hazard rate ϑ is the product of functions of observed characteristics, elapsed duration of unemployment and unobserved characteristics:

$$\vartheta(t|x, v) = \psi_1(x)\psi_2(t)v \quad (6)$$

where x is a vector of observed characteristics, t is elapsed durations, and v represents the unobserved characteristics. Usually, for the observed characteristics an exponential specification is used: $\psi_1(x) = \exp(\beta x)$ where β is a vector of parameters. This set-up became known as a mixed proportional hazard (MPH) specification. Lancaster assumed a Weibull distribution for duration dependence, $\psi_2(t) = \alpha t^{\alpha-1}$, and assumed that v follows a gamma distribution with unit mean and variance σ^2 . Thus, Lancaster was able to test whether there was duration dependence in the hazard rate ($\alpha \neq 1$) and whether or not there was unobserved heterogeneity that mattered ($\sigma^2 > 0$). The main conclusions are that age, local unemployment and replacement rates have negative effects on the outflow of unemployment. Lancaster finds that negative duration dependence and unobserved heterogeneity ‘compete’. If it is assumed that there is no duration dependence ($\alpha = 1$), there is significant unobserved heterogeneity ($\sigma^2 > 0$). If it is assumed that there is no unobserved heterogeneity ($\sigma^2 = 0$), there is significant negative duration dependence ($\alpha < 1$). However, it is not possible to estimate both parameters at the same time and get sensible results.

Later on, a detailed comparison of the papers by Steve and Tony Lancaster was made in Lancaster and Nickell (1980) which has the subtitle ‘Read before the Royal Statistical Society on Wednesday, December 12th, 1979’ and contains a lengthy discussion of the two papers. This discussion is followed by the reply of the authors which was provided later, in writing. An important element in the discussion is whether it is possible to disentangle the effects of unobserved heterogeneity and duration dependence. Elbers and Ridder (1982) show the conditions under which it is possible to separate the two effects. Lancaster’s functional form of unobserved heterogeneity and duration dependence was later on used by many other researchers. However, Nickell’s intuitive idea to assume a discrete distribution for unobserved heterogeneity with two (or more) points of support also became popular due to Heckman and Singer (1984). Steve did not have an extensive follow-up on his pioneering work. Narendranathan et al. (1985) and

Narendranathan and Nickell (1985) are two companion papers analysing the duration of unemployment. The first focuses on the effects of unemployment benefits, while the second presents a structural model in which wages and reservation wages are also included.

4.2 Employment and Wages

Nickell (1978) is on employment and labour demand over the cycle. It is a theoretical paper indicating how fixed costs of adjustment affect the dynamics of labour demand. Nickell (1984a) is on the determinants of manufacturing employment in the UK. Steve estimates a model that takes into account expectations, adjustment costs and aggregation of labour types. Nickell (1986) is Steve's contribution to the first volume of the *Handbook of Labor Economics* dealing with dynamic models of labour demand.

An early paper on the role of unions in wage determination is Nickell (1977c) in which Steve presents parameter estimates of industry-level wage equations for Britain for the years 1966 and 1972. The main conclusion is that higher wages are associated with increased collective bargaining coverage. Wages of both men and women are higher in industries with a higher coverage. Although the association is stronger for women, Steve notes that 'unions are not entirely blameless when it come to the significant underrepresentation of women in high wage industries' (ibid.: 206). Another early paper on collective bargaining is Layard et al. (1978) analysing the role of unions in wage inflation and confirming the relationship between coverage and wages. Traditional bargaining theory assumes that wages and employment are determined sequentially. Once wages are negotiated, employment is determined through the labour demand curve because, conditional on wages, this gives firms the highest profits. The more recent theory of efficient bargaining suggests that it might be welfare improving if firms and unions bargain on wages and employment simultaneously because it is then possible for both firms and unions to be better off. In this case, the wage-employment combination will not be on the labour demand curve and it is possible that unemployment is reduced. Analysing data from 219 UK manufacturing firms over the period from the early 1970s to 1982, Nickell and Wadhvani have a series of three papers on wages and employment. In the first paper, Nickell and Wadhvani (1988) analyse whether or not unions bargain over employment, i.e. whether bargaining is efficient. They conclude that their 'evidence is plausibly consistent with the labour demand model' (ibid.: 733). They also refer to a paper, later published as Layard and Nickell (1990),

which presents a theoretical model showing that the macroeconomic implications of bargaining over employment are different from the inferences based on a partial equilibrium model. Depending on the type of production function, it is even possible that unemployment is higher under efficient bargaining. In the second paper, Nickell and Wadhvani (1990) address the issue of insider forces in wage determination. In a competitive market, an increase in labour productivity at the given wage will induce firms to expand output and employment. However, with insider forces productivity gains may translate into higher wages. The main conclusions of this second paper are that insider forces are important but also that outsider factors, such as aggregate unemployment and the share of long-term unemployment, have an impact on wage determination at the firm level. The third paper, Nickell and Wadhvani (1991), also indicates that unions in the private manufacturing sector do not bargain over employment.

Denny and Nickell (1991, 1992) find that the presence of unions has a negative effect on investment by firms. This may be caused by unionised workers capturing some of the return of new projects in the form of higher wages thus reducing the incentive to invest. It may also be because the presence of unions makes it more difficult and therefore more costly to install new machinery when this requires a change in work practices. Nickell and Kong (1992) is on the role of insiders in wage determination. Analysing British industry data over a period of 25 years, the main conclusions are that insider forces are important and are related to union power and product market power. Also, the state of the aggregate labour market is important. Bell et al. (2002) and Faggio and Nickell (2005) confirm that wages are influenced by unemployment rates.

5 Unemployment

5.1 Prologue

Following a few papers on unemployment durations based on individual data, Steve's first paper dealing with aggregate unemployment is Nickell (1979c) in which, among other things, he discusses temporary employment subsidies introduced in Britain in the mid-1970s. According to Steve, this policy appears 'to have been rather successful in fulfilling its objective' (ibid.: 218). Nickell (1979d) shows that the number of unemployment spells over a worker's lifetime is influenced a lot by their level of education. Layard and Nickell (1980) discuss marginal employment subsidies as a

possible anti-unemployment measure. Under such a scheme, any firm which expands its employment will be paid a subsidy for each additional job it provides above its average level of employment during some base period. The performance of the marginal employment subsidy depends on the size of the deadweight subsidy to additional jobs that would have been provided anyway. Andrews and Nickell (1982) present a non-competitive model of the labour market with a price-setting equation, a wage-setting equation and a level of unemployment associated with a constant inflation rate. Aggregate unemployment is partly explained by some labour market institutions, in particular, taxes, employment protection legislation, unemployment benefits and union power. Nickell (1982) is on the determinants of equilibrium unemployment in Britain where equilibrium is defined as a situation in which inflows into unemployment equal outflows from unemployment. The inflow rate into unemployment is $A = (I/N)$, where I is the number of workers flowing into unemployment and N is the number of employed workers. The outflow rate from unemployment is $B = (O/U)$, where O is the number of workers who leave unemployment and U is the number of unemployed workers. There is a steady-state equilibrium in unemployment when $I = O$. Then, the equilibrium unemployment rate is $u^* = U/(U + N) = A/(A + B)$. Steve's strategy is to estimate the parameters of A and B and then compute u^* . The role of labour market institutions is limited to unemployment benefits, unfair dismissals and pressure put on the unemployed to obtain work.

Layard and Nickell (1985a) develop a structural macroeconomic model of the labour market with three equations, labour demand, price determination and wage determination, explaining the non-accelerating inflation rate of unemployment (NAIRU) and deviations from it. Using British data over the period 1954–1983, both the natural rate of unemployment and the actual rate of unemployment are found to have increased significantly. This rise in the natural rate of unemployment is attributed to increases in employers' labour taxes, unemployment benefits and union power. In Layard and Nickell (1985b), the three equation macro model is estimated for five countries: France, Germany, Japan, the UK and the USA. Union power is found to have had a significant effect in all countries except the USA; no effect of unemployment benefits is found, while taxes had significant effects only in the USA and the UK. Layard and Nickell (1986) are on male unemployment in Britain: it rose from 2% in the 1950s to 17% in 1985. The parallel increase of equilibrium unemployment is attributed partly to unemployment benefits and mismatch but mostly to union militancy and to an increase in employment protection which made employers reluctant to fill vacancies. Bean et al. (1986) is a study of the rise in unemployment in 19

OECD countries. One of the conclusions is that structural differences in labour markets can be related to national differences in institutional and social characteristics.

5.2 Magnum Opus

In 1991, the magnum opus, *Unemployment: Macroeconomic Performance and the Labour Market*, written jointly with Richard Layard and Richard Jackman, was published. The book builds on many previous studies often single authored by Steve but also many jointly written with Layard. It is a great example of an analysis that combines various issues that are rarely analysed in combination. There is a distinction between stocks and flows in the labour market. Long-term unemployment is more than an indicator of labour market performance but it has an effect on wage formation because it reduces the effectiveness of the matching between the unemployed and vacancies. Employment is more than a fixed number of jobs that cannot be redistributed without costs, i.e. there is no lump of labour. There are alternative theories to explain ‘the facts’.

Unemployment is assumed to be the result of imperfections in both the product market and the labour market. There is a price-setting relation in which imperfectly competitive firms determine product prices as a markup over the nominal wage. There is a wage-setting relation in which an imperfectly competitive labour market wages are determined given the price level. There are several explanations for the wage-setting equation ranging from efficiency wages via unions and bargaining to search and matching models. In the model, there is a certain unemployment level that reconciles price setting and wage setting: this is the natural rate of unemployment, i.e. the NAIRU.

The (simplified) structure of the model is as follows:

$$\text{Price setting: } p - w^e = \beta_0 - \beta_1 u \quad (\beta_1 > 0) \quad (7)$$

where p is log price, w^e is log expected wages, and u is the unemployment rate.

$$\text{Wage setting: } w - p^e = \gamma_0 - \gamma_1 u \quad (\gamma_1 > 0) \quad (8)$$

where w is log wage and p^e is log expected prices. In equilibrium, prices and wages are equal to their expected levels and this determines the equilibrium unemployment rate, u^* :

$$\text{Equilibrium unemployment: } u^* = (\beta_0 + \gamma_0) / (\beta_1 + \gamma_1) \quad (9)$$

Any factor that exogenously raises wages (γ_0) or prices (β_0) raises the equilibrium unemployment rate. Any factor that raises real wage flexibility (γ_1) or price flexibility (β_1) reduces the equilibrium unemployment rate. In a non-equilibrium situation, there is a relationship between changes in inflation and the difference between the actual unemployment rate and the equilibrium unemployment rate:

$$\Delta^2 p = -\theta(u - u^*) \quad (\theta > 0) \quad (10)$$

where $\Delta^2 p = \Delta p - \Delta p_{-1}$ is the change in inflation. This is a standard Phillips curve relationship. Assuming that the equilibrium unemployment rate depends on labour market institutions z , Eq. (10) can be rewritten as:

$$u = \gamma z - \delta \Delta^2 p \quad (\delta > 0) \quad (11)$$

Thus, if inflation is constant ($\Delta^2 p = 0$), labour market institutions are the sole determinants of unemployment, which in this case is at its equilibrium value ($u = u^*$). Referring to u^* , Layard et al. (1991) note that the term non-increasing inflation rate of unemployment would be a more accurate term than the NAIRU. If inflation is increasing, unemployment goes down; if inflation is decreasing, unemployment goes up. Taken to the limit, unemployment can only be reduced through a reduction of equilibrium unemployment for which changes in labour market institutions are needed. Eq. (11) has been estimated frequently using cross-country time series data. Often, but not always, five-year or six-year averages over different time periods are used instead of yearly information to remove the effect of cyclical fluctuations.

The main conclusion of the analysis presented in the book is that distortions in the labour market are predominantly related to the system of benefits and to the process of wage determination. Unemployment benefits introduce a moral hazard problem because it is less costly for workers to remain unemployed. In wage negotiations, decentralised unions and employers set the wage too high because they do not take into account negative spillovers on employment. Early retirement and work-sharing are policies to be advised against because they are based on the lump of labour fallacy. This is the idea that output and thus the number of working hours is fixed and can be redistributed without costs. The main reason why the idea of a lump of labour is a fallacy is the response of wages and prices. If unemployment were to be reduced initially through early retirement or shorter working hours, wages would increase, for example, because the union

bargaining position improves. Such a wage increase would induce firms to charge higher prices, thus causing inflation to go up. This rise in inflation can only be stopped if unemployment goes up, back to its original equilibrium value.

The first print of the book on unemployment was in 1991. In 2005, a reprint edition was published. This reprint edition had the same content as 1991, except for an extensive introduction stating that the book had ‘stood the test of time’ (Layard et al. 2005: xiii). The first print of the book was reviewed rather critically by Phelps (1992), while Blanchard (2007) wrote a favourable review of the reprint version. Both review articles were published in the *Journal of Economic Literature*, and thus, it is the only book about which two review articles have appeared in this journal.

5.3 Follow-up

The book on unemployment popularised the analysis of the relationship between labour market performance and labour market institutions. It was by no means the end of Steve’s research on unemployment issues. He wrote several review papers on unemployment. Nickell (1990b) presents an extensive survey of the determinants of unemployment, providing again a motivation for the macroeconomic model consisting of wage setting, price setting and long-term unemployment being determined by supply-side factors. Nevertheless, according to Steve, ‘pinning down the supply side factors which determine levels in the long run has proved to be very tricky’ (ibid.: 431) and ‘a lot has been achieved, but we remain a long way from a generally accepted view of the fundamental causes of unemployment’ (ibid.). Nickell and Bell (1996) compare European and US unemployment to investigate whether the differences in development are due to a shift in demand for the unskilled on both sides of the Atlantic, with relative wages being rigid in Europe and flexible in the USA. The conclusion is that this is not what has been going on. In many European countries, the demand for both skilled and unskilled workers has fallen.

Nickell (1997) is one of Steve’s most cited articles. It compares unemployment and labour market rigidities in Europe and North America. Nickell (1998) is a companion paper with an overlapping empirical part (the difference being that one has the owner-occupation rate as explanatory variable, while the other does not, even if this does not make much of a difference). Based on 20 OECD countries and two time periods—1983–1988 and 1989–1994—unemployment rates are regressed on labour market

institutions and changes in inflation. Unemployment benefits—both level and duration—as well as union density and union coverage and the tax rate have positive effects on unemployment, while active labour market policies and coordination of wage bargaining have negative effects. Employment protection has a negative effect on short-term unemployment only. Nickell (1997) concludes that high unemployment is related to generous unemployment benefits in combination with little or no pressure on the unemployed to obtain work. Furthermore, high unionisation is an important determinant of unemployment when wages are bargained collectively and there is no coordination between either unions or employers in wage bargaining. Unemployment is also influenced by high overall taxes or a combination of high minimum wages for young people associated with high payroll taxes. Finally, unemployment is high in the case of poor educational standards at the bottom end of the labour market. Labour market institutions that are more or less irrelevant are: employment protection legislation, generous levels of unemployment benefits and high union density and coverage as long as they are offset by high levels of wage coordination. Nickell (1998) stresses that understanding differences in unemployment rates between countries is easier than understanding why unemployment at the end of the 1990s was so much higher than in the 1980s: '[W]e do not have a really satisfactory answer' (ibid.: 813). The main reason is that the factors that could explain the rise in unemployment from the 1960s to the 1980s have lost their influence when comparing the 1960s to the 1990s: '[I]ndustrial militancy is no worse, oil and commodity prices are no higher, benefit systems are not much more generous, real interest rates are not much higher and labour markets are not much more rigid' (ibid.: 814).

Nickell and Layard (1999) provide a general overview of the relationships between unemployment and labour market institutions. They conclude that the main institutions influencing unemployment are unions and social security systems. In addition, they also claim that to reduce unemployment, governments should encourage product market competition to reduce union power and eliminate the negative effect of unions. Finally, governments should link the reform of unemployment benefit systems to active labour market policies in order to move people from welfare to work. The last sentence of the paper reads that, 'by comparison, time spent worrying about strict labour market regulations, employment protection and minimum wages is probably time largely wasted' (ibid.: 3030).

The work by Steve and Richard Layard has had quite a few follow-up studies. Belot and van Ours (2001), for example, argue that the role of each labour market institution depends on the rest of the institutional

framework. They show that if country fixed effects are included and the analysis is restricted to direct effects of labour market institutions only home ownership has a significant (positive) effect on unemployment rates. However, if in addition interactions between labour market institutions are allowed, there are significant effects of centralised bargaining in combination with employment protection and union density and of unemployment benefits, while tax rates turn out to be not significant. Belot and van Ours (2004) find that there is a direct negative effect on unemployment of the unemployment insurance (UI) replacement rate, while there are positive interaction effects between taxes and UI replacement rates and between union density and centralisation. Other studies on the effect of labour market institutions on labour market performance include Scarpetta (1996), Elmeskov et al. (1998), Daveri and Tabellini (2000), Arpaia and Mourre (2005), Bassanini and Duval (2006, 2009), van Ours (2015), and Dixon et al. (2017).

The discussion on whether unemployment is influenced by changes in labour market institutions in combination with economic shocks or by changes in labour market institutions alone goes back to Blanchard and Wolfers (2000) who claim that labour market institutions in Europe did not change a lot in the 1980s, whereas unemployment rates went up substantially. They investigate in particular the interactions between labour market institutions and economic shocks, finding that shocks have a larger positive effect on unemployment when unemployment benefits are high and long-lasting, employment protection is strict, union density is high, and coordination in wage negotiations is low. Nickell et al. (2005) conclude that changing labour market institutions provide a reasonably satisfactory explanation of the broad pattern of longer-term unemployment shifts in the OECD. Changes in benefit systems, increases in labour taxes, changes in union variables and employment protection contribute to changes in unemployment. They conclude that interactions between average values of institutions and shocks make no significant additional contribution to the understanding of OECD unemployment changes. Thus, the paper deals with the criticism by Blanchard and Wolfers (*ibid.*). The 2005 paper is the last of Steve's papers in which cross-country time series data are used to study the effects of labour market institutions on unemployment. The idea that to understand differences in labour market performance labour market institutions have to be taken into account is still very much alive (Boeri and van Ours 2013). The highly influential OECD Jobs Study in 1994 mirrors the ideas of Steve and his co-authors, while their research is also reflected in various other OECD and IMF papers.

6 Conclusion

One of the main characteristics of labour market institutions is that they rarely change and, if they do, the change is more often than not marginal. This makes it hard to establish the labour market effects of a change in a particular institution, let alone the simultaneous change of many institutions. In his work on unemployment, Steve is well aware that it is hard to make causal inferences on the basis of aggregate data. Summarising the complexity of any labour market institution in one or two numbers is indeed a serious limitation. In a 1998 paper, Steve stresses that differences in labour market institutions give ‘some understanding of why unemployment varies such a great deal across different countries’ (ibid.: 813), while in a 1997 paper he emphasises the limitations of cross-country time series analysis: ‘[W]e see them as a helpful overview of the correlations in the data and nothing more’ (ibid.: 65). Establishing causality is often restricted to particular labour market institutions focusing on a specific policy change or discontinuity in coverage in a certain country. There are many more studies on the effects of unemployment benefits (Tatsiramos and van Ours 2014) or particular active labour market policies (Card et al. 2010) for which variation over time or discontinuities in coverage can be exploited than there are on employment protection legislation, union coverage or wage bargaining, where a clean research design is harder to find.

In many of Steve’s publications, he advocates product market competition to reduce the influence of unions. This does not imply that he is an advocate of a labour market without institutions. After all, the 1991 book on unemployment was dedicated ‘to the millions who suffer through want of work’ (Layard et al. 1991: v). In the 2005 version of the book, it is stressed that it is important to not just focus on jobs but to make a distinction between unemployment and inactivity: ‘[I]f we are concerned with human misery and frustration, unemployment and inactivity are totally different’ (Layard et al. 2005: xxxix). In response to Minford et al. (1983) who among other policies recommend a reduction in the level of unemployment benefits such that some 15% of the unemployed will suffer a 20% fall in real disposable income, Nickell (1984b) states that the expected reduction in unemployment is based on a misspecified model and, once corrected for, the effects of the proposed policies ‘are very much smaller than those which he [Minford] presents. Relative to the hardship and social disruption which these policies would cause, the benefits then seem rather small’ (ibid.: 953). In an assessment of the Thatcher years, Layard and Nickell (1989) conclude that there are two main pluses, the increase in productivity and the fall in inflation. However, the poor unemployment performance and the increase in

inequality are considerable minuses. In Nickell (2004), Steve states that relative poverty in Britain has risen massively since the late 1970s because of increasing worklessness, rising earnings dispersion and benefits indexed to prices, not wages: ‘So poverty is now at a very high level’ (ibid.: C24).

Steve’s research was not developed according to a master plan. Surprisingly, some of his major contributions to the literature were almost stand-alone. Or, perhaps unsurprisingly. Describing his research, Steve has stated that: ‘I worked it out and moved on’ (personal conversation). Clear examples of working it out and moving on are the dynamic fixed effects panel data model and the model explaining individual unemployment durations. Steve was at the forefront of these two models which were then further developed by other researchers. Steve never pursued a PhD but supervised some 70 PhD students who successfully completed their doctorate mostly under his sole supervision, probably more than any other UK economist. From 2011 to 2016, he worked at the UK Office for Budget Responsibility because, in his own words, he ‘wanted to do something useful’ (personal conversation).

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35

Christopher A. Pissarides (1948–)

Etienne Wasmer

1 Introduction¹

It is a great honour to write about Christopher Pissarides' contributions to economics. My own 'match' with his work started in the year before studying at LSE: my co-students on the MRes (Econ) degree at DELTA (a forerunner of the Paris School of Economics) were, in the mid-1990s, in search of a robust framework to analyse the labour markets in Europe. Two decades after the oil shocks, Western European economies had been strongly hit by a deep economic recession. Europe faced a rapid rise in unemployment in all segments of the working age population, including, and this was new, the segment of most skilled workers. We were searching for a relevant model of labour markets where unemployment would not be either voluntary or involuntary and where Keynesian demand effects would not be the only factor behind the level of employment. We found the perfect framework in Chris's *Equilibrium Unemployment Theory* (Pissarides 1990). It was not yet a classic read, but we felt it would soon become one. In his approach,

¹A part of this chapter is a translation of a survey previously written by the author (Wasmer 2011) reproduced here with the permission of, and with thanks to, the editor of *Revue d'Économie Politique*. I also thank Christian Haefke and Etienne Lalé for a discussion on the first draft and Rachel Ngai and Claudio Michelacci for insightful exchanges. Samuel Fitoussi provided valuable assistance with the translation. All remaining errors are mine.

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equilibrium unemployment was the outcome of a balance between job creation and job destruction. The analysis would make it possible to analyse structural policies, demand effects, the impact of changes in the interest rate and active and passive labour market policies. *Equilibrium Unemployment Theory* offered a beautiful explanation of the medium-run unemployment rate, arising as a balance between the hiring costs and labour market congestion on the one hand, and profits, technology and wage-setting institutions or individual bargaining power of workers on the other. Digging further, one would find well-defined second best concepts and interesting dynamic implications that matched the data qualitatively. The framework could also be extended to growth and price dynamics. That looked easy. What more could we ask? We adopted the framework for all issues concerning labour markets, and this is how it all started for a generation of labour economists. With the book in mind and an illegal photocopy in our luggage (the first edition published in 1990 had not been reprinted and was no longer in bookstores), a few of us crossed the Channel to attend courses in the PhD programme at the London School of Economics (LSE) and I naturally asked Chris to supervise my doctorate.

Equilibrium Unemployment Theory was a ‘mid-term’ achievement: it was the culmination of what by then was two and half decades of consistent work on the determinants of unemployment in an equilibrium labour demand and supply framework. This research programme would, however, be further developed by Chris and his main co-authors in the subsequent two and half decades: he would systematically fill in the gaps in the theory. In particular, Chris went on to analyse endogenous job destruction, empirically measuring the so-called matching function, discussing the design and inefficiencies of labour policies (employment protection, unemployment insurance, training) and exploring the issue of creative destruction.

In doing so, Chris was also contributing to the recognition that Keynesian solutions to unemployment based on cyclical deficits or monetary policy were neither a good starting point nor a final response to the unemployment problem in Europe. This achievement is probably overlooked, but it may be one of his most important, with a long-lasting impact on economic policy debates, especially in Europe. At the same time, however, it does not mean that the demand side is forgotten about; indeed, factors which affect the demand for labour are clearly present in the framework. Such mechanisms are actually prominent in Chris’s policy discussions on the post-Great Recession, in particular on the management of the Greek crisis by international institutions.

After the period devoted to the study of frictional unemployment, a medium-run issue, Chris's work has more recently focused on longer-run issues such as the sectoral reallocation of labour, technical change and growth. The contiguity is obvious: these are cousin topics of those covered in his work on unemployment because there is only a small step from a labour matching function to the process of reallocation of labour across sectors. Finally, and most recently, in public lectures Chris has addressed the important and related topics of robotisation of the economy, and artificial intelligence and its labour market impact, inspired by his early contributions to the capitalisation effect of growth.

If there is a common denominator to all these topics, it is the central idea of the reallocation of labour across jobs, occupations, skill categories and places. The matching function described later in this chapter is indeed a very convenient tool with which to highlight the costs associated with reallocation. These costs are either time costs or financial costs and have a significant cyclical behaviour that requires proper and rich analytical tools. Such reallocation costs notably depend on the current value of inputs such as the number of job seekers, the effort they put into search and the number of firms willing to recruit and their advertising effort, and have been shown to be central and empirical determinants of unemployment levels and fluctuations.

It is impossible to summarise all the work done by Chris and its influence in the field a fortiori. Excellent literature reviews have been written, such as Mortensen (1986), Mortensen and Pissarides (1999a) in the *Handbook of Labor Economics* and their survey in the *Handbook of Macroeconomics* (Mortensen and Pissarides 1999b).

I will instead show the coherence of his matrix of analysis by discussing its inspiration, then developing the role of frictions in quantitative macroeconomic work, the role of frictions in economic policy prescriptions, the role of frictions in thinking about longer-term issues such as growth and sectoral change and, finally, the likely development of the approach beyond the labour market, in financial and goods markets in particular.

Chris's life and career, as portrayed in Nobelprize.org (2010), on which this section is drawn, have been rich. He was born in February 1948 in Nicosia, Cyprus. His parents had roots in the village of Agros in the Troodos Mountains. Both were at one time or another involved in the clothing business, with some success. Chris's schooling was disrupted by events related to independence from British colonial rule, and his years in Nicosia were often punctuated by the sounds of 'soldiers, flying bullets and bombs', as he would put it later. He then travelled to London to study for his A Levels,

followed by an undergraduate degree in economics at Essex University, where he came under the influence of Richard Lipsey, Michael Parkin and Chris Archibald. Despite a subsequent offer to study for a doctorate at Harvard and an early meeting with Dale Mortensen who offered to supervise his PhD at Northwestern, Chris chose to go to LSE. He would report that he experienced there a 'disorganised approach to doctoral studies' (*ibid.*); however, he was encouraged to read the classics, Keynes and Hicks under the guidance of his supervisor, Michio Morishima.

Having secured a PhD on search theory, Chris was appointed to a position in the research division of Cyprus's central bank in early 1974. Personal matters meant that he was, somewhat fortuitously, aboard one of the very last civilian flights out of Nicosia Airport before the overthrow of the Makarios government on 15 July 1974 by the Greek army, replacing it with Greek military rule. This was followed by an invasion by the Turkish army and the division of Cyprus, which has lasted to this day. Chris was stuck in Athens. The partition of Cyprus had two personal consequences: first, he lost all of his belongings and a piece of land he had just acquired; second, he was in desperation at not being able to return to Cyprus. When he heard that the UK had vacant academic positions, he considered them seriously.

Chris returned to the UK, taking up a one-year lectureship at the University of Southampton. He would soon get a position back at LSE, in 1976, where he has been ever since. It was on his return to LSE that Chris became involved with the labour research group being established by Richard Layard. Even though he did not consider himself to be a labour economist, Chris's association with the so-called Layard Group resulted in a fuller appreciation of the importance of the empirical implications of his research and helped shaped his focus on the then very topical problem of unemployment. Two research visits to the USA followed, including a particularly productive six-month stay at the Industrial Relations Section at Princeton University. Meanwhile, the influential Centre for Labour Economics was born at LSE, where Chris would meet and discuss economics with Bob Solow, Jacques Drèze, Edmond Malinvaud, Orley Ashenfelter, Olivier Blanchard and Rudi Dornbusch. However, Chris felt that his work on search theory was outside the dominant approach of the Centre, making it certainly more original but leaving it in relative isolation.

In 1990, Chris and his young family spent a year at the University of California, Berkeley. It was during this time that he began his collaboration with Dale Mortensen. Within a few months, this led to their famous endogenous job destruction paper, on which they worked together in many places, including the famous Bellagio Center in Italy. In the mid-1990s, when I got

to know him, Chris would devote more time to other activities, including academic administration at LSE: he chaired the Economics Department from 1996 to 1999, and a particular legacy was the decision to build a little bridge (a ‘passerelle’) connecting the Department to another building. Chris also had a major involvement with the University of Cyprus.

2 Origin and Influences

Working our way backwards, we can start at the main achievement, the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, awarded to Peter A. Diamond, Dale T. Mortensen and Christopher A. Pissarides in 2010 for their work on markets with search frictions, in large part covering the so-called DMP (Diamond–Mortensen–Pissarides) model.

The 2010 Prize was one of a long list of prizes which had, over the previous 15 years, rewarded work on market imperfections. In particular, Leonid Hurwicz, Eric Maskin and Roger Myerson received the 2007 Prize for mechanism design in the presence of asymmetric information, George Akerlof, Michael Spence and Joseph Stiglitz’s shared the 2001 Prize for the analysis of markets in the presence of asymmetric information, and the 1996 Prize was awarded to James Mirrlees and William Vickrey for their analysis of incentives in the presence of asymmetric information. More recently, the 2012 Prize was awarded to Alvin Roth and Lloyd Shapley for the theory of stable allocations and the practice of market design. The DMP model is also deeply connected to the foundations of game theory developed by John Nash (1994 Prize) and to the dynamic macroeconomic theory proposed by Tom Sargent (2011 Prize).

What made the search and matching approach novel was its careful discussion of the terms of economic exchange, as an input of production and not as an abstract construct or an absentee concept such as the Walrasian auctioneer. Indeed, most economic models assume that contact and thus exchange between two segments of a market is instantaneous, that this is technologically possible and that trade is limited only by the inability to conclude the exchange due, for example, to a lack of income, lack of production or the price not being suitable to satisfy both parties.

Instead, in the search approach, agents are only imperfectly informed of their environment and are not aware of all available exchange opportunities. This relative lack of knowledge can for instance be due to the importance of heterogeneity in the labour market, the geographical dispersion of economic opportunities, the variety of skills needed and the sectoral division of labour.

The search approach develops the foundations of this more realistic world where contacts and subsequent exchanges are costly. Agents need to devote significant resources to acquire knowledge about the information (time searching, opportunity cost). In the theory, after a contact occurs randomly, exchange opportunities may still not be realised, but this is the outcome of a rational decision taking into account the intrinsic difficulty in contacting alternative trading partners from that point in time.

The theory of search markets thus differs conceptually and drastically from that of perfect markets. Exchanges are the result of a well-defined economic process. The frictional approach only relies on imperfect information about trading opportunities and willingness to trade. Information can be assumed to be symmetrically distributed within the set of parties involved in exchange (the firm and the employee). The model includes informational capital, i.e. the existence of past search effort is capitalised in the economic value of the match for each agent. But it does not require information asymmetry among them, which complicates the analysis in introducing strategic interactions and additional informational rents. Of course, given its tractability, the frictional model can also easily integrate these asymmetries.

One can wonder why search theory did not gain more recognition earlier. The following quote, due to Kiyotaki and Moore (2001: 4) and emphasised, perhaps sarcastically, in Lagos and Wright (2002: 2), illustrates well the polarisation of the debates surrounding the importance of search frictions: ‘The matching models are without a doubt ingenious and beautiful. But it is quite hard to integrate them with the rest of macroeconomic theory—not least because they jettison the basic tool of our trade, competitive markets’. However, why this basic tool, ‘competitive markets’, should be kept *by whatever means* is a good question. The answer provided by Chris’s analysis is that the theory of competitive markets is a specific case, a point of zero mass in the universe of models of search and matching frictions: it is kept as a limit, but profoundly enriched. The search approach is a theoretical generalisation bringing additional degrees of freedom to the parametrisation of the model. By adjusting the key parameter of the model, namely the speed of adjustment of demand and supply, one can replicate many more labour market ‘facts’ relating to the duration of unemployment and unfilled vacancies.

Another characterisation of Chris’s work is the flow approach to labour markets: the model has parameters that allow for a perfect fit of the unemployment hazard rate, that is the transition rate from unemployment to employment, which is, after all, what jobless workers care about most. The model’s primary concern is not so much about the level of unemployment, a stock that does not directly inform us about the well-being of the unemployed

labour force. In that flow view of labour markets, having a job is the same as holding an asset with stochastic returns, the value of which can be characterised by transition flows. When reading a book chapter on asset trading at the LSE Library, Chris started to consider that a fruitful approach might be that the labour market be modelled as a frictional market and that jobs possessed asset values.² The Library was certainly inspiring: quiet, populated with students without an office and professors looking for rare books. The parallel between housing markets and labour markets is indeed an interesting one: both have vacancies, and transactions take time and are highly cyclical.

The origin of and a subsequent development within search theory was based on the idea of a non-degenerate distribution of wages that search frictions would naturally explain. Stigler (1961, 1962) clearly outlined the concept of search frictions. He initially sought to find out the reasons behind the disparity in wages between Chicago University graduates and argued that incomplete information on wages led graduates to accept offers in a possibly larger interval as long as wages were above a reservation wage.³ McCall (1970) introduced a formal analysis of the reservation strategy positing the wage distribution as given. Diamond's (1971) celebrated result established that there were in fact strong forces towards price convergence once prices were made endogenous. He assumed that consumers must pay a cost to know the prices of other sellers but considered the offers from sellers as endogenous. These two premises led to the famous Diamond Paradox. Transaction costs are insufficient to generate a price distribution, contrary to Stigler's intuition: in equilibrium, prices for a homogenous good are unique, but at the seller's monopoly level, that is far from the competitive level. Furthermore, the equilibrium price is discontinuous. When transaction costs tend towards zero, prices remain different from the competitive price. This illustrates the importance of the buyer's search costs: they do not need to be very high to have a significant influence.

As Diamond made it clear in his December 2010 Nobel Lecture in Stockholm, the interaction between search costs and firm behaviour explains his result in the goods market. Firms exploit the small local monopoly

²Credit for this anecdote should be given to Rachel Ngai. The chapter, according to Chris, was Karlin (1962).

³Stigler (1961, 1962) assumed the dispersion of prices and salaries. Only the distribution is known to economic agents. To have access to a draw from the distribution (obtain an exchange possibility, purchase a good or respond to a job offer), agents must pay a cost, which can either be a direct cost, a sampling cost or simply the cost of waiting for a new offer. Offers come in randomly and their dispersion is exogenous.

power conferred to them by the existence of search costs, and their collective actions contribute to the raising of prices. It turns out that this result that small transaction costs have large effects on equilibrium transaction prices is a result that also holds true in the labour market: hiring costs are not large, but their impact on frictional unemployment tends to be very important quantitatively, as compared to other entry costs for firms.

However, the existence of a unique price was not entirely satisfactory. In developing the competition between firms to attract consumers (Burdett and Judd 1983), equilibrium price dispersion would in fact arise in more frequent contexts. Labour market analysis soon integrated this insight and nice mechanism with the work of Burdett and Mortensen (1989, 1998). An entirely new research group emerged, driven by Mortensen (2005), Manning (2003), Bontemps et al. (1999, 2000), Postel-Vinay and Robin (2002), Albrecht and Axell (1984), Albrecht et al. (2006), Menzio and Shi (2011), and Galenialos and Kircher (2009), among others. This approach would augment Diamond's (1971) model. Search frictions not only would be a key in generating unemployment but also would lead to equilibrium wage dispersion.

This is where the search literature diverged, between the *random search approach* and the *directed search approach*. In the directed search approach, interpreted in a broad sense, workers target offers depending on the wage of the position they are prospecting for. The wage dispersion would be both a key driving force of the equilibrium and sometimes an outcome of the equilibrium, while in some cases the equilibrium converges to a single point. In the random search approach, workers would instead try to establish contacts with vacancies prior to knowing the wage and would apply a reservation rule to accept it or not. Wage dispersion might arise, but only as a side product.

In the random search approach, most of the insights in the macroeconomic models developed by Chris are orthogonal to the discussions of the causes of underlying heterogeneity in wages. Frictions were sufficient to provide a rich set of determinants of unemployment, as well as some of its cyclical properties, as in his 1985 seminal paper (Pissarides 1985). Chris did not really consider wage dispersion as a key driving force of equilibrium unemployment. It was therefore more convenient to assume *random search* as he did in most of his contributions. This simplification was instrumental in that it helped to address various issues that would however still involve heterogeneity: with a matching function, one can characterise the three flows (hiring, separation and mobility) that depend on both a large number of macroeconomic variables such as productivity, mark-ups or wages and individual heterogeneity; one can characterise long-term unemployment

and in particular discuss the important issue of skill de-cumulation and re-accumulation (as in Pissarides 1992) and its influence on the incidence of long-term unemployment; and one can address individual insurance against idiosyncratic risks and policies such as employment protection (as in Pissarides 2001, 2010). However, the benchmark model can easily generate a non-degenerate distribution of wages (as in the stochastic job matching extension in Chapter 6 of Pissarides (2000) for instance).⁴

A common denominator to these works and more generally to all of Chris's contributions is the remarkable ability to summarise a complex problem into a clear question and a neat answer, thanks to the right assumptions. This particular skill impressed his PhD students a lot, not only the simplifying assumptions leading to rich answers, but how they often lead to path-breaking modelling strategies, with an interesting periodicity of roughly five to six years. Thus, there were the building blocks of matching theory created in the late 1970s, the 1985 *American Economic Review* contribution to business cycles, the full integration realised in the first edition of *Equilibrium Unemployment Theory* in 1990, the 1994 *Review of Economic Studies* article on endogenous job destruction with Dale Mortensen, the 2001 *Journal of Economic Literature* survey on the matching function with Barbara Petrongolo and the 2007 *American Economic Review* piece with Rachel Ngai on balanced growth.

In some perhaps less known works, Chris followed the same logic of a simplifying assumption and inspiring theory: among them, the 'unrelated' work for the World Bank on learning by trading⁵ was presented at LSE in one of the macro seminars in the 1990s with these words from Chris: 'You will be surprised that there is no matching function in this work'. When Chris presented the learning function $F(B, A-B)$ where A and B are the number of varieties of capital goods in the 'North' and the 'South', and F is a homogenous function, his colleague Charles Bean could not resist

⁴This discussion may not pay full justice to the considerable impact of the meeting at the University of Pennsylvania in January 1969 of nearly all the contributors to the classic Phelps (1970), as highlighted in a 2006 Nobel Prize-related 'scientific background' release about Phelps: 'Phelps's work here is a precursor of the search and matching theory of unemployment, where Peter Diamond, Dale Mortensen, and Christopher Pissarides have made especially important contributions' (Kungl. Vetenskapsakademien 2006: 8). As a matter of fact—I thank Chris for this insight—the class of models in the so-called Phelps volume was criticised. To quote Chris from his Nobel Lecture (Pissarides 2011: 1092): 'The articles in the Phelps volume, however, especially those by Phelps (1970) and Dale Mortensen (1970) which had explicit models of the Phillips curve, required a wage distribution to obtain the microfoundations of the Phillips curve. As Peter Diamond (1971) and Michael Rothschild (1973) pointed out, this was not consistent with the other assumptions of the models'.

⁵Pissarides (1997).

saying what everybody in the room thought: ‘But this looks very much like a matching function!’, leading the entire room to burst out laughing. That ability to build simple and elegant models would be a constant concern and would apply to all topics examined by Chris. (See, in particular, the more recent contributions to the economic theory of growth and sectoral reallocation of labour (e.g. Ngai and Pissarides 2007), described later in this chapter.) The virtue of such an approach is that it helps to derive measurable concepts and empirically testable predictions that Chris and his co-authors would generally take seriously (see, for example, his empirical works with Petrongolo on matching functions or on economic growth with Giovanna Vallanti (Pissarides and Vallanti 2007)). Chris’s concern for empirical observation can be found in his early contribution to the modelling of the British economy (Pissarides 1972), but more importantly the description of the Beveridge curve in Pissarides (1986) and the discussion of the inter-regional mobility of labour in Pissarides and Wadsworth (1989).

3 From Microeconomic Foundations to the ‘Standard’ Macroeconomic Model

3.1 Towards Macroeconomics: From the Probabilistic View to the Existence of a Matching Function

At the most microeconomic level, there are three interconnected dimensions of frictions: (i) lack of information about trading partners; (ii) economic costs of transactions (both direct and opportunity costs); and (iii) technological constraints limiting the spread of information and the speed at which contacts arise between agents with a desire to trade. These dimensions are obviously linked: the less information, the costlier it is to acquire it and the slower the contacts arise, *ceteris paribus*. Competitive markets are particular cases in which agents do not bear any direct cost and find trading partners at an infinitely fast speed.

Here is a simple way to grasp the idea of frictions. Imagine two actors in each segment of the market: one is attempting to sell a particular good or service (e.g. labour) while the other is attempting to purchase this good or service (hire the worker). These two actors meet randomly. Nature decides in a probabilistic way whether or not the contact will eventually happen. In case it does, the exchange may take place if the agents are able to agree on a price for the exchange (the wage), which is discussed below.

Assume that at a given point in time t there is a stock U of unemployed workers. In continuous time (the specification chosen by Pissarides (1985), Mortensen (1982a, b), and Diamond (1982a) in their seminal contributions), consider the probability of a meeting during an arbitrarily small time interval between t and $t + dt$. Denote this by $p \cdot dt$ where p is the instantaneous probability (per unit of time).

The number of contacts from the unemployed pool will thus be $U \cdot p \cdot dt$. Here, p can be any real positive number, including anything greater than 1. The probability $p \cdot dt$ will be less than 1 for an arbitrarily small time interval, while p will be interpreted as a parameter from Poisson's Law.

On the other side of the market, if V is the number of vacancies, the total number of contacts during the interval of length dt will be $V \cdot q \cdot dt$, where q is another positive number that can be greater than 1. Similarly, q is interpreted as a parameter from Poisson's Law.

The number of contacts from the pool of vacancies needs to be equal to the number of workers from the pool of unemployment, and we thus have the fundamental identity:

$$p \cdot U = q \cdot V \quad (1)$$

or,

$$p = q \cdot \theta \quad (2)$$

where $\theta = V/U$ represents the ratio of job offers and workers looking for a job and thus is a measure of labour market tightness. For large values of θ , the market is said to be tight from the firms' perspective, and if θ is low, the market is said to be loose from the firms' perspective and hence tight from the point of view of unemployed workers. More recently, Hall and Schulhofer-Wohl (2015) argued that the main contribution of the DMP model is to summarise (rightly) all labour market fluctuations with this single statistic, θ .

Here, the important point to take away is that identity (2) implies that p and q cannot be exogenous simultaneously, that is independent from the endogenous quantities U and V . For example, if p is exogenous, then q varies as the inverse of θ . If, on the contrary, q is exogenous, then p is proportional to θ . In all cases, the probability of at least one of both sides of the market (firms or unemployed workers) will depend on the conditions of the market, θ .

A general way of expressing the dependence of p and q on the market conditions (summarised by θ) is to assume that:

$$q = q(\theta) = A \cdot \theta^{-\eta} \quad (3)$$

where η is a parameter of a value between 0 and 1 inclusive, and A is a scale parameter capturing the efficiency of the matching process. When η is equal to 0, q is exogenous. When it is equal to 1, p is exogenous. Finally, when η is itself independent of U and V , the total number of vacancies filled per unit of time is a Cobb–Douglas function:

$$p.U = q.V = A.V^{1-\eta}.U^\eta \quad (4)$$

The idea of a matching function between two segments of a market thus appears quite naturally. More generally, the matching function between two segments of size U and V (denoted by $M(U, V)$) will be assumed to be increasing and concave in each argument, with constant returns to scale, such that $M(0, V) = M(U, 0) = 0$. Furthermore, the returns of this matching function will be infinite in 0:

$$\begin{aligned} M'_U(U, V) &\rightarrow +\infty \text{ when } U \rightarrow 0 \\ \text{and } M'_V(U, V) &\rightarrow +\infty \text{ when } V \rightarrow 0 \end{aligned} \quad (5)$$

The case of competitive markets is obtained when the scale parameter A tends towards infinity.

Starting from identity (1) to build the matching function is generally not done in literature, as it is usually taken as given. Lagos (2000) is a rare example of where the analysis begins with identity (1). The ability of the matching function to generalise perfect labour markets to include frictions of varying intensity in a simple way has made it a very flexible tool. As Pissarides (2000: 33–35) noted, the first contributions based on a function linking job offers, unemployment and hires go back to Phelps (1968), then Hall (1979), Pissarides (1979), and Bowden (1980).

3.2 The Asset Value of a Job and a New Open Position (Vacancy)

Another key contribution made by Chris has been to find an elegant formalisation of the concept that a job is an asset for the firm, with a value denoted by J . It is governed by a well-defined recursive equation (of the Hamilton–Jacobi–Bellman type) where the flow value of a job rJ , where r is the discount rate, is equal to the sum of the net income generated by the worker (labour marginal productivity y minus wage w), the capital loss $-s(J - J_V)$ arising from a separation shock of Poisson intensity s where J_V is the value of the firm without its worker, and, out of the steady state, of the capital gain

from time dJ/dt . Similarly, the asset value of a vacant position is equal to J_V and is the sum of the negative of hiring cost $-c$, the capital gain from hiring a worker $q(\theta)(J - J_V)$ and out of the steady state, the capital gain of time for the value of a vacancy dJ_V/dt .

An interesting innovation (Pissarides 1979, 1985) was to introduce a free-entry equation for firms, a closing condition affecting, not the value of a filled position, but, consistent with the idea of search frictions, the value of a vacant position. At any time, there would be enough new vacancy entries (or vacancy exits if in excess quantity) to balance the expected search entry costs and expected profits.

In the most general way, the value of a vacancy would be a convex combination (weighted by $q/(r+s+q)$) of perpetual profits $(y-w)/r$ and perpetual recruitment costs $-c/r$ with the complementary weight.

When labour market tightness θ is near 0, firms find it infinitely easy to recruit and thus the weight above is 1 and the value of a vacant job offer would equal the value of perpetual profits. If instead, θ is very large, $q(\theta)$ tends towards zero and thus the weight also tends towards zero. In this case, the value of a vacant job offer is the value of perpetual losses suffered by the firm when it fails to hire workers. This value is thus negative. Therefore, by continuity, there exists a single value (for a given wage below productivity) that makes the entry value of a job J_V equal to zero. This entry equation makes it easy to characterise the determinant of equilibrium labour market tightness as a function of the interest rate, labour turnover rate, costs of hiring frictions, labour productivity and, of course, wages.

3.3 Free Entry Equilibrium with Exogenous Wages

Going back to identity (1), one can finally connect equilibrium labour market tightness to unemployment. In a stationary state, the number of workers entering the unemployed pool is $(1-u)s$ (the product of the number of jobs, the workforce being normalised to 1 and the rate of job destruction). The number of exits from the unemployed pool is $\theta q(\theta)U$.

Setting these two quantities equal to each other, we obtain the familiar equation:

$$U = s/(s + \theta q(\theta)) \quad (6)$$

which is decreasing in θ . The equation relates the number of vacancies to the number of unemployed workers, i.e. Beveridge curve.

There is a nice discussion of Pissarides (1986) by Richard Layard (1986). Layard argued that from the observation of co-movement in prices (or inflation) and unemployment, the Phillips curve, one could recover the source of economic fluctuations: a demand shock would drive prices and unemployment in opposite directions while a supply shock would lead to the opposite co-movement. Chris's work was doing no less than doubling the number of insights.⁶ Observing unemployment and vacancies to increase simultaneously in the same direction would be revealing a labour reallocation shock, while if they co-moved in opposite directions, this would imply a shock to firms' entry decisions, e.g. either a demand or supply shock. This idea would be successfully applied to the data a few years later (see, for example, Blanchard and Diamond (1992) reviewed below), and it would be concluded that, indeed, labour reallocation and matching shocks play an important role in unemployment fluctuations.

3.4 Determination of the Terms of Exchange: Wage Bargaining

Another key to the success of the matching literature was the derivation of a wage equation again expressed, thanks to the beauty of Nash bargaining, as a weighted average of the worker's labour productivity and the reservation wage of the worker, the weight characterising the relative bargaining power of the worker and the firm. The underlying assumption, and the one most commonly made in literature, was that the worker's compensation would maximise the Nash product (see Mortensen 1982a):

$$w = \text{ArgMax}(J - J_V)^{1-\beta}(W - U)^\beta \quad (7)$$

where β is a parameter between 0 and 1, and $(W - U)$ is the surplus of the worker measured as the difference between the expected discounted value of employment and the expected discounted value of unemployment.

β is interpreted as the ratio of the implicit discount rates of the two agents in a negotiation that would happen instantly in a Rubinstein game of offer and counteroffer (see, for example, Osborne and Rubinstein 1990; Binmore

⁶To resolve these questions, we only have two important further pieces of information—the behaviour of inflation and the behaviour of vacancies. Thus, roughly speaking, we increase our knowledge by almost 50% when we bring in vacancies' (Layard 1986: 541).

et al. 1986). The more patient the worker is relative to the firm, the more likely he is able to obtain a surplus. Again, this specification demonstrated its flexibility by reflecting very different polar cases, such as the one in which the worker is paid his reservation wage (when β is equal to 0), and the case in which the worker gets all the surplus, when β is equal to 1.

3.5 Main Additions

From the most microeconomic issues to the most macroeconomic ones (or from the short-term dynamics to the long-term aspects of the theory), the matching model is easily extended to: investment and physical capital; labour market participation and endogenous job search; education and human capital; technology adoption; and balanced growth over the long term. Other topics such as endogenous job destruction, welfare, tax and policy, and dynamics and business cycles are treated in the next section.

On the first point, the extension to investment and physical capital, seeing the value of a job as an asset makes it straightforward to add a theory of the firm's capital and investment behaviour. The asset value of a firm is the optimal value when the firm's owner simultaneously chooses a number of vacancies and the amount of investment as control variables. The resulting level of unemployment and capital stocks are naturally the state variables, and the dynamic constraints are the matching technology features of the evolution of employment as a balance between hires and job separation and the evolution of capital as a balance between investment and depreciation. This perfect symmetry between labour and capital is presented in Chapter 3 of the second edition of *Equilibrium Unemployment Theory*, published in 2000. It shows the equivalence of the small-firm model (one worker) and the large-firm model. The equivalence holds under constant returns to scale in the production function, an assumption made in this book as well as in all of Chris's articles. It would subsequently be shown that the equivalence property holds under constant returns to scale even if the firm sets wages strategically, as in Stole and Zwiebel (1996a, b).

Another control variable that is easy to introduce is the choice of effort to increase the efficiency of search. This can be done by both workers (job search effort) and firms (investment in advertising), and this has been studied in particular in Pissarides (1984a) and discussed in greater detail in *Equilibrium Unemployment Theory* (Pissarides 2000: Chapter 5).

Participation in the labour market thus arises naturally as the interior solution of search effort, while non-participation is the corner solution of it, a discussion that was developed in a series of insightful papers in a partial equilibrium set-up in the 1970s, one by Chris with a focus on the worker's level of discouragement (see Pissarides 1976) as well as two papers by Mortensen (1977) and Burdett and Mortensen (1978). These seminal works would considerably deepen our understanding of labour market participation in a context of unemployment and would, in particular, define the important empirical concept of attachment to the labour market that would be key in properly measuring unemployment, as some marginally attached workers might not be counted as unemployed workers.

Similarly, human capital can be easily incorporated. An individual's productivity can decrease as a result of job displacement as well as long periods of unemployment (see Pissarides 1992), this being consistent with the view that skills and technology may become obsolete (see Mortensen and Pissarides 1998, discussed later on). The 1992 article was written during a sabbatical at UC Berkeley and was certainly inspired by the discussion on hysteresis in unemployment that had been invoked to explain the persistence of European unemployment. This idea would later attract greater interest and would be extended within an influential quantitative framework by Ljungqvist and Sargent (1998).

Technology adoption was an important topic in the 1990s, especially after the revival of Schumpeterian growth theory by Aghion and Howitt (1997). Mortensen and Pissarides, who had first extended the matching model to endogenous job destruction, also developed the model with technology growth and embodied and disembodied technology and endogenous obsolescence (see Mortensen and Pissarides 1998). Incidentally, it is striking to realise that Chris's collaboration with Dale has been so profoundly original and deep, while at the same time very limited in the number of formal academic articles: there are a few book chapters and two handbook surveys, but in essence there are only two main models, the 1998 one on technological obsolescence and the 1994 *Review of Economic Studies* paper on endogenous job destruction.

Finally, the model was made to accommodate the long-term growth of real variables, both of technology and population with the aim of identifying a long-term growth path where ratios would remain constant (see the discussion on this issue in Section 6 below), and nominal variables (see the discussion on money growth and inflation in Pissarides 1990).

4 Quantitative Analysis and the Role of Frictions

4.1 The Matching Function Block

Numerous works have tried to estimate matching functions directly. This research has been summarised by Petrongolo and Pissarides (2001). Blanchard and Diamond (1989) started from a Cobb–Douglas specification and obtained a hiring elasticity with regard to vacant positions of between 0.53 and 0.62, and a positive and significant elasticity of hires with respect to the number of unemployed workers of lower than 0.5, specifically between 0.3 and 0.45. Total returns to scale were around 1, or between 0.9 and 1, in most specifications. In their literature review, Petrongolo and Pissarides (2001) underline that many studies obtain results of constant returns to scale but indicate that the elasticity of hires with respect to the number of unemployed workers is generally higher at between 0.5 and 0.7. This suggests that unemployment is an important factor in the process of job creation. There is thus an interesting contrast here with traditional models of disequilibrium and the model of Keynesian rationing: both suggested that the number of hires would instead be limited by the number of vacant positions and demand. They would therefore predict a low elasticity of hires with respect to the number of unemployed workers, or even a negative link between hiring and unemployment if the unemployed do not consume and therefore contribute to a reduction in the aggregate demand for goods. One could argue that these empirical works were as much a confirmation of aggregate matching and the random search approach as a partial invalidation of approaches based on strong aggregate demand mechanisms leading to persistent underemployment.

4.2 Cyclical Implications of the Model

4.2.1 Qualitative Implications of the Model on Hiring and Vacancies

By extending his analysis from the standard model to the dynamic case, Pissarides (1985) had shown that his model predicted a regularity observed in the data. There are indeed, from the model, movements out of the stationary state in counterclockwise loops around the Beveridge curve in a

$U - V$ space when productivity goes up and down. This is in fact what is observed in time series in many countries.

Blanchard and Diamond (1989, 1990, 1991, 1992) have pushed Layard's intuition (outlined in his 1986 discussion of Chris's work Layard 1986): search theory and the Beveridge curve have a strong informational content on the underlying sources of economic fluctuations. Blanchard and Diamond decomposed the movements of vacant job offers and the number of unemployed workers in movements of aggregate productivity and movements linked to the matching process. This can be done using different methodologies. One of these consists in using time series to estimate a matching function with a term that can capture a deterministic drift of scale parameter A of Eqs. (3) and (4) above. Blanchard and Diamond (1989: Table 1) thus find a negative drift during the 1968–1989 period. This shows the Beveridge curve's progressive movement away from the origin. Another method is based on the strategy of decomposition of structural shocks of Blanchard and Quah (1989), adapted to the analysis of the evolution of unemployment, the number of available vacancies and the active population. The three shocks are distinct (sectoral, aggregate and reallocation). Identification is based on the assumption that an aggregate shock created a negative co-movement of job offers and unemployment during a period of nine months. Their analysis suggests that aggregate shocks dominate, but sectoral shocks also play a significant role. In the short term (less than nine months), aggregate shocks represent more than 50% of the variance of unemployment. In the long term, aggregate shocks represent a lower 25% to 30% of the variance and play only a minor role in explaining fluctuations in the frequency of real cycles. These results lead to the perception that matching difficulties have two notable characteristics. First, their deviations from equilibrium are reabsorbed rapidly during the convergence towards the stationary state, which allows us to concentrate on the stationary model for the analysis of the real cycle. Second, their stationary values are very meaningful in the long term, when we look at secular movements of the labour market or at the difference between the natural rates of unemployment across countries.

4.2.2 Qualitative Implications of the Model on Lay-Offs and Job Destruction

When the first empirical analyses on firm databases by Davis and Haltiwanger (1990, 1992) were published, it became apparent that the standard matching model with exogenous job destruction did not take

into account all phenomena. Davis and Haltiwanger showed two important facts. First, there is a great heterogeneity between firms, even at infra-sectoral levels. While some firms can experience a growth in employment, other firms in the same sector can simultaneously experience a decline in employment. But in the standard model, the homogenous behaviour of firms was assumed. The second thing that David and Haltiwanger showed was that job destruction played an important role in macroeconomics, and that this was very volatile in the short run, more so than job creation.

The standard model then had to evolve to take into account these two findings. The collaboration between Mortensen and Pissarides led to a great improvement of the standard model. A new endogenous variable was added to the model, the rate of job destruction, previously captured through other variables. The specification presented in Mortensen and Pissarides (1994) allowed this variable to be endogenous and made the model reflect cyclical movements and firm heterogeneity. Firms are subject to idiosyncratic productivity shocks, which leads to fluctuations in the value of jobs for firms and makes firms heterogeneous. In the cross section of firms, there are naturally ones of different productivity levels. When the value of idiosyncratic productivity falls below a certain threshold, it is optimal for the firm (and for the worker, because of the surplus sharing rules) to cancel the match and terminate the relationship. The destruction decision thus becomes endogenous, and the job destruction rate becomes a function of the frequency of productivity shocks and of the probability of falling below the threshold. Furthermore, the important volatility of job destruction in the cycle that David and Haltiwanger's work showed finds an explanation in this extension: like its number of vacant positions, the job separation threshold for firms is a variable that reacts instantly to anticipations of any type (aggregate profits, technology shocks and wage shocks), which can thus vary discretely from one instant to another.

4.2.3 Model-Generated Macroeconomic Volatility

In the 1990s, young scholars (actually, even PhD students at the time; see, for example, Danthine and De Vroey 2017) would attempt to import the main intuitions of Chris's model into quantitative macroeconomic models. Merz (1995) and Andolfatto (1996) would provide the benchmark dynamic general equilibrium models of search unemployment. The inclusion of job destruction in the model would lead to interesting methodological developments on the propagation of shocks, such as the approach developed by den

Haan et al. (2000). Eran Yashiv would pursue calibration exercises in several contributions alone or with Monika Merz (see, for example, Yashiv 2000; Merz and Yashiv 2007), pushing the logic of the tight connection between labour markets and finance, where the value of the firm as an asset plays a central role.

These extensions of search theory to integrate it into quantitative macroeconomic theory led to various puzzles and empirical challenges. For instance, Cole and Rogerson (1999) analysed data patterns and the cyclical behaviour of job creation and destruction in detail. They would show that the model did not produce enough volatility, and that the only way to fix this would be to redefine the concept of search unemployment: if the model was calibrated for an unemployment rate of 14% instead of 7% or 8%, then, they argued, the model would be able to reproduce cycles accurately. This recalibration had an interesting interpretation provided by the authors: the unemployment rate must take into account workers officially looking for a job but also those who have stopped searching; they are not in the labour force statistics and thus are 'administratively inactive', but they actually might take up a job if the opportunity arose.

A second and more influential area of criticism of the DMP model comes from Shimer (2005). In a dynamic version in discrete time, he established that starting from 'observed' technological shocks, the model could only generate a log standard deviation of about 3.5% for labour market tightness θ , a value which is approximatively 10 times lower than what was measured in US labour market statistics. Can the model replicate a log standard deviation of labour market tightness of 35–38% with only productivity shocks? A partial negative answer is that not all shocks are productivity shocks: the empirical correlation between unemployment and productivity shocks is only around 0.4. Therefore, the DMP model should only match volatility inferior to this by θ , about $0.4 \times 38\%$, or 15% (see Mortensen and Nagypal 2007). Pursuing this logic, Pissarides (2009: 1351, fn. 15) even suggested an elasticity of labour market tightness to productivity shocks of about 7.56.

Others have put forward the argument that a radically new parametrisation of the model would match the volatility in labour market tightness. Assuming for instance that the value of leisure would be close to the wage (the small surplus assumption), one can indeed observe a substantial improvement in the degree of matched volatility by the model. Hagedorn and Manovskii (2008) argue in particular that if the value of non-employment (leisure and unemployment benefits) is close to the wage (between 3% and 5%), then the model generates sufficient volatility to match the data. Costain and Reiter (2008), who had been among the

pioneers in their calibration efforts, have however criticised this calibration as it makes the model sensitive to the variation of other parameters of public policy, in particular unemployment benefits that are counted in the value of leisure. Nevertheless, it is interesting to note that the idea of increasing the value of non-employment in the calibration raises the equilibrium unemployment rate in the model, which is, incidentally, similar to the strategy of improvement of the calibration chosen by Cole and Rogerson (1999) noted above.

Shimer (2005) and Hall (2005) would favour rigid wages as a solution to the puzzle, but what matters for job creation is new wages, which turn out to be quite cyclical in the data, more so than the wages of incumbent workers. Indeed, the rigid wages of incumbents do not affect volatility since new jobs are unaffected by these wages. Instead, the highly volatile wages of new entrants as observed in the data would limit volatility as they reduce profits in good times and increase it in bad times.

Hence, the main building block of the matching model (endogenous wages depending on current productivity) would again resist the suggested transformation into rigid wages. Pissarides (2009) took a different road by arguing that the cyclical nature of labour market tightness is insufficient because recruitment costs are too procyclical: their value, equal to $c/q(\theta)$, increases with labour market tightness, which slows down the incentive to create jobs in a period of economic expansion. Pissarides therefore recommends a reparametrisation of the model in which a fixed part of H , creation costs, adds itself to the procyclical parts, in order to obtain a total cost of job creation C that is less cyclical in elasticity, as follows:

$$C = H + c/q(\theta) \tag{8}$$

The model then generates the value of 7.56 proposed by Pissarides. Recently, Ljungqvist and Sargent (2017) have developed a thoughtful generalisation of the concept of surplus (what they call the ‘fundamental surplus’) which includes most of the new ingredients in the literature, including the cost of financial frictions affecting new firms. The cost of financial frictions indeed adds more volatility, and its effect can be interpreted as a financial multiplier, amplifying the consequences of cyclical shocks.

A fair summary from two decades of quantitative research between 1990 and 2010 is that the DMP model, which was not initially built to be calibrated (unlike the real business-cycle model), could however reproduce second moments in conformity with US data, that is if a few parametrisation changes are made. The exact nature of the necessary changes is still a pending question, and this is discussed in the Conclusion below.

5 Economic Policy Implications of Frictions

Beyond the key contributions to macroeconomics and labour market theory, Chris has also contributed to many policy debates. His intellectual influence in Europe has been notable. He was a member of the Employment Taskforce chaired by Wim Kok, which delivered an important report on the Lisbon Strategy. Chris has worked extensively on the European unemployment and macroeconomic experiences. He has been very active in European academic institutions and journals: Chris was Vice-President (2009) and President (2011) of the European Economic Association and published many times in the *European Economic Review* for instance, most often on European labour market institutions and European macroeconomic performance.⁷

These policy analyses were deeply anchored in search theory: the framework is essentially a second-best theory. In this framework, many policy instruments can be analysed and, in general, do not have an unambiguous effect. The model leads to rich and non-trivial implications and balanced analyses that policy makers in Europe have found more attractive than laissez-faire strategies. This does not mean demagoguery: Chris has always defended the view that unemployment compensation had to be active and not passive, that training and skills investment was key, that the job search effort was a key factor, and that unemployment could not be seen as purely involuntary.

5.1 Benchmark for Efficiency

The standard matching model does indeed allow a large role for economic policy and government intervention, contrary to models based on the abstraction of efficient markets. In these models, disequilibrium (unemployment and job rationing) would be the result of inefficiencies such as entry barriers or rigid prices. Instead, since matching models start from a situation in which frictions play a structural role in markets and cannot be eliminated, only reduced, the policy implications are quite different.

The model naturally leads to the concept of a constrained optimum, a second-best efficient situation. The social planner tries to maximise the net output of production costs (production efforts, investments and matching

⁷As an illustration, the word 'Europe' appears 36 times in his resume (covering panels, associations, policy forums, article titles and discussions), whereas America, United States and USA appear only 12 times, mostly for journals and more rarely with respect to policy discussions.

efforts), under the constraint that the number of matches must be lower than or equal to what the matching technology permits. The constraint will be binding at the social optimum: more efficient matches always lead to a better allocation of resources, which helps move closer to the first-best optimum.

The first-best optimum is therefore a useful benchmark, even though it cannot be achieved. One can only partly relax the matching constraint by improving efficiency; for instance, we can create platforms of information for unemployed workers, such as employment agencies (see Pissarides 1979), targeted training sessions for unemployed workers, and more generally, we should allocate public funding to improve matching in the labour market (such as counselling) rather than only passively spending to compensate the loss of revenues linked to unemployment. It is therefore possible for a policy to affect the search effort by unemployed workers to find new jobs; search effort is an input in production. The same goes for the firms' recruitment efforts and, more generally, the efforts of any economic agent to engage in exchanges with other agents. These efforts all contribute to achieve collective efficiency and must be encouraged. More generally, the spirit of Chris's research is that unemployment itself is a resource: unemployed workers are not assumed to be 'lazy' people, but temporary and necessary factors in the aggregate production process.

Further, the second-best optimum is only achieved when the externalities generated during the matching process are internalised by an adequate transfer system. Indeed, the matching function, $M(U, V)$, contains no less than four externalities. The more unemployed workers there are, the easier it is for firms to recruit (positive externality), but the harder it is for unemployed workers to find a job (negative externality). Conversely, the more vacancies there are, the easier it is for workers to be re-employed (positive externality) and the harder it is for firms to recruit (negative externality).

For a model with exogenous job separation, job creation is the only dimension that the social planner can affect. If the social planner creates too few positions, unemployed workers will remain unemployed for too long and production will be too low. If the social planner creates too many jobs, competition between vacancies will be too high and firms will pay excessive recruitment costs. Production will be high, but inputs (recruitment efforts) will also be too high. The second-best optimum is only reached when these two contradictory forces cancel each other out. In practice, a decentralised equilibrium reaches an optimum equilibrium when agents are paid what they contribute to the matching process.

Hosios (1990) and Pissarides (1990) showed that an elegant second-rank optimality condition was when the share of workers in the surplus was equal to the elasticity of unemployed workers in the matching process:

$$\beta = \eta(\theta) \quad (9)$$

For the record and for the history of economic thought, it is interesting to restore some facts. Mortensen and Pissarides (2011) reported that, ‘Diamond (1982b) had already hinted that there might be an internalized Nash wage rule and Pissarides (1984b) derived explicitly the share of labor in the wage bargain that internalizes the externalities, what became later known as the “Hosios rule” (Hosios 1990)’. This is actually an understatement. The Pissarides (1984b) paper cited in Mortensen and Pissarides (2011) derived much of the Hosios rule but stopped one step short of its final formulation. Indeed, Pissarides (1984b) showed in its Eq. 17, page 105, that the efficient bargaining share β needed to be equal to $(M_U + M_V - M/V)/(M/U - M/V)$, where M_U and M_V are the marginal contribution of each input. Although this equation was not simplified, note that under constant returns to scale, M_U is equal to $\eta(\theta)M/U$ and M_V is equal to $(1 - \eta(\theta))M$. With one line of algebra, the simplest Hosios condition follows. This result was already obtained in 1984 and just needed to be simplified.⁸ The interpretation of the condition is as follows: when the unemployed workers contribute significantly to the creation of matches, which is the case when η is large and closer to 1, firms have to be ‘taxed’ by giving a larger wage to workers, and vice versa.

The normative implications of matching models are that the optimal rate of unemployment can be positive as opposed to zero in competitive models, because unemployment is an input in production. This leads to richer implications: there is no ‘obvious’ or a priori policy measure. In some cases, the optimal policy can be the opposite of what would be advocated in a pure competitive model; however, in most cases, models would have similar normative implications. Finally, Moen (1997) elegantly showed that the possibility of endogenous segmentation of labour markets would lead to second-best efficiency as a rather generic situation, an interpretation and a conclusion that, I think, Chris never completely accepted: in most writing and discussions, including Pissarides (1984a), the focus is rather on the low likelihood of second-best efficiency, and therefore the need for policy intervention.

⁸Chris was apparently aware of the simplification and had revised the paper accordingly, but the 1984 publication (Pissarides 1984b) was a conference volume, and the organisers had already sent the submitted papers to the publisher.

For completeness, one should also remark that in this last paper, Chris emphasises a negative efficiency result: no wage can internalise the different externalities, in apparent contradiction with Pissarides (1984b), but interpreted, in Pissarides (2000), as the fact that Pissarides (1984a), as with other important papers in the search literature, ignored the negative externality of search effort on other workers, leading in that specific case to the result that the decentralised effort would be inefficiently low.

5.2 Policy Instruments

There are numerous policy instruments that can be used in the labour market. The matching model has precise predictions for four of them: unemployment benefits, progressive income tax, hiring subsidies and firing costs. Here, we consider their impact on social welfare, defined as the sum of the utility of employed workers, unemployed workers and firms weighted by their numbers, or equivalently, the total net production of transaction costs and leisure.

Pissarides (1998) discussed the role of income taxes and in particular income tax progressivity in the search and matching model as well as in alternative models of the labour market. He showed that the progressivity of income tax would reduce equilibrium wages: progressivity reduces the marginal gain from higher wages for workers and reduces the total surplus as wages grow. Hence, bargaining parties converge to lower wages, and this raises employment.

Unemployment compensation is also a key determinant of search models. Unemployment benefits usually play a negative role in terms of social welfare. This arises from the fact that only passive compensation reduces the surplus of workers and thus of firms and also reduces incentives for workers to search and for firms to create vacancies. In Pissarides (1983), Chris would however take a more moderate position: the adverse effects of unemployment insurance raise the reservation wage, but this can be alleviated with progressive income taxation, an insight that he would reuse in the 1998 paper mentioned above. In addition, risk aversion and workers' inability to insure themselves are absent from the benchmark analysis, and so its potential positive impact (insurance) is absent while it is central in the theory of optimal unemployment insurance with imperfect insurance.

Hiring and employment subsidies also play an important role in equilibrium unemployment theory. Employment subsidies can accommodate a too large share of wages in bargaining, but Chris would rather advocate structural

changes in labour market institutions that would reduce bargaining. Hiring subsidies, by alleviating the cost of frictions, may be an interesting alternative and be less costly as it affects a smaller base (new hires). An application to policy in France (Cahuc et al., forthcoming) suggests that this may be an efficient policy.

Regarding firing costs, there is once again a tension between competitive models, which see them as an obstacle to the reallocation of labour (e.g. Hopenhayn and Rogerson 1993), and matching models. Mortensen and Pissarides (1999c) followed this view of employment protection as a tax reducing the cross-sectional efficiency of allocation of workers to firms. There is likely to be a Danish influence here: the policy implication, the flexi-security model, would lead to a reduction in employment protection and combine it with generous and active unemployment compensation. However, in positive terms, in the Mortensen and Pissarides (ibid.) model, the net effect of employment protection on unemployment is ambiguous and might therefore be justified. Further, in a world of imperfect financial markets, Pissarides (2001, 2010) developed variants of the model in which we can analyse the role of employment protection in a detailed manner when agents are risk averse and also constrained by the credit market. He notably showed how to combine severance payments and notice periods, the second element being unnecessary if the first element is at its optimal level.

A last dimension that is interesting to consider is the minimum wage. A positive impact of the minimum wage on employment can only arise when firms have some monopsony power over workers, a dimension that is naturally present in the DMP model thanks to search frictions. Although the benchmark DMP model has no skill heterogeneity, incorporating ex ante heterogeneity in these skills leads to rich insights into the analysis of the minimum wage under monopsony. An entire book has been written by Christopher Flinn (2010) that discusses, with and without heterogeneity, the way the minimum wage interacts with search frictions.

6 Labour Market Reallocation Across Sectors and IT

Another research area in which Chris has contributed is the impact of structural change and the reallocation of labour across sectors. The long-term growth determinants of countries are usually associated with total factor productivity (TFP), innovation, political institutions, capital investment and

education; however, when productivity growth affects some sectors in particular, the issue of labour reallocation and relative sectoral growth becomes an important one.

Chris developed this topic in a series of contributions with Rachel Ngai when she arrived at LSE. In their widely cited 2007 paper (Ngai and Pissarides 2007), they developed a model of sectoral TFP growth in which each sector produces a specific good consumed by a representative consumer, produced with labour and a unique type of capital good. An important result is that differential productivity growth does not necessarily lead to persistent changes in the various ratios (capital to labour, capital to output). Thus, a balanced growth path, where consumption, capital and output grow at the same pace, is possible despite continuous reallocation across sectors. This is an important result, very elegantly demonstrated and consistent with Chris's continuing effort to keep balanced growth paths in his models (see, for example, the discussion in Chapter 3 of Pissarides 2000). If the 2007 paper with Ngai dealt the demand side of sectoral change, the two of them would soon focus on the supply side and the allocation of hours across sectors in response, in particular, to taxation: contrary to the (quite naive) *laissez-faire* view that too high taxes explained too low hours, they would show that taxes would only drive households to work at home to produce the goods most substitute to those produced by the market (such as French and Italian cooking and German babysitting traditions—my words).

If taxes can help to explain the sectoral levels of unemployment and their cross-country differences, in Chris's view, long-run labour reallocation and employment changes are closely linked to technological change. The way to model this has been very influential. Beyond differential sectoral trends in technical progress, Mortensen and Pissarides (1998) discuss the distinction between 'embodied' and 'disembodied' technical progress. Disembodied technical progress affects all jobs in the economy, such as the impact of new information technologies and communications technology. The steady state of an economy without technical progress is therefore simple to generalise: if disembodied technical progress grows at a constant rate g , then in the model it is enough to replace the interest rate r by the interest rate net of growth, $r-g$, in most of the equations. This is sometimes referred as the 'capitalization effect', which Pissarides (2000) attributed to Aghion and Howitt (1994) as the positive employment effect of growth.

Instead, when technological progress only affects new jobs, technical progress is said to be embodied in new jobs. This arises because technical progress leads to the appearance of new sectors and jobs, such as iPhone apps leading to developer jobs. This may also be due to the fact that

capital investments are, to a large extent, irreversible, leading to high costs of updating existing jobs to the new technology. In this instance, a vintage model featuring a trade-off between letting obsolete jobs disappear or investing to update them is necessary. This was analysed by Mortensen and Pissarides in 1998 and subsequently used in the literature to connect labour issues to the work by Aghion and Howitt (1994, 1997) on Schumpeterian growth and innovation. (See, for instance, Pissarides and Vallanti (2007) for an empirical investigation of the relation between TFP and unemployment.)

7 Aggregate Demand Effects

The first edition of *Equilibrium Unemployment Theory* starts with a very clear statement:

Keynes's famous statement that the unemployment of workers between jobs can be ignored in the study of more important kinds of unemployment is unverified conjecture. Descriptively, it is false: with the exception of a few "discouraged" workers, unemployed workers are always between jobs, or between some other states and a job (Pissarides 1990: x).

One of Chris's key but overlooked contributions has been to develop unemployment models where demand effects are not the driving force and, further, unemployment exists even in the presence of price and wage adjustment. This was no coincidence. In many macroeconomic discussions, Chris has emphasised his preference for approaches where Keynesian effects would not be prominent. For instance, in his review of Jeff Frank's book, *The New Keynesian Economics: Unemployment, Search and Contracting* (Pissarides 1987), he starts by stating that the first 135 pages of the volume, devoted to IS-LM economics, might not be the most fruitful approach, arguing instead that approaches where search frictions play a central role are best suited to the analysis of unemployment. Chris also points out that the classic distinction between voluntary and involuntary unemployment is not useful, a point he would make quite early on in the Preface to *Equilibrium Unemployment Theory* (see Pissarides 1990: x). One can find this view expressed earlier on, including again in Pissarides (1987: 511), where Chris wrote: 'The distinction between voluntary and involuntary unemployment is as common in macroeconomics as it is unhelpful'. This is another battle that Chris has won: nobody would still argue that unemployment is either voluntary or involuntary. It must therefore be either a little bit of both and

thus indeed be irrelevant, or depend on the regime (classical, Keynesian or inflationary) in which the economy found itself at the time.

Indeed, the view that economies were sometimes stuck in a Keynesian underemployment regime was an influential one in the 1980s. The European economics literature on this subject had been deeply influenced by the works and synthesis by Jean-Pascal Bénassy (1982), Jacques Drèze (1991) and Edmond Malinvaud (1977) as well as by Robert Barro and Herschel Grossman (1971) on Keynesian unemployment and Keynesian regimes. The commonly held view was that, given price and wage rigidities, the economy could be in different regimes and that policy prescriptions would depend on the regime. The literature then progressively shifted to models of a unique regime, yet with rich policy implications: the model of price setting-wage setting in Layard et al. (1991 [2005]) and Blanchard (1986) would be examples of this approach. In essence, this predicts that facilitating price and wage adjustment improves output and employment, but that complicated dynamics may arise and aggregate disequilibrium and inflationary pressures may be quite persistent.

The search and matching equilibrium approach sought to define a unique equilibrium and then find factors which result in persistence. Yet since unemployment is the consequence of the technological constraint featured by the matching function, price rigidity only plays a secondary role in determining the *first moment* of the model, the level of unemployment. As discussed above, it would be recognised that price rigidity also plays an important role regarding the value of the *second moment*, leading to new insights about the role of wage determination (see Hall 2005; Shimer 2005). But this addition would not restore the faith in Keynesian effects and would remain confined to the traditional issue of volatility in macroeconomics.

The search and matching literature, based on its apparently innocuous assumption of a frictionless goods market, has been successful, possibly too successful, in eliminating Keynes from the debate. The search and matching model was perceived as sufficiently rich to address the question of unemployment almost independently of the traditional Keynesian effects. Numerous parameters or policies—bargaining strength of workers, the value of unemployment benefits, interest rate policy, employment protection, taxation of labour income, tax progressivity, active labour market policies, reallocation of labour, turnover, education and technology adoption—were enough to provide the main obstacles and solutions to restore equilibrium unemployment: the menu was long enough to keep researchers away from the temptation to search for imperfections in other markets.

That said, the model has been able to accommodate richer demand effects as well as other types of imperfections in goods and credit markets. One should also point out that, in recent public discussions, Chris has always emphasised that proper policy management would include a mix of aggregate demand and supply policies. For instance, in his public interventions on the Greek crisis, he argued that, if the origin of the crisis was a supply problem, austerity and wage cuts have ‘compounded the structural problems’ (Ioannides and Pissarides 2015: 349), and similar discussions have been present in the collection of policy recommendations by prominent Greek economists (Meghir et al. 2017). In Ioannides and Pissarides (2015) and Pissarides (2013), Chris developed the view that austerity had led to a fall in wages but not a corresponding fall in prices, this resulting in a huge contraction in demand and that, in the absence of structural reforms, austerity had amplified the crisis. Its origins were rooted in the structural problems of the Greek economy at the time of the country’s adoption of the euro.

8 Conclusion

Search frictions and the elegant modelling of matching frictions that Chris has contributed to the macroeconomic literature have proved very useful in analysing labour market equilibrium and the determinants of unemployment and its fluctuations. These contributions, however, rely on firms’ hiring costs, which are generally small (see, for example, Silva and Toledo 2009), but are nevertheless amplified, consistent with the insights of the search literature since Diamond (1971). The fact is that firms’ entry costs are more important quantitatively than only hiring costs. They include investment in capital when the investment is partly irreversible, financial frictions (new firms have imperfect access to the capital market as opposed to larger firms that already have capital and collateral) and goods market frictions (new firms must first create their market, which can be interpreted as meeting the demand generated by their customers).

From this empirical observation, one can make two additional remarks here: first, the model can very easily accommodate these additional dimensions of entry costs, and this has already been done on many occasions. For instance, Pissarides (2009) argues that the introduction of an exogenous fixed entry cost leads to better cyclical implications of the model. Benchmark models are based on equilibrium between the firm’s hiring costs and profits. Hiring costs are highly procyclical because of the matching externality. The more firms are willing to recruit, the higher the entry costs. So, when

profits increase due to increased aggregate demand, better technology or higher levels of confidence, vacancies should also rise, but if labour market tightness rises too fast, so do hiring costs, which dissipate the additional profits. As a result, the aggregate response of vacancies is too small. In adding a fixed cost, one lowers the ‘calibrated’ importance of the cyclical entry cost to match the level of unemployment and thus reduces the dissipation effect of profits. This entry cost also features, remarkably, the cost of investing in capital in an irreversible way since this cost cannot be recovered by firms in the future.

The second observation is that entry costs only need to be less variable with respect to tightness in the labour market; they do not need to be exogenous. They can thus be linked to other dimensions, such as financial frictions (see Bernanke and Gertler 1989, 1995; Bernanke et al. 1996) and whether they affect more new firms than older firms. Finally, profits themselves can be weighed down by frictions in goods markets and in particular by the length of spells where firms produce but cannot make profits since demand is absent or not met. Hence, from the benchmark model, much more can be done, with relatively moderate modelling complexity, in order to apply the matching analysis to new markets and give it full general equilibrium implications.

Indeed, this is the legacy of *Equilibrium Unemployment Theory*. It explains unemployment as the result of a number of labour market and non-labour market parameters in a flexible enough way that accommodates most country and specific time-period experiences. This is not a small achievement. At the end of this chapter, one is left with the very positive impression that Chris is someone who, over the decades, was consistently right about many theoretical and policy issues and, as such, has won most of his intellectual battles.

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Correction to: Michio Morishima (1923–2004)

Naoki Matsuyama

Correction to:

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In the original version of the book, the affiliation of author “Naoki Matsuyama” is incorrectly printed as “School of Economics, University of Hyogo, Kobo, Japan” in Chapter 26, which has been now corrected as “School of Economics, University of Hyogo, Kobe, Japan”.

The correction chapter and the book have been updated with the change.

The updated version of this chapter can be found at
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