

The magic market and the Nobel turn: a review article

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Abstract This is a Review Article of *The Nobel Factor—The Prize in Economics, Social Democracy, and The Market Turn* by Avner Offer & Gabriel Söderberg. The authors make an excellent case for tempering the individual, welfare-based, market-mechanism dominated orthodox economics, buttressed by ill-conceived bracketing of a Nobel Memorial Prize in Economic Sciences, with the social democracy of political economy. A vast array of relevant literature, both technical and more-common journalistic sources, is harnessed to make their case. This reviewer believes the authors have succeeded in their aims—if with qualifications.

Keywords The market mechanism · Social democracy · Social insurance · Welfare triangles · Consumer surplus

JEL Classification A13 · B25 · C18 · C40 · C60 · C80

A Review Article (RA) of *The Nobel Factor—The Prize in Economics, Social Democracy, and The Market Turn* by Avner Offer & Gabriel Söderberg (henceforth, referred to as ‘O&S’), Princeton University Press, Princeton & Oxford; pp. 323+xx, \$35.00. I was not uninfluenced, in my choice of a title for this RA, by a recent reading of Thomas Mann’s *Der Zauberberg* (*The Magic Mountain*) and by my ‘constant’ companion, *The Magic Number Seven, Plus or Minus Two* (Miller 1956)—see, however, O&S, p. 19, too. The alternative title would have been: *The Miracle of the Market* (see, O&S, p. 233), where Lucas’ *Making a Miracle* (Lucas 1993) and *The Asian Miracle* (World Bank 1993), would have reminded me of the *Magic* in the *Miracle* that is the *Market* (*Mechanism*)! I would like to add that I applaud O&S in the choice of the elegant painting of Alva & Gunnar Myrdal, in their study (in *Gamla Stan*), decorating the dust-jacket of the book. The original is now hanging in the *Nationalmuseum*, Stockholm.

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“Nel mezzo del cammin di nostra vita... la diritta via era smarrita. Ahi quanto a dir qual era è cosa dura... ma *per trattar del ben ch’i’ vi trovai, dirò de l’altre cose ch’i’ vi ho scorte.*” Dante, **Inferno**, *Canto 1*; italics added.¹

1 Introduction

“[I]n a larger plot in which the market-liberal advocates strove to capture the command of policy worldwide were *founded on deficient models of reality, which is the central thread of this book.* The Nobel Prize played a part in all of this, *if only a minor one*”.²

O&S, p. 230; italics added.

Avner Offer, and his younger colleague, Gabriel Söderberg, have written an absorbing, compelling, narrative of the interaction between a sometimes fragile, always arrogant, orthodox economic theory, the *Riksbanken Prize*³ in *Economic Sciences*⁴ and Swedish Social Democracy as a Political Movement. The three *dramatis personæ* of this compelling book of a drama of three acts, and many scenes, are—respectively—Assar Lindbeck, Per Åsbrink and Gunnar Myrdal.

The book richly deserves to be read, its poignant message digested and its valuable contents discussed in as many fora as is feasible, in a topical and timely way.⁵

This review article is structured as follows. The rest of this Introduction touches upon the general themes of ‘the central thread’. Section 2, is titled *Dissent on Orthodoxy, Defending Social Democracy and the Role of the Dramatis Personæ* (where more than the three key characters, named above, play their parts in the drama). It is a summary of the 11 numbered chapters and the unnumbered

¹ In Allen Mandelbaum’s English translation (italics added):

“When I had journeyed half of our life’s way,

...

I had lost the path that does not stray.

...

But to retell the good discovered there,

I’ll also tell the other things I saw”.

² The *hilfenkonstruktion* to this *central thread* is that sustained *experimental, historical and institutional* research, emphasizing *epistemology*, without the overwhelming dominance of *rational methodology* (as in orthodox theory), could help uncover aspects of *reality* that may serve as foundations for the models that underpin *empirical* policy, expressed *numerically*. I am reminded of Carl Nägeli’s acid—but ultimately irrelevant—marginal comments to the great Mendel’s humble letter of 31/12/1866: “You should regard the *numerical* expressions as being *only empirical*, because they can not be proved *rational*” (Mendel 2000; 1905, p. 6, italics added).

³ Also called the *Nobel Memorial Prize in Economic Sciences* (NMPES in contrast to O&S referring to the winners as NPWs)—to distinguish it from the more established Nobel Prizes in Peace, Literature, Medicine and Physiology, Chemistry and Physics, all of which date from 1901 and are explicitly mentioned in Alfred Nobel’s original last—the third—will & testament of 1895.

⁴ A direct translation of the Swedish *Ekonomisk Vetenskap* (see O&S, pp. 48–9)—the English rendering of which is *Economic Sciences*; does it denote a plurality of scientific approaches to economics?

⁵ An ultra-concise ‘updated’ rendering of the main message in the book can be found in the senior author’s ‘blog’, in *The Katmandu Post* of 21/11/2016 (accompanied by an informative *cartoon!*). I myself heartily endorse Offer’s characterization of Lindbeck, in this ‘blog’, as ‘the prize kingmaker’ (with respect to *many* of the NPWs).

introductory and concluding chapters of this fascinating book. They are, as is inevitable, uneven in respective lengths, more-&-less critical in tone and subject matter, richly sourced and invokes a wide range of literature from an interdisciplinary social science, which includes economic history and the history of economic doctrines.

The concluding Sect. 3 tries to draw the threads of the stance the authors take, and the dissenting agreements—if that is not an oxymoron of a phrase—of this reviewer, with some thoughts on how this drama may play out, in the years to come.

I am writing the final version of this review article on a day, now 86 years ago, of melancholy remembrance: *14th May, 1931*, the day when what is almost universally known as *Ådalen'31*⁶ shootings took place. That incident, I am sure O&S will agree, was a proximate result of the breakdown of the economic systems of the advanced industrial countries, with repercussions elsewhere, due to what has notoriously become known as the *Great Depression*, initiated in the Wall Street 'crash' of October, 1929. I date the origins and cause of the rise, rule and of almost uninterrupted 44 years of dominance, from the general elections of September, 1932, of social democratic politics in Sweden, and its ethos, to *Ådalen'31*.⁷

I find it surprising that this is not mentioned in O&S, but I suspect that it is because it cannot be tied, as the authors would wish, to the revamped macroeconomics of the Stockholm School, pioneered by the neo-Wicksellians, Lindahl, Myrdal and their largely social democratic followers⁸ (and to the nascent mathematical general equilibrium theory that underpinned the emerging microeconomics of orthodoxy). Whatever the reason, I feel that the incident merits at least a mention in this exemplary book, which is obviously partial to the political economy of social democracy.

For those readers, like me, fundamentally sympathetic to the message in O&S and to social democracy, it may have been helpful if the authors had outlined the place Swedish social democracy had, in the broad development of the European socialist movement, of the last one-hundred years, or thereabouts. Hence, references to two of the classics on this topic (in English) by Sassoon (1996) and Judt⁹ (2005), in this book by O&S, may have been useful.

Above all, in a book justly extolling the virtues of *social insurance* by the social democrats, as a foundation in the structure of the welfare state, one would have

⁶ Made 'famous' by Bo Widerberg's award winning film of 1969.

⁷ To this I add the 'suicide', in March, 1932, of the financial magnate, monopolistic manufacturer and owner of the famed Swedish matchstick industry and, as many would add (cf., Partnoy 2009), swindler, *Ivar Kreuger* (for some mysterious reason, consistently written as 'Krueger' in O&S). Lindahl, in a speech on Sweden's 'new monetary policy', of 'departing from the gold standard', felt that the 'Kreuger affair' speeded up this transition (see Per Jacobson's letter to Lindahl, 18/4/1932).

⁸ I am not sure whether Ernst Wigforss, the ruling social democrat's finance minister from 1932 for 17 years could be considered a 'simple follower' of Lindahl and Myrdal—or anyone for that matter! Sir John Hicks wrote me, on 23rd December, 1983, that a paper I had sent him 'made [him] think of Lindahl and Wigforss (I once had lunch with them together)' and (the second) Mrs. Gertrud Lindahl told me, in a personal conversation in 1982, that it was at the request of Wigforss, made at their home, to Lindahl and Myrdal, that the latter wrote *Finanspolitikens Ekonomiska Verknningar* (Myrdal 1934; but cf., also Myrdal 1930), which formed the theoretical basis for the Frisch-Tinbergen *Theory of Economic Policy*, which was the subject of the *Lucas Critique*, of a much later age.

⁹ The late Tony Judt was one of my valued Tutors at King's College, Cambridge, in 1973/74.

expected a reference to Bernard Shaw's witty essay on *The Vice of Gambling and the Virtue of Insurance* (Shaw 1944). Its absence must be an oversight by the authors of a persuasive book of 278 pages of text, with 734 references,¹⁰ and at least 25 pages of a fairly comprehensive index, a table of the *Nobel Prize Winners (NPWs) in Economics, 1969–2015*—without the reasons for the award. The pages of the *Preface and Acknowledgement*, tellingly, includes *only two* who *may* be considered *economic theorists*—(the late) Tony Atkinson and Vincent Crawford.¹¹

As for the 734 references in a book whose main text is 278 pages long, I can only remark, on the positive side, that it is a truly interdisciplinary bibliography; but on the negative side, many citations are 'journalistic' and some—numbering at least a hundred—are superfluous, irrelevant or included for the wrong reasons. Of the latter, the arguments in the references to Backhouse, Boumans, Cherrier, Ingrao/Israel, Levy/Peart, Kanbur, Kirman, Leijonhufvud, McCloskey, Milberg, Mirowski, Morgan and Rizvi are, at least, unnecessary, since the assertions in them are either technically incorrect or doctrine-historically irrelevant.

In particular, Cherrier, whose elementary errors on Myrdal, and Levy/Peart on absurd mistakes about algorithms underpinning the socialist calculation debate and Backhouse's naïve discussion of the methodology, in general, and of Popper and Lakatos, in particular, do not enhance one iota the central themes of this book. If what O&S say about the Ingrao/Israel claim is correct (p. 128, footnote 3), about the *socialist calculation debate*, then that is a false assertion by them (Ingrao & Israel).¹² Kirman's much quoted claim on the *representative agent* lacks any anchoring in the history of economic thought. Leijonhufvud's tribal metaphors are as original and as dubious as his claims on the analogy between the actions of *Maxwell's Demon* and the *Walrasian auctioneer*. Milberg's claims are both incompetent and uninformed, and his quote from Lucas Jr (1990), is completely out of context. Mirowski's technical assertions and claims in *Machine Dreams* are particularly absurd. It is a pity that O&S rely on Morgon's unfortunate interpretation of the construction and workings of the Phillips electro-mechanical hydraulic analogue¹³ machine (MONIAC)¹⁴. Rizvi's assertion about the *S-D-M theorem(s)* are absurd and shows a lack of a mathematical understanding of this result,¹⁵ which, by

¹⁰ Infelicities mar this list, even by omission, for example by the absence of any reference to anything by Walras, Wicksell, Pareto, Pigou or Keynes—surely an understanding of the economic theory of chapter 3, and much of the rest of the book, and the origins of the ubiquitous 'Harberger triangle', requires at least a nodding acquaintance with some of the classics by these venerable authors (and others—Frank Ramsey, Wald, von Neumann/Morgenstern, etc.). Misprints and omissions appear, both in the text and the footnotes, 'Falkst', instead of *Falskt*, already on p. 1, just 'Rubin' without mentioning the co-author, Peplau (p. 3, f.n. 6), and so on. I do wonder whether the final manuscript had been subject to serious proof-reading?

¹¹ I am tempted to add Andrew Scott to this 'duo', but I shall resist the temptation, *for now!*

¹² The story of existence proofs in general economic equilibrium theory, where O&S rely on the narrative in Ingrao/Israel, is particularly jaundiced—from mathematical, economic and doctrine-historical vantage points.

¹³ Analogue as distinct from a Digital machine (cf., O&S, p. 16).

¹⁴ *MONetary National Income Analogue Computer*—as 'christened' by Abba Lerner.

¹⁵ Which requires a mastery of classical real analysis in general and a thorough comprehension of Weierstrass's approximation theorem, Hilbert's 13th Problem and Kolmogorov's Theorem on it.

the way, was anticipated by Dimitriev (1974; 1904, p. 129, ff.), but also by Clower (1965, Sect. 6), when deriving the ‘dual-decision’ postulate by imposing ‘structural constraints’ on rational, individual decisions, leading to aggregate (effective) demands. This may well be the reason Clower, as *Editor* of the *Western Economic Journal* (which became *Economic Inquiry*), published in it, one of Sonnenschein’s less-technical, expository papers on the *S-D-M theorem* (Sonnenschein 1973).

The same kind of observations, although about different concepts, can easily be made about Boumans, in particular, and McCloskey.

In concluding this Introductory section let me point out that these ‘superfluous’ references, and at least the hundred others that can easily be eliminated, without diminishing in the slightest ‘the central thread’ of the book, make no experiment in the same sense of *many*—but *not all*—physicists, chemists or physiologists, that underpinned the Nobel Prizes that were awarded in the sciences.¹⁶ The kind of experiments fostered by a Rutherford or a Pauling, those done by a Crick & Watson or a Chadwick, the kind of sustained work done by Cockcroft & Walton, as described in *The Fly in the Cathedral* (Cathcart 2004), not only in conducting—and failing almost like Robert the Bruce, and rising again—but also in constructing the experimental apparatus in which the ultimate, theoretically sanctioned, success depended, is conspicuously absent in the work of the NPWs. My main criticism of the O&S book is the lack of at least one case-study of an experimental success in economics, free of ideology—or, in lieu of the lack of such an example from the core areas of economics, then, an outline of some ‘famous’ work, as that of Cockcroft & Walton.¹⁷

*I am a computable economist.*¹⁸

2 Dissent on orthodoxy, defending social democracy and the role of the dramatis personæ

“The arguments of economists are *supposed* to have a special authority, quite different from the pleadings of other parties: they are the counsel of *reason*, disinterested and *objective*. ... There is an irony here, which economists rarely

¹⁶ Kirman might claim that his work on agent based work contradicts my claim, but this is not tenable, if my observation is taken seriously. Agent based ‘exercises’, now very popular, are pure simulations without a theoretical basis for the ‘emergence’ that the purveyors of these ad hoc simulations claim. Something similar can be said about so-called behavioural economics and fashionable experimental economics—they are nothing other than ‘bicycle repair shops’ of core neoclassical economics.

¹⁷ It will *not* do, as Prescott and other NCMs do, to invoke Frisch’s *Cassel Festschrift* methodology, for which the great Norwegian was awarded the first Nobel award in economics (‘for having developed and applied dynamic models for the analysis of economic processes’), because there was, as Zambelli (2007) has shown, a great deal of ‘cooking of the results of simulations’ to fit the a priori theoretical stance of this noble NPW. The ‘black magic’ of the saintly Tinbergen, who shared the first prize with Frisch, was justly—in this reviewer’s opinion—criticised by Keynes (1940, p. 156).

¹⁸ O&S, p. 266, refer to this reviewer as a ‘non-computable advocate’! I have tried to show—‘rigorously’—the *uncomputability* of almost every approach to formalized economic and game theories. Perhaps this why O&S refer to me as a ‘non-computable advocate’!

acknowledge: they consider private self-interest to be the prime motivator, but not of their own advice”.

O&S, p.3; italics added.

The *Introductory*, un-numbered, chapter is divided into five sections, *Just World Theory*, *Social Democracy Against Economic Doctrine*, *Competing Visions*, *Theory and Performance* and *Gist of the Book*. Without doubt, at least in the mind of this reviewer, the first and last sections are crucial in outlining the themes of the book, dealt with in detail in the following 11 chapters. I think the middle three sections could easily be amalgamated into just one middle section, to convey the important messages they summarise. It is an admirable outline of the social democratic visions of O&S, against the claims for the *magic market*, sanctioned by a series of Nobel awards to economists (of a variety of ideological persuasions).

Quite apart from an idiosyncratically—not for that reason less relevant—defined concept of a *Just World Theory*, adapted for use in this book’s visions, from ‘social psychology’, the most important contrast is between Akerlof’s unjustly (in my opinion) ‘famous’ paper on the *market for lemons* (Akerlof 1970), and Offer’s unfortunately less famous critique in terms of the *markup for lemons* (Offer 2007)¹⁹; the former takes too many liberties with every kind of economic theory, whereas the latter is a more focussed, from the point of view of the issues emphasised in the general critique of orthodoxy’s lack of a historical focus, the former, in its faulty modelling of a single market, yet emphasising implications for the general equilibrium of multiple markets, relying on a copious quota of hand-waiving, for an audience who are conventionally critical of orthodox theory and its empirics, has had a citation success that is unwarranted. I would use Offer’s exceptionally well-argued *markup for lemons* to contrast with the claims and methodology of the *market for lemons* to show the vacuity of the latter, whether in microeconomic or macroeconomic courses, whether in theory or applied aspects of them.

I am not sure the claim, by O&S (p. 3) that the concept of *Just World Theory* (p. 3)²⁰ ‘is used differently’, from the way it is studied in ‘social psychology’—really *cognitive psychology*—is justified (sic!). In economics—orthodox or heterodox—it is used, albeit implicitly (often), ‘ubiquitously’ (p. 4). The authors could have profitably mentioned that it should be a subject for empirical testing, as a *belief*, in *conventional behavioural economics*,²¹ on a par with various concepts that define this field.

¹⁹ The distinction between the nebulous, if also conventionally—i.e., from an optimisation perspective—sanctioned word *market* and the distinctly non-optimal connotations associated with *markup* pricing is, itself, sufficient to show the differences in aims and methods.

²⁰ The reference, in f.n. 6 of this page is to Rubin (1975), rather than Rubin and Peplau (1975); moreover, the social/cognitive psychological literature refers to a ‘belief’, instead of a ‘theory’ of a *Just World*. This reviewer is of the opinion that *all* economic theories—whether ‘Economics is a cluster of doctrines ...’, p. 2, or not—are *teleological*, which is another name for a *Just World* belief or theory. However, not all theories of political democracy are necessarily teleological, although they are, invariably, ideological; hence, I do not agree with O&S (p. 5) that ‘Social Democracy [is] (also a *Just World Theory*), and in any case it is not part of a cluster of *economic* doctrines.

²¹ Or *Modern Behavioural Economics* (MBE), as Kao and Velupillai (2015) have called it.

In extolling the achievements of social democracy,²² O&S claim (p. 7; italics added):

‘Inequality in advanced countries fell to the lowest levels since the Middle Ages’.

Without a clear indication of what kind of inequality—income, wealth, social, political enfranchisement, etc.—the authors mean, this is a disputable claim; moreover, the composition of ‘advanced countries’ changed during the long period from ‘the Middle Ages’ to now.

As far as ‘competing visions’ are of any relevance for the stories narrated in O&S, I do not want to underestimate the importance of Myrdal’s *LO* monograph of 1944, nor of Hayek’s mendacious *Road to Serfdom*. However, I was surprised not to find any mention of an equally—if not more—important ‘social democratic policy report’, of the same year, based on ‘new’ macroeconomic theories, particularly of Keynes (1936), but—implicitly—also of variants of the economics of the ‘Stockholm School’—*Full Employment in a Free Society* (Beveridge 1944).²³

That the author of the *markup for lemons* allowed the obviously *incorrect*²⁴ ‘assumption that marginal revenue equals marginal product is pervasive in economic modelling’ (p. 4; italics added) is surprising.

The *pièce de résistance* of this chapter is the story of the origins, the personalities and the role of the *Mont Pelerin Society* (MPS) in the Nobel awards for economics. The dubious role played by the ideology of the MPS in determining the ‘market turn’, and the concomitant anti-liberal—anti-social democratic—sentiments, together with the ‘celebration’ of the achievements of formal (or informal, Bertil Ohlin, for example) present (or past, John Rawls, for instance) members of the MPS is also a poignant theme in the narrative that delineates this book. I share, wholeheartedly, the ironical, skeptical, tone expressed by O&S, regarding the influence of the MPS in determining the tone of the times.

Chapter 1, on *Imaginary Machines*,²⁵ ranges widely on a variety of topics, ranging from the *Phillips Machine* (p. 16) and the *Antikythera Mechanism*²⁶ (p. 25, ff.) to models (first &

²² Also designated a *Just World Theory*—but I am in disagreement here. I also disagree with the O&S claim (p. 11; italics added) ‘Of NPWs [Nobel Prize Winners], only Gunnar Myrdal can be regarded as a direct advocate [of Social Democracy].’ Surely, both Ragnar Frisch and Jan Tinbergen ‘can’ also ‘be regarded as [direct advocates of Social Democracy]’.

²³ It is of particular significance for the social democratic policy stance adopted by O&S, particularly because it is a sequel to the earlier work by Beveridge on *Social Insurance*, and the fact Shaw (1944; note the year!) was basically, inspired by ‘*The Virtue of Insurance*’, as set out by the two Beveridge works. Kaldor’s appendix in Beveridge (1944) was particularly relevant for the full employment policies that underpinned almost all liberal (cf., O&S, p. 110), macroeconomic policies. I do *not* take seriously Hayek’s later, utterly incredible, claim that the Beveridge Report of 1944 was written by Kaldor, *not* by Beveridge (Kresge and Wenar (1994), p. 76)!

²⁴ After all, ‘revenue’ and ‘product’ are measures of *value* and *quantity*, respectively!

²⁵ An apt epigraph for this chapter may well be (Mukherjee 2016, p. 198; italics added):

‘This is not science fiction, Science fiction is when you ... can’t do anything experimentally...’.

In the spirit of this would be epigraph, I would have titled this chapter as *Fictional Machines*. I doubt many practitioners of orthodox economic theory, particularly the NCMs, have any imagination!

²⁶ I must confess to being perplexed by references to Freeth and *not* to de Solla Price (1974)!

second sections, in general,²⁷ and in particular), wind-tunnel modelling (p. 22) and ‘the secret of market magic’ (p. 19), in the context of aspects of economic theory. In many ways it is akin to *The Two Cultures*—the *scientific* and the *humanistic*²⁸—of C. P. Snow (1964), and the attempts by O&S to contrast the confusion that can result by conflating the *methodology* of the former, with the *philosophy* of the latter. The consequence, as I think—correctly—O&S claim, is a technically meaningless *epistemology* of an economic theory, without roots in the methodology of the one, or the philosophy of the other. Once again, although I am in great sympathy with the aims with which O&S set out the framework of this chapter, I am *not* sure they succeed in achieving them.

Let me give some examples from this ambitious chapter²⁹ to illustrate my skepticism about the success of the aims of O&S (not necessarily in any chronological order, or even in the sequence in which the issues appear in the book).

O&S seem to be slightly out-of-date, when they claim (p. 22; italics added):

“New aircraft models are tested in *wind tunnels: calculations* alone are *not reliable*, and *hands-on investigation* is also required”.

Surely, O&S must know that *wind tunnel* testing is a relic of ‘my’ generation of engineering students—i.e., those who studied and designed aircraft wings and the solutions of, say, the slide-rule based, *hands-on calculations* underpinning the Navier–Stokes equations, being taught with the textbooks of Lamb, Prandtl & Tietjens, etc., till about the end of 1960s or early 1970s! Today, testing is by way of *simulations on digital computers*, but that ‘calculations are not reliable’ is a significant observation in that the programs that are executed by these machines must be written with a ‘hands-on investigation’ that must take into account it’s—the machine’s—engineering, i.e., physical, constraints. If one blindly uses *real number mathematics*, instead of *interval arithmetic* or *computable numbers*, in writing the program that is to be executed by a digital machine, there is a high likelihood of ending up with many kinds of physical debacles.

O&S also state (p. 19, italics added):

“In 1954 [Arrow/Debreu & McKenzie] showed that the *unique equilibrium* ... was capable of a mathematical *solution*”.

This is a disingenuous—but fallacious³⁰—claim, as clearly stated in Arrow/Debreu (1954, p. 266;³¹ italics added):

“*Neither the uniqueness nor the stability of the competitive solution is investigated in this paper*”.

Although McKenzie (1954) did consider uniqueness, it was for a restricted competitive model (using the *Kakutani fixed-point theorem for mappings*, but Arrow/

²⁷ Not, however, in the sense of *Model Theory*, as understood in mathematical logic.

²⁸ For Snow, the dichotomy was between the *scientific* and the *literary*.

²⁹ O&S tread on controversial grounds when, in footnote 3, p. 17, define classical and neoclassical *value* theory without any mention of the labour or utility theories (and without mentioning Marx, Walras or Menger—including Pareto & Samuelson!).

³⁰ I am not sure that O&S have seriously read, or understood, Arrow/Debreu, 1954 (or McKenzie 1954); see also O&S, pp. 230/1.

³¹ Which is just the *second page* of their classic article!

Debreu also invoked it, albeit *implicitly*—through Nash Jr (1950)). Later, in McKenzie (1959), it was a case of ‘pure existence’ in a general model of competitive equilibrium, with *explicit* use of the Brouwer fixed-point theorem.³² But in what he wrote between 1954 and 1959, Arrow et al. (1958, p. 17; italics added), cautioned ‘all & sundry’:

“[W]e want to stress that *solutions* which are not *effectively computable* are not properly *solutions* at all. *Existence theorems* and equations which must be satisfied by optimal *solutions* are useful tools toward arriving at *effective solutions*, but the two must not be confused”.

On the other hand, O&S are absolutely correct, when they observe (p. 20):

“[I]f [this kind] of model is true,³³ then society is redundant”.³⁴

The sections on *How Models Work*, *The Ricardian Vice*, *Rational Expectations*, *Is It True?*, *New Classical Macroeconomics Against Social Democracy*, *What Is It About?* and *Dissension Within Economics*, in this chapter, although interesting, do not convince this reviewer that it is relevant for the general themes of this book, partly because some of the mathematical and economic theoretical arguments leave much to be desired, but mainly because the arguments are a ‘rehashing’ of ‘ancient’ debates, but particularly of the 1920s and 1930s. For example, the NCM stance against social democracy is, really, based on an appeal to the two fundamental theorems of welfare economics and the Lucas critique, which amounts to *policy nihilism*. This is just a Hayekian dogma, elegantly taken apart by Lowe, Sraffa, Myrdal, Keynes, Kalecki, Knight and Adarkar, but clothed in different, supposedly more ‘rigorous’, terms and language of so-called ‘modern’ general equilibrium theory.

However, I fully endorse the perceptive observation by O&S (p. 21):

‘Analogy is also the method of poetry,³⁵ and like metaphors in poetry, the model can be beautifully suggestive, without being true’.

³² None of these worthies—Nash, Arrow/Debreu & McKenzie, seem to have realised that Kakutani’s theorem invokes Brouwer’s in its proof and, hence, it is also both non-constructive and uncomputable.

³³ But at least after Gödel & Tarski, one has to differentiate between ‘true’ & ‘provable’, and Arrow/Debreu (1954) is about ‘provable’, not about ‘true’, theorems of existence. I must add, here, that I disagree with the last two full sentences on AD (p. 19), attributed to Arrow and Hahn—and also with footnotes 15 & 16 (in *ibid*), particularly with the claim by O&S that Hahn, in his *Inaugural Lecture*, claimed that AD

‘[W]as mostly useful as a refutation of the ‘invisible hand’.’

Hahn, who extolled the virtues of (mathematical) rigour and precision *ad nauseum*, should be more precise about ‘mostly’!

³⁴ This is not unrelated to Thatcher’s ‘infamous’ assertion, thirty years ago, that ‘*there is no such thing as society*’? Thatcher’s is an ideological statement, whereas O&S base their observation on a dissection of the economic theory that attempted to justify ‘the market turn’—perhaps, therefore, it is also ‘ideological’, but at one remove!

³⁵ In this reviewer’s opinion, it is also one of the methods of *literature* (see also the last chapter in O&S), and is copiously confirmed by many NPWs in this field. When the Nobel Peace Prize was awarded jointly to the economist, Professor Muhammed Yunus, and the movement he found, which became the Grameen Bank, and the committee announced that it was ‘*for their efforts to create economic and social development from below*’, it augured well for economists, in general, and to development economists, in particular. O&S have many observations on the issue, but it must be remembered that Myrdal did not become a NPW for his work on development or international economics (p. 123), and the award for Sen was *not* for his work on development economics, but ‘for his contributions to welfare economics’! I have

Had the Nobel awards in economics been compared to that awarded for *Literature*, without any ‘scientific’ shackles imposed upon it, orthodoxy’s non-experimental, ahistorical, contents—severely (and justly) taken to task by O&S and other critics who (unfairly) compare it to the awards in the sciences—may well be blunted!

Chapter 2 could easily be more concise and made a section—or two—of chapter 4, which is, in my opinion, the most significant for the main theme of this book. Almost all of what I have to say, as a reviewer, would be couched in terms of what I wrote of chapter 2 and its contents—‘warts & all’,³⁶ but with gems dotting it, here and there. One such *gem*, akin to that on poetry in the previous chapter, is the following (p. 67):

‘Methodologists discuss knowledge and persuasion in economics as if it was largely a matter for seminars, conferences and journals’.

In fact, instead of suspending (or even eliminating) the prize in economics, as advocated by some,³⁷ periodically, it might be a good idea—certainly consistent with the vision advocated in this book—to impose a moratorium on ‘seminars, conferences and journals’, although enforcing it would not be easy!

One would expect, as this reviewer did, that the section on *Money and Gold*, in chapter 3, *Bitter Roots; Finance and Social Democracy Between The Wars*, as well as the final two on *Safeguarding Capitalism: The Bank of International Settlements and After the War: Central Banks Reclaim Autonomy*, would bristle with historical originality and analytical insights that would be unavailable (to ‘novices’ like me) in the standard literature, say of Ahamed (2009) and King (2016). After all, O&S, p. 57, do refer to their ‘own sub-discipline of economic history’. I suppose—but I may be wrong—that the second section, *The Stockholm School of Economists*,³⁸ is the attempt by O&S to give theoretical content, in the context of money and finance, to *social democracy*, the ‘other’ part of the title of this chapter. In this case, I expected the younger author, with his Swedish and economic history background, to provide insights that we—laymen³⁹ and specialists, alike, of a study of *Stockholm School Economics and Economists*—did not have.

Footnote 35 continued

hopes for a future when the economics committee ‘could manage to get themselves thought of as humble, competent people, on a level with dentists’ (Keynes 1931, p. 374—see also O&S, p. 276).

³⁶ I chose this phrase guardedly, because this chapter, too, has its share of less and more serious infelicities. An example of the former is the claim, on p. 58, that Lundberg was the ‘economics committee chairman’, in 1969; he was a member of the committee that year (till 1979), but became the chairman only in 1975. I think I echo the voice of the profession – orthodox and heterodox—when I dissent that Koopmans was ‘the *econometric* guru’—his invectives against Rutledge Vinig, and on the NBER methodology, was that of a ‘*mathematical economics* guru’ (but, in this reviewer’s opinion, not a very convincing one—it is only in this that I find myself, embarrassingly, on the same side as the NCM/NPWs, see, Kydland et al. 1990, p. 3).

³⁷ Not least by scions of Alfred Nobel, the original benefactor of the prize in peace, literature and the sciences (O&S, p. 1, p. 100 & p. 103).

³⁸ Note: *Economists*, not *Economics*!

³⁹ The reference list in O&S does contain some pronouncements by ‘laymen’, even of a ‘journalistic’ *genre*, on the economics and economists of the *Stockholm School*, all of which can safely be eliminated without any harm to ‘the central thread’ of the book.

Hume's 'famous' specie-flow mechanism, originally developed with the facts (!) of the sixteenth century price revolution in mind, and then as a critique—like that of his friend, Adam Smith's—of the mercantilists, makes its first appearance in this chapter.⁴⁰ Apart from invoking the Sonnenschein-Mantel-Debreu theorem several times, to discuss short-comings of a type of orthodox general equilibrium theory, Hume's specie-flow mechanism and Harberger's triangle also play significant roles in the critique of orthodoxy by O&S of automatic mechanisms and (static) efficiency postulates. On the whole, I agree with the way these results have been used by O&S. On the other hand, none of these three frameworks are necessary (or, indeed, sufficient) for any of the powerful criticisms they make against 'the market turn' of orthodox economic theory. Though I shall return to the Harberger triangle, briefly, later, I don't think the reader of this important book needs to understand any of these three concepts to make sense of the critique—and defense (of social democracy, at least of the 'older' variety) that delineates this book.

I shall, therefore, concentrate my comments on just the second section, *The Stockholm School of Economists*, of this chapter.

At the very outset I must point out that the little over four pages that makes up this section is culled out of a more substantial book by the younger author; my melancholy observations must be made against this background. Secondly, *Can Lloyd George Do It?* (p. 78) was co-authored, by Keynes, with Hubert Henderson. Finally, footnote #9, above, must be read in conjunction with the observations that follow.

The 'bitter roots' of the social democracy that Per Albin Hansson and Ernst Wigforss laid the foundations for building, was rooted in the economics of the *Stockholm School* of Lindahl, Myrdal, Hammarskjöld and Lundberg⁴¹ who, in their turn, built their interlocking theories on the basis of the monetary theory that Wicksell had constructed in 1898 (Wicksell, 1898a, 1898).

First of all, Wicksell's pioneering contributions to monetary theory emphasized at least *three* aspects, all of them, each in its own way, taken up by others as fundamental: *the irrelevance of Say's Law for short-run dynamics*; *Gibson's*

⁴⁰ Hume, like his friend Smith, and like their enlightened follower, John Stuart Mill, was both a *deductivist* and an *inductivist*, as all pragmatic people were, when confronted with empirical data. They wore their philosophical or epistemological hats, whenever it was convenient for them to do so, when addressing different audiences! None of them let an 'ugly fact' disturb their philosophical or epistemological equanimity; finding inconsistencies between the different ways they addressed issues was left to lesser mortals (like Jevons, in the case of Mill and countless defenders of mercantilism in the case of Hume and Smith). Current purveyors of orthodoxy are, in the famous phrase Joan Robinson used for the title of her *Inaugural Lecture* at Cambridge, *The New Mercantilists!*

⁴¹ I leave out Ohlin—deliberately and controversially—for many reasons. The main one is his preposterous and pompous second footnote in the second of his EJ articles of 1937 (Ohlin 1937, p. 221; bold italics added):

"*The Swedish books contain only a scanty analysis of interest theory*, so I do not know to what extent the *second* half of this section—...—*is accepted by my Stockholm colleagues*".

'Scenty analysis', indeed! This, after Wicksell, Cassel, Lindahl, Karin Koch, Myrdal! In the Lindahl archives, 'notes' between Lindahl and Hammarskjöld on these EJ articles by Ohlin, not very complementary, can be found. Ohlin opposed Hammarskjöld at every turn, but Wigforss's steadfast support—he even appointed the latter-day Diplomat as his Permanent Secretary—was firm. In his letter to Lundberg, of 13th March, 1933, Lindahl wrote: 'Even Ohlin is now trying to learn monetary theory ...' (my translation of: 'Även Ohlin söker nu lära sig penningteori...'). See also Lundberg's letter to Lindahl, of 12th August 1941, on Ohlin's review of Lindahl (1939), in the Lindahl archives.

paradox; and *a norm for price stabilization*.⁴² In all three cases, Wicksell developed his ideas on the basis of observable facts—both about policy (for example the Bank Act of 1844) as legislative laws, and on the consequential behavioural and institutional facts, represented by descriptive statistical data.

Secondly, Wicksell worked within a *static equilibrium* framework, struggling to connect the *real* and *monetary* parts of a national economy. The two volumes of his Lectures, were divided into a first, *theory of value*, part, and a second, *monetary theory* part. He never managed to link the two coherently.

Thirdly, it must not be forgotten that Wicksell's first post-Malthusian, post-Austrian-capital theoretic work, before his fundamental monetary theoretic contributions, was *Finanztheoretische Untersuchungen*,⁴³ (Wicksell, 1896)—which was followed by Lindahl's Lund dissertation of 1919, *Die Gerechtigkeit der Besteuerung* (Lindahl 1919).

These three issues, together with the points made in footnote 42, determined the work in Lindahl (1939),⁴⁴ Myrdal (1939), Hammarskjöld (1933) and Lundberg (1937)—and supplemented by the contributions of Alf Johansson, Karin Koch and Ingvar Svennilson—made up the gist of what came to be known as that of the Stockholm School economists and economics. Of course, to this must be added the important national accounts work done by Lindahl, from the late 1920s through the whole of the 1930s; all the macroeconomic policy proposals of the social democrats, particularly those originating from the social democratic finance minister of the period, Ernst Wigforss, was predicated upon the macroeconomic theories developed by the Stockholm School Economists. They developed a monetary macroeconomic framework of disequilibrium dynamics, in a model of sequential equilibrium, and went beyond Wicksell, and *anticipated* many of Keynes' innovative constructions in

⁴² The latter two points were taken up, already, in Wicksell (1898a)—see p. 17 & p. 18, respectively, of *ibid*; the first was almost the starting point for Wicksell's fundamental discovery of the *fallacy of composition* and, consequently, of the field of *macroeconomics*, which encompassed the then disparate fields of taxation & public finance, business cycle theory, monetary theory, growth theory and the theory of economic policy. Wicksell's major achievement, unrecognised as he was, in all official and orthodox circles at this time (cf., Wicksell 1898a, b, p. xi), was to unify these five ostensibly disparate fields into the one unifying subject of Macroeconomics. It was this framework that Lindahl, Myrdal, Hammarskjöld and Lundberg, with important inputs from Karin Koch, Alf Johansson and Ingvar Svennilson, developed *dynamically*. In many significant ways, Wicksell (1898a—see, again, p. 18 and the footnote on the same page) is a summary and a preview of what was to appear, almost simultaneously, as Wicksell (1898a, b).

⁴³ I was once the proud owner of Östen Undén's 1911 copy of this book; mention of 1911, reminds me that O&S may, to bolster their otherwise convincing argument on 'the market turn' of orthodox economic theory, refer to Wicksell's illuminating essay on *Varuprisens stegring* in the *socalistisk månadskrift*, TIDEN (Wicksell 1911), where a 'practical' application of an updated version of his *theory of monetary price dynamics* appears in the context of a 'lockouten i byggnadsfacket'—like that faced by the *Hammarbetarförbundet* in Gothenburg, today (May, 2017). Östen Undén was the interim social democratic prime minister between the death of Per Albin Hansson, the 'father' of the 'Folkhemmet' concept that almost defined Swedish social democracy, and the beginning of the long reign of Tage Erlander.

⁴⁴ In addition to the two classic Lindahl monographs mentioned in footnote 21, p. 92 of O&S, Lindahl (1924) and Myrdal (1927). As Lundberg pointed out in his *Raffaele Mattioli Lectures*, the change between Lindahl (1924) and the Lindahl monographs of 1929 & 1930, was Myrdal's 1927 work on anticipations, expectations and the concomitant probabilistic framework of macroeconomic policy.

the path from the *Treatise* to the *General Theory*.⁴⁵ This is why, as Bent Hansen wrote me (Hansen, *op.cit.*), Lindahl ‘did not expect much from Keynes’.

Let me conclude my extended comments on this chapter with the following two comments. On p. 85, O&S state ‘the tireless preacher of BIS economic philosophy’, Per Jacobson’s ideals for it: ‘the gold standard, independent central banks, balanced budgets and free markets’.⁴⁶ Contrast this with the characterizing triptych of *microeconomic general equilibrium theory* (MGET): rationality, equilibrium and optimality. It is ‘easy’ to show, technically, that there are many ways to deviate from one or more of Jacobson’s ideals and still be committed to the canons of MGET. It is this that the social democratic economists did, in the 1930s, with the help of the Stockholm School Economists. This is also why those wedded to the triptych of MGET—like Arrow and Hahn—could, at the same time be critical of the Hayekians and the NCMs. It is also why Lindahl and Leif Johansen could be allied to the Social Democrats or to further ‘left’ political ideologies, and still adhere to the triptych of MGET (the former in public finance and taxation, the latter in value theory).

The other point is about the ‘sound money doctrine’—and, as O&S rightly say (pp. 85/6; bold italics, added):

“Jacobson’s biography was aptly called *A Life for Sound Money*.⁴⁷ ... Jacobsen was an articulate and gregarious policy analyst, but *not* a profound thinker, a failing of which he was acutely aware. ... His views ... tend towards the oracular and mushy... **But his convictions were crisp**. His fundamentalist commitment to **sound money** was an article of faith, partly arising from laissez-faire **political** convictions, partly the conventional wisdom of policy-makers at the time. He would not have been able to ground them in rigorous analysis, and he never tried”.

I could not agree more! I shall go further: both Åsbrink and Lindbeck (substituting ‘political’ for ‘economic’ in the case of the latter),⁴⁸ can be characterized in the same way.

The most significant chapter of this book, *The Riksbank Endows a Nobel Prize*, documents Åsbrink’s shenanigans on finding ways to enhance the role of the Institution he was heading with the Nobel Prize, whose prestige—at that time—was high, if also

⁴⁵ It must be remembered that the *Treatise* was a full employment model of price dynamics; the *General Theory* was a theory of static (or short-term) under-full employment equilibrium.

⁴⁶ The four are *not* independent—it is easy to show that the fourth implies the first!

⁴⁷ The phrase should be referred to as *sound finance*, which, in many ways, is more general. In this sense, it is worth recalling Kalecki’s ‘warning’ on the connection between ‘sound finance’ and the ‘balanced budget’ doctrines, and, hence, also of *austerity policies* and those of their ‘handmaiden’, *money neutrality* (Kalecki 1943, p. 325):

“The social function of the doctrine of ‘sound finance’ is to make the level of employment dependent on the ‘state of confidence’.

⁴⁸ And to the third banker who is mentioned in the same vein, by O&S, Ivar Rooth (the Ivar becomes ‘Ivor’ in the index). When I was an undergraduate student of *nationalekonomi* at Lund, now almost half-a-century ago, my teacher in the ‘2-betygs’ course in the subject used to say: ‘If you study more economics than this level, you will not be qualified to become the Governor of the Central Bank’. How true!

chequered,⁴⁹ and which seem to have helped shape the unfortunately increasing dominance of ‘the market turn’ in economics. O&S tell this story convincingly and the chapter merits careful and critical reading—and appreciation. I myself doubt Åsbrink realized the nature of the ‘power’ his entirely short-sighted, though ‘noble’ aims, bestowed upon the committee that determined who would receive the prize. I think he felt that his aims were furthered and his ‘good intentions’ were well founded, since the *Swedish Academy* had, largely, administered the award reasonably well.

Now, almost 50 years later, we might disagree, as—I think—O&S do, on well-founded reasons.

I think, however, O&S muddy the waters that Åsbrink almost poisoned, with his inadvertent shenanigans, by conflating the quest for independence of the central bank with the funding by the Riksbank of what eventually became the Nobel Memorial Prize in Economics. In this conflation they also sully the reputation of a man of impeccable integrity in his quest for the norm of price stability and the independence of the institution primarily responsible for monetary policy: Erik Lindahl.⁵⁰ I am not persuaded by this part of the arguments in O&S, but the overall story of the *Riksbankens endowment of the Prize*, is both persuasive and believable.

I shall divide my comments on the rest of the chapters—from 5 to 11, and the interesting, unnumbered, ‘Conclusion: Like Physics or Like Literature’—into six separate items.

The methodology of chapter 6 is fundamentally incorrect, for two reasons: for using the ‘Bass model’⁵¹ and for relying on (probability)⁵² distributions of a continuous nature. O&S, and Bjork (a coauthor of chapter 6) seem to have missed the wholly unrigorous statement in Bass (1969), p. 219 (in a subsection titled ‘The *Discrete Analogue*’-italics added):

“The basic model is:

$$S(T) = pm + (q - p)Y(T) - q/mY^2(T).$$

In estimating parameters, p , q , and m from discrete time series data, we use the following *analogue*:

$$S_T = a + bY_{T-1} + cY_{T-1}^2.$$

This kind of ‘analogue’ reasoning, of ‘equating’ a *nonlinear, continuous* time, *deterministic* model, to a *nonlinear, discrete* time, *deterministic* model, and then

⁴⁹ The ‘high prestige’ was mostly due to the award for the sciences—physics, chemistry and medicine/physiology; ‘chequered’ because of controversial prizes—and lack of them—in peace and literature—but also some in the sciences—especially in the case of Einstein and Born (see chapter 2, O&S, especially p. 46).

⁵⁰ See, pp. 92/3, of O&S; that Lindahl would have argued for ranking ‘full employment’ *lower than* ‘stable prices’ I find unbelievable, from a knowledge of many of his writings from about 1924 till his premature death—but I confess to not having had access to the reference in footnote 28, p. 93.

⁵¹ Almost every mathematical assertion about the ‘Bass model’ O&S state on p. 126 is incorrect or irrelevant. It is certainly not ‘an *inspired algorithm*’ and does not (necessarily) ‘*converge iteratively* on a *stable value*’, (ibid., italics added).

⁵² Quite independently of the kind of probability underpinning the ‘Bass model’, in the O&S work—with & without Bjork—and the wholly absent argument about the connection between a deterministic determination of theoretical work and the stochastic nature of the outcome.

adding a ‘stochastic’ term, is wholly inappropriate.⁵³ In addition, I am thoroughly skeptical of any use of ‘citation data’, even in the severely qualified sense of O&S, for any kind of data analysis.⁵⁴ Moreover, ‘histogram’, naïve ‘curve fitting’ and simple tabulation of a variety of Nobel prize winners’ classifications, is more than sufficient to drive home the point that O&S seem to wish to highlight.

The ‘Harberger triangles’ play a significant role⁵⁵ in the criticism of orthodox theory’s emphasis on static allocation principles in arguing for a ‘market turn’, as portrayed by O&S.

This, unfortunately, is not related to the way Mirrlees model, via its use of the modernized version of the calculus of variations—i.e., optimal control theory—is used, within the restrictive framework of the Arrow–Debreu model of *static allocation* postulates of *efficiency* and *equilibrium*. In other words, a theoretically illegitimate conflation of static and dynamics is, schizophrenically, adopted, by orthodoxy, to justify the ‘market turn’, in the story narrated by O&S.

The ‘legitimacy’ of the ‘conflation’, as I pointed out above, is what the Stockholm School Economists achieved—and the applied social democracy that was the hallmark of the economic policies adopted by Ernst Wigforss.

In a related sense, I would also like to point out that the ‘theory of the second best’, first broached by Corlett/Hague (1953/54),⁵⁶ is *not* developed inside a ‘constrained’ AD model.

On the other hand, Lancaster did make available, to a wider audience—including, hopefully Lindbeck and his collaborators—an idea of the concept of efficiency in dynamic models (Lancaster 1973). Using this concept, which is quite different from that which Lindbeck used *ad nauseum* in his tirades against social democracy, it is easy to show that a suitable mathematical formalization of the crucial aspects of a ‘Meidner model’ is more *dynamically efficient* than ‘capitalism’. This would enhance the positive aspects of Meidner’s proposals and, at the same time, substantiate some of the stances taken by O&S.

I am in full agreement with everything (negative) in O&S write about Lindbeck⁵⁷—in fact, I would go further in my criticism of his ‘kingmaker role’ in the determination of Nobel prize winners in economics. Alas, his influence will survive his over-long tenure, in the committee for awarding the Nobel prize in economics—but, surely, it will not be ‘indefinite’ (at least I hope so, as I am sure, O&S do, too!).

⁵³ Although I have used the word ‘rigorous’ earlier, I am loath to use ‘unrigorous’, instead of inappropriate, here—mainly because I do not know how to define ‘rigorous’ in any precise sense!

⁵⁴ I think O&S would agree with the discussion prompted by the articles on ‘citation indexes’ in *Nature*, 23/6/2005, 26/4/2017, and 25/5/2017!

⁵⁵ I do not think any of its use in O&S is warranted—especially because of the ambiguous nature of the ‘rigorous’ definition of *consumer surplus*.

⁵⁶ *Not* in the 1956 paper of Lipsey/Lancaster, p. 60, O&S.

⁵⁷ Lindbeck is exemplary in his *ignorance* of the ‘nuts & bolts’ of the paper by Arrow/Debreu, in particular, and of general equilibrium theory and CGE, in particular, shared by his sometime colleague, if one is to go by his one-dimensional case for Allais to be awarded the Nobel prize, Ståhl! Scarf, in a personal conversation in 2012, told me that he had ‘inadvertently’ snubbed Lindbeck and, consequently, did not ever expect to be awarded the Nobel prize for economics! I also think many other *economists* were better painters than Lindbeck—as well as more ‘complete’ economists (Richard Goodwin and Robert Mundell—NPW, 1999—just for ‘starters’).

Finally, in the interesting, unnumbered, concluding chapter, O&S state on p. 265 (italics added)⁵⁸

“In the words of the philosopher Thomas Nagel, ‘... [W]e are confident that questions of *arithmetic always have determinate answers*’. Perhaps that is why economics seeks the supposed finality of *mathematics*. In the trade-off between *rigour* and relevance, rigour wins out. But when theory is confronted by evidence, there is no secure *foundation* to be found, and everyone, to some extent, is whistling in the dark”.

This is patently incorrect—as can be determined from *Gödel’s incompleteness results* and the *(Recursive) Undecidability of the Halting Problem for Turing Machines*; but also, for example, by the ‘fate’ of Goldbach’s conjecture and many results in the mathematics of geometric constructions, using even ‘ideal’ (‘imaginary’?) tools, from Greek times to the enunciation of the *Entscheidungsproblem*—i.e., Turing Machines and Ramsey Theory. Nagel is simply re-stating a Hilbertian belief: *Wir müssen wissen—wir werden wissen*—something which is the basic backbone of Debreu (1959), and is fundamentally antithetical to Lindahl’s belief.

In this sense, O&S are completely correct in their attribution of belief in the Hilbertian finality of mathematics—and mathematical economists, particularly of the Frank Hahn variety!

3 Concluding thoughts

“Let theorists do theory. But policy requires more humility Economics has many powerful traditions to draw upon *A tradition of empirical investigation goes back to Adam Smith and political arithmetic before him*. This almost amounts to another canon. It is more modest in its claim from theory, but its findings and understanding are more secure”.

O&S, pp. 276/7; italics added.

Political Arithmetic, in its contemporary incarnation as *National (income & wealth) Accounting*, seems to be the endeavour, by economists (of any persuasion), that O&S seem to favour most⁵⁹ (for e.g., p. 123, pp. 276/7), and the three NPWs

⁵⁸ The short paragraph which contains this quote, invoking Nagel, a philosopher, Blaug, a historian of economic thought and Varoufakis, in his role as a political economist, is a testimony to the wide variety of sources O&S invoke, to substantiate their case. Incidentally, ‘foundation’ is spelt incorrectly, in the quoted paragraph.

⁵⁹ But O&S seem to think ‘its powerful breakthrough was already a thing of the past’ (p. 123). I am afraid the authors are unaware of the recent literature in NCM (with which, of course, I do not agree)—subscribed to by *all* the NPWs of this persuasion (cf. Chari 2007). The ‘econometrics’ that ‘was recognized in the very first award’ (say to Tinbergen in 1969) is, surely, very different from the econometrics that has been ‘recognized ... periodically ever since’ (say in the award to Sims, in 2011); *ditto* for the behavioural award to Simon, in 1978 and the one to Kahneman, in 2002. Simon was the pioneer of formal behavioural economics; Kahneman’s notion of behavioural economics is very different from that of Simon’s. It may be apposite to point out that Simon was *not* awarded the prize ‘for his behavioural concept of bounded rationality’, but ‘for his pioneering research into decision-making

they identify as practitioners of this noble art are Kuznets, Leontief and Stone.⁶⁰ It may be useful to add Hicks' name—also, of course, a distinguished NPW—to this illustrious list; after all, to the question posed by Arjo Klammer, in June, 1988 (Klammer 1989, p. 180; bold italics, added):

“*Would you like to be remembered as **the accountant of the economic profession?***”

Hicks replied (*ibid*):

‘I would not in the least mind. No, I would not’.

This ‘Accountant among economists’, more than a quarter of a century earlier, in the *Lindahl Festschrift* (Hicks 1956; 1982, p. 221; italics added) claimed that:

‘In all its main forms, modern *economic dynamics* is an *accounting theory*’.

And added, some years later (Hicks 1973, p. 8; first set of three italics, added):

‘[Lindahl] was the father of *Social Accounting theory*’.

In a letter of 2nd January, 1985, Bent Hansen (1985), wrote me (italics added)

“Had [Lindahl]⁶¹ lived another ten years or so⁶² he would have been *our obvious Nobel-prize laureate*”.

Leif Johansen, a *life-long member of the Norwegian communist party*, theorist and applied economist *par excellence*, died at the age of 52, in 1982; he contributed fundamentally to multisectoral models of growth theory, in a setting of consistent *national system of accounts* (Johansen 1959, 1960).⁶³ No one person embodied the Lindahlian vision and traditions, coupled to the methodology and practice of Richard Stone, more fundamentally than Leif Johansen. Richard Stone, the NPW of

Footnote 59 continued

process within economic organizations’. Words like accounting, behavioural, econometrics, games, and even phrases like business cycles, general equilibrium theory, etc., have a way of evolving and becoming things they were not, like chameleons changing colour! All the terms and phrases within quotes are from p. 123 of O&S.

⁶⁰ Neither Kuznets, nor Leontief, were awarded the prize, formally, for any contributions to the development of systems of national accounts. A systematic study of the ‘official’ reasons for the awards, suggests to me that the committee, itself of changing composition, has been trying hard to find ways of identifying NPWs for non-identical, pioneering, contributions.

⁶¹ In May, 1973, during an interview for admission as a PhD student in the Faculty of Economics & Politics, Cambridge University, Kaldor asked me (see also the next footnote!):

‘Why do you want to come to Cambridge when you have Lindahl in Sweden?’

Kaldor himself, increasingly unhappy at the LSE—not least due to Hayek—was taken to the UN Economic Commission for Europe by Myrdal. Incidentally, it was here that Kaldor first became acquainted with the famous ‘Verdoorn Law’, which was to play an important role in Kaldor’s proposals to reverse some of the directions in growth policy, in the UK.

⁶² Lindahl died on 6 January, 1960.

⁶³ He was, of course, instrumental in providing a theoretical *rationale* for *putty-clay* models of capital in growth models (Johansen 1959).

1984, was awarded the Prize for ‘having made fundamental contributions to the development of systems of national accounts and hence greatly improved the basis for empirical economic analysis’—he was, as the first Director of the Department of Applied Economics, in the Faculty of Economics and Politics at Cambridge, instrumental in developing also *Computable Models of Growth* (long-term) and short-term models of *Keynesian Macrodynamics*.

So, had Leif Johansen lived a few more years, it would have been ‘natural’ for him to have been a NPW, together with Stone, partly because the tradition of national income accounting, within which Leif Johansen developed his multi-sectoral growth models (which had its origins in the work of the Swedish and Norwegian economists, which pre-dated, at least by a decade, the work of Keynes), which was the basis for Stone’s work. Awarding a shared prize to Leif Johansen (and to Wynne Godley⁶⁴ and Lance Taylor), the Nobel Committee may have allowed a *broader* interpretation of the ideological background of the NPWs—thus correcting the ‘ideological bias to the right ... especially during the 1990s’.⁶⁵ I would add, since O&S are too well-mannered to do so, not only ‘during the 1990s’, but both before and after—but ‘especially during’ the long reign of ‘the prize kingmaker’ (cf., footnote, 4, above).

Even more importantly, the lack of a recognition of the works of Hirofumi Uzawa, Herbert Scarf and Takashi Negishi in making the *theoretical* work of the general equilibrium theorists, Hicks, Arrow, Debreu and Allais—all NPWs—*empirically applicable* in a (restricted) *computational* sense, remains mysterious, at least to this reviewer. After all, the NCM awardees, Lucas, Prescott, Kydland, Sargent and Lars Hansen, who claim to have made the Arrow–Debreu static general equilibrium models dynamic and computational, owe much to the pioneering work of the trio of Uzawa–Scarf–Negishi.

Why O&S do not mention either of these issues—although their ‘immanent’ claims on behalf of (the elder) Galbraith and Joan Robinson is admirable, but is entirely based on a variety of citation indices—remains a puzzle to me. Is it because they have given up on NPWs originating in heterodox traditions? I am one who is wholly sympathetic to the visions of O&S, nevertheless I have *not given up hopes* that wholly new traditions may be recognised in a future that is unforeseeable—especially by the kind of citation analysis, and other determinants, that seem to be—and has been—extolled by various committees, selecting NPWs.

Surely, hope for change is second only to the deed of changing!

⁶⁴ Died, unheralded in official circles, in 2010.

⁶⁵ The age difference between Stone & Johansen was not very different from that between Hicks & Arrow! On the other hand, the committee could have waited another decade, or two, provided Leif Johansen was still alive, to award the prize to him, to be shared with Wynne Godley & Lance Taylor, two others who worked intensively in integrating flow-fund analysis in national accounting with macrodynamics of a Keynesian variety. Again, it would have signalled the committee’s willingness to recognise the contributions of non-orthodox institutional affiliations—but, then, both Meade & Stone were representatives of a kind of Keynesian-Cambridge and Modigliani and Hayek had been affiliated with the *New School of Social Research* (NSSR). By the way, at least one former Chairman of the committee was once a member of the Swedish communist party!

Finally, O&S seem to be ‘ambivalent’ about the role of Hicks, especially in the ideological spectrum they construct. However, this is the same Hicks who, in 1983 (p. 375; italics added), wrote:⁶⁶

“I can understand for those who are concerned with the defense of ‘capitalism’, to show the possibility of an arm’s length equilibrium (an ‘invisible hand’) is a matter of importance. *But that was not, and still is not, my concern*”.

Surely, this should convince O&S that they were less than charitable in being ambivalent about Hicks’ ideological stances!

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⁶⁶ This was also the subject of the seminar Hicks gave, *at my invitation*, at the European University Institute, in 1984.

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