

Arnold Heertje. *Schumpeter on the Economics of Innovation and the Development of Capitalism*. Edited by Jan Middendorp. Cheltenham: Elgar, 2006. Pp. vii + 142. ISBN 1 8454 2445 X. £56.99; US\$90.00.

Peter Earl*

For the past quarter of a century, Arnold Heertje has played a commendable role in fostering the revival of interest in the work of Joseph Schumpeter. This short book is a collection of eleven of his essays written during this period, three of which have not been previously published. Despite his enthusiasm for Schumpeter's dynamic view of capitalism, Heertje never lionises him and repeatedly draws attention to shortcomings in his work, not merely about his views on the future of capitalism but also regarding, for example, how little he had to say about the role of a steady stream of minor technological improvements in generating continuous change, the processes by which innovations diffuse and why some of them fail to succeed (pp. 90-1).

The collection has been put together by Heertje's former student, Jan Middendorp, who has done a good job in terms of sequencing them on the basis of their focus rather than chronologically. It begins with Heertje's *New Palgrave* essay on Schumpeter and ends with his critical review of the work of neo-Schumpeterians such as Dosi, Freeman, Nelson and Winter relative to that of mainstream economists such as Romer and Tirole whose work on technological change, endogenous growth and international trade has some themes in common with Schumpeter's vision. To browse in this book would undoubtedly be a good way of getting a quick overview of Schumpeter's work, and no doubt it is on that basis that many economics librarians will order it. Unfortunately, despite Heertje's standing in the Schumpeterian literature, the book's origins as a set of papers, rather than a substantial, integrated work written from scratch, ensure that it will be a great disappointment to serious scholars of the history of economic thought.

The essential problem with this book is that, despite its brevity, there is an enormous amount of repetition – particularly regarding Schumpeter's various visions of the nature of production functions, the causal independence of innovation from invention, and the reasons why Schumpeter's predictions of the demise of capitalism have not eventuated – and this gets in the way of covering anything in depth. The dust-jacket blurb promises that it will give 'a valuable insight into the life and work' of Schumpeter, but as far as the former is concerned, far more can be gleaned from Swedberg's superb introduction to the 1992 edition of *Capitalism, Socialism and Democracy*. As far as Schumpeter's work is concerned, there is no examination of its practical impact, despite Gao (1998, pp. 97-8) having drawn attention to Schumpeter's considerable influence in Japan, particularly via some of his Japanese postgraduates. Nor is there any thoroughgoing analysis of his own intellectual influences in the manner of, say, the paper in this *Review* by Michaelides and Milios (2005) on the extent to which he was influenced by Hilferding. Instead, it is simply emphasised that his knowledge of the history of economic thought was prodigious. Even the relationship between Schumpeter's vision for capitalism and that of Marx is covered very briefly each time it is mentioned. While Heertje very neatly makes the point that Schumpeter saw technological change as often being capital-saving rather than labour-saving and hence he reached a different conclusion from Marx about why capitalism would

eventually get displaced, the significance of this is probably going to be lost on undergraduates who lack prior training in Marxian economics.

For Schumpeter, capitalism's demise would come about not due to falling rates of profit but due to questions being asked about its rationale, as it came to be dominated by giant firms in which innovation became a routine activity undertaken by teams rather than by outsider individuals going against the stream in pursuit of new products and processes, new markets and new ways of doing business. Heertje sees Schumpeter's ultimate view of the future of capitalism as flawed due to his losing sight, in his successive books, of the likely ongoing significance of the creation of new firms in the manner so evident in the past couple of decades. However, he does not look around for other features of modern capitalism that might lead to challenges to the role of big business, such as joint ventures and strategic alliances that, as Kay (1996) has argued, enable smaller firms to assemble the resources to undertake major new projects that Schumpeter would have tended to see as only possible via giant monopolistic enterprises. Nor does Heertje explore the modern diversified corporation as a vehicle for pursuing Schumpeter's 'new combinations' in terms of economies of scope. Such firms commonly piece together new products by raiding their existing products' parts bins and finding new applications for the technologies at their disposal. Heertje acknowledges the career risks that are being taken by those who sign off major projects whose outcomes could have major implications for shareholders (p. 34). However, he does not explore the recent explosion in executive pay and the possibility that it might eventually lead to a repeat of the late-1960s public displays of discontent with big business that he mentions (p. 45) as consistent with Schumpeter's thinking but which fizzled out without leading to capitalism's overthrow.

With the fall of communism since 1989, Heertje naturally has things to say about its demise as well as the state of capitalism. His main vehicle for this is a review essay of Joseph Stiglitz's *Whither Socialism?*, where he stresses that competition is more important than privatisation. He clearly regards Stiglitz as a far greater economist than Schumpeter, though he chastises Stiglitz for failing to devote enough attention to the problem of dealing with environmental issues in a mixed economy. One issue that Heertje raises (pp. 71-2) is that modern economists such as Stiglitz seem have forgotten Ricardo's observation that non-reproducible goods have no natural (reproduction cost-determined) price. Hence they also forget that, if production takes place at the cost of wiping out the natural environment, prices for non-reproducible goods could rise to 'unbelievable' levels. He also explores contrasts between Schumpeter and Keynes, essentially portraying Schumpeter's view of the trade cycle as driven from the supply side via swarms of innovations, rather than from the demand side, with new products generating new wants (p. 78).

Austrian and evolutionary economists might expect this volume to compare and contrast Schumpeter's view of entrepreneurship with that of Israel Kirzner, and the latter does indeed figure here several times. Heertje contends that Kirzner's view of entrepreneurship, which emphasises the alertness of entrepreneurs to profit opportunities, is of the 'utmost importance' (p. 35) for fleshing out Schumpeter's analysis (see also p. 9). Heertje argues this on the basis that entrepreneurs need first to be alert to possibilities for innovation and that market prices are, to those who are alert to their implications, information carriers that serve as flashing lights to mark profit opportunities (p. 125). In doing so, he takes attention away from the fact that whereas Schumpeter saw entrepreneurial

activity as primarily being concerned with innovation-based non-price competition that upsets the established order, Kirzner focusses essentially on entrepreneurs as equilibrating arbitrageurs. Worse still, Heertje fails to notice that Schumpeter's analysis, with its focus on entrepreneurs experimenting with 'new combinations', makes more sense than Kirzner's view. Whilst entrepreneurs may indeed be 'on the look out' for profit opportunities, the latter are not lying around tagged as such. Rather, entrepreneurs use their creative thought processes to form connections, some of which they *conjecture* to be ways of making money. Heertje also argues that entrepreneurial alertness cannot be made a matter of routine (p. 35), which is a reason why new firms will continue to be significant. However, if we see it as forming 'new combinations', then there is surely considerable scope for doing so in a systematic manner, not merely in terms of looking at as-yet-untried combinations of technologies and parts modules within a manufacturing firm's portfolio but also new genres of products that might be formed from elements of existing products offered by firms in the arts, entertainment and catering sectors.

* School of Economics, University of Queensland, St Lucia Qld 4072, Australia.
Email: p.earl@economics.uq.edu.au.

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<p>William Coleman, Selwyn Cornish and Alf Hagger. <i>Giblin's Platoon. The Trials and Triumphs of the Economist in Australian Public Life</i>. Canberra: Australian National University Electronic Press, 2006. (Pp. xvi + 263). ISBN 9 20942491 1 (pb.); 9 20942505 (electronic). Price not stated.</p>
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Peter Groenewegen*

This is a most enjoyable book. I read it first in the United States at the beginning of April, having purchased a copy from one of its authors, William Coleman, at Duke University for \$US5.00. Duke University was of course a very appropriate place to start reading it, given Craufurd Goodwin's status as an historian of Australian economics. I continued reading the book in Washington, another appropriate place since my hotel was close to both the Capitol and many of

Washington's economic policy institutions, finishing it on my flight from Washington to London. This shows that a serious book, if interesting enough, can also make good plane reading.

Within its ten chapters, the book presents the life and activities of four Australian economists in the 1920s: Giblin (1872-1951), Brigden (1887-1950), Copland (1894-1970) and Wilson (1904-1996). In turn, these chapters deal with the platoon's Tasmanian genesis, the building of the platoon at the University of Tasmania, gold and capital as early interests, economic science and political power as early experience, Giblin and Keynes (or the birth of the multiplier), the Great Depression and breaking inflation, open letters and private correspondence, the seven-pointed star of one Commonwealth and six States in the Australian federation and its financial relations, war and peace, concluding with 'the last ridge' or the final years of the four economists who made up the Giblin platoon. Much research for the book involved archival work, as clearly indicated in the acknowledgements and notes on sources provided at the start of the book. The new material on the four economists discussed within its covers gives considerable freshness to its contents.

It is a nicely illustrated book. The superb Dobell portrait of Giblin on the cover is a particularly enjoyable bonus, as are the many black and white photographs inside, including the striking portraits on the opening pages of the book's four 'heroes'. Although they were all economists who contributed greatly to public life from the 1920s to the 1950s, only Wilson was essentially a public servant (serving in Treasury, for a long time as its permanent head). The other three occasionally worked in public service positions during their economic careers. Thus Giblin was a Deputy Statistician in Tasmania until 1929 before his academic career as Ritchie Professor in Melbourne; from 1929 Brigden was Economist and Deputy Chairman of the Australian Overseas Transport Association; and Copland held ambassadorial positions after his Melbourne academic career. Giblin, Brigden and Copland served together from 1927 to 1929 as members of an Official Inquiry into the Australian tariff, which reported to the Bruce-Page government in early 1929, not long before the commencement of the Great Depression. This very important report from the 1920s is one striking demonstration of the enormous quality of Giblin's 'platoon' and the contribution it made to Australian public life and policy. The Tariff Inquiry itself does not get much coverage in the book, but Samuelson is reported (pp. 66-71) to have argued its significance for the creation of modern, as distinct from 'classical', international trade theory. In Australia it was seen as a landmark in the entry of economics and academic economists into public life. As a Sydney University academic economist I may be allowed to note that its two academic economists in 1927 (R.C. Mills and F.C. Benham), as staunch free traders, were not permitted to serve on the inquiry and therefore criticised its outcome severely, especially in the case of the latter (p. 72).

The platoon was involved in more than tariff inquiry, important though this topic was for Australia in the 1920s. Some were involved in preparing national income estimates, the first since Coghlan's work in this area. A delightful story is Brigden's crude estimate of Australia's national income in 1915 as £377 million which, surprise, surprise, perfectly matched ('compared decently' as the authors' understatement has it, p. 36) with Butlin's careful 1962 estimate of ...£377 million. Back-of-the-envelope calculations can be useful. Another 'statistical' story of interest is Wilson's account of postgraduate research failures at Oxford in the

1920s. Wilson's (unnamed) tutor at Oriel College 'knew nothing' about his subject; and when Wilson requested a calculating machine to aid his research, he was offered seven-figure log tables instead. Economic calculations at Oxford in the 1920s were virtually all done in pencil (p. 46). Wilson nevertheless gained his Oxford D.Phil. and went on to complete a Ph.D. at Chicago under Viner. On publication, Viner described Wilson's Ph.D. thesis as a 'distinct advance' on knowledge in its field (pp. 46-7), subsequently discussing the book from the thesis in his *Studies in the Theory of International Trade*.

Giblin's independent discovery of the multiplier is also a fascinating 'chapter' in this book, even if it is a quite familiar story. Particularly interesting for me was the possible association (ultimately denied in this book) between Giblin's 'invention' and that by de Lissa in the 1890s (pp. 85-7). However, the links between de Lissa's multiplier and Quesnay's dynamic *Tableau économique* are not recorded even though this aspect of the matter was fully reported in Groenewegen and McFarlane (1990, pp. 72-6). The chapter on their work on federal financial relations is likewise an interesting one, and discusses in detail Giblin's contributions to the early work on fiscal equalisation and special grants conducted by the Grants Commission as one of its foundation Commissioners.

My Sydney University background (and my current role as historian of its Faculty of Economics) leads me to make a few more nit-picking points. *À propos* a remark on p. 75, the Economic Society at the Branch level continued to be strongly supported by the economist community until well into the 1960s at least. For example, the monthly meetings of the New South Wales Branch during that decade were still quite well attended. I myself went regularly as an undergraduate student, and was by no means alone in this respect among students. Society membership and subscription to the *Economic Record*, I might add, were strongly encouraged for university economics students at first-year lectures. I therefore started my membership of the Australian Economic Society in early 1957 and have now, for almost fifty years, continuously been a member.

Furthermore, although it is implicitly acknowledged (p. 96) that Swan's economic studies started at the University of Sydney (his teachers are listed as R.C. Mills, Ronald Walker, S.J. Butlin and John La Nauze), it omits to say that he graduated from Sydney with first-class honours in economics and the university medal. He also taught at Sydney University before leaving for Canberra University College (the forerunner of the ANU) in 1949 as one of its foundation professors. His Sydney experience is interesting if only because as an undergraduate he had already come under the influence of H.C. Coombs, who was teaching part time in Economics at Sydney in the late 1930s.

Given its coverage of Australian applied economics, the book makes an important contribution to the history of Australian economics in the middle of the twentieth century, which emphasises both its practical orientation and its often statistical nature. It fails to address, however, the intriguing question for me of why Australia produced so much interesting applied economics over its history of two and a quarter centuries. Perhaps it was the difficulty of the subject which attracted economic writers of quality, a characteristic of the subject well caught in a quotation from one of Marshall's letters which appeared on the frontispiece of the 1929 Report on the Australian Tariff: 'My favourite *dictum* is:- Every statement in regard to economic affairs which is short is a misleading fragment, a fallacy or a truism - *Alfred Marshall to Louis Fry, 1914.*' This was a motto clearly familiar to at least some of Giblin and his platoon while

they laboured on their theoretical and applied contributions to economics. This careful evaluation of their work constitutes a valuable contribution for all those interested in Australian economic policy old and new or, for that matter, in economic policy *per se*.

* School of Economics, University of Sydney, Sydney NSW 2006, Australia.
Email: Peter.Groenewegen7@bigpond.com.

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Vincent Barnett. *The Revolutionary Russian Economy, 1890-1940: Ideas, Debates and Alternatives*. London: Routledge, 2004. Pp. xii + 142. ISBN 0 15 31264 7. US\$120.00.

Michael C. Howard*

The main title of this new book is perfectly appropriate. The period with which it is concerned was obviously revolutionary, both politically (as the events of 1905-6, 1917-21 and 1928-33 attest) and in the organisation of economic modernisation (under Sergei Witte's tutelage in the 1890s and, especially, with the onset of Stalin's command planning in the 1930s). But there were also important continuities that make it a period that can be sensibly studied as a unity. The 1890s were not the years initiating Russian modernisation, but intensive industrialisation did begin then; the 1930s did not complete industrialisation, but they did end significant institutional change at the economic level in the Soviet Union. Moreover, the dominant motive throughout was a constant – to achieve a military defence capability against expansive Western powers. Thus the Russian state played a leading role throughout, the emphasis was always on capital investment rather than consumption, and the focus was on the rapid incorporation of advanced technology and the transformation of primitive peasants into urban workers. Judged in terms of this geopolitical objective the overall verdict is one of huge success. Between 1941 and 1945, the Soviet Union fought the vast bulk of the Nazi forces on the Eastern Front, and did so largely from its own resources. It emerged unambiguously victorious.

Vincent Barnett deals with the economic history of this half century and with the economists who were influential on this history. The best analysis in the book is of the New Economic Policy (NEP) in the 1920s and of the Stalinist command economy in the 1930s. But the pre-1917 Listian strategy of Tsarist ministers and bureaucrats is also competently outlined. The ideas of economists who contributed to the design, management and improvement of all these projects are given close attention, including those of M.I. Tugan-Baranovsky, N.I. Bukharin, V.K. Dimitriev, A. Bazorov, S.G. Strumlin, L.N. Yurovsky and L.V. Kantorovich. These people constituted a huge stock of intellectual talent, and, on the whole,

Barnett makes a worthy attempt to integrate the role of human agency with structural imperatives.

Nonetheless, there are some surprising omissions. While space is devoted to discussing the ideas of Western thinkers like John Maynard Keynes and Thorstein Veblen, the vastly more important ideas of Ludwig von Mises and G.A. Feldman make no appearance, while those of Friedrich von Hayek merit less than a page. Moreover, apart from Bukharin, the ideas of leading Bolsheviks are virtually ignored. Leon Trotsky makes an appearance as a critic of Stalinism, but his principal intellectual achievement – to accurately forecast in 1905 the character and structure of the revolutions in 1917 – is passed over in silence. Nor can this be justified on the ground of its being ‘political’ rather than ‘economic’, because Trotsky’s diagnosis is based upon the historically specific nature of Tsarist industrialisation. The very same analysis also emphasised that the socialist revolution would confront a huge contradiction because the interest of the overwhelming majority of the peasants would inevitably become incompatible with it very quickly. It was this problem, above all else, which brought on the Stalin revolution in the late 1920s (Howard and King 1989, chapter 12; 1992, chapter 2).

The final chapter asks whether a rationalist transformation of society is at all possible, although no answer is attempted. This is perhaps forgivable because of the complexities involved, but not the absence of any serious analysis of alternative trajectories in Russian history, since ‘alternatives’ explicitly figure in the sub-title of Barnett’s book. The human costs of Russian economic development between 1890 and 1940 were very large (however, recent research indicates that they were less than typically claimed by Western historians during the Cold War. See Gregory and Harrison 2005), and it is likely that many of these costs were unavoidable – every modernisation endeavour to date has had its dark side, and typically a very substantial one. That said, posing the counter-factual questions is essential for comprehending the causation of the actual-factual, as well as for shedding light on the knowledge, ignorance and morality of those who shaped it. It is a pity that no enlightenment is on offer, given the huge impact that Russia and the Soviet Union have had on world history during the twentieth century.

* Department of Economics, University of Waterloo, Ontario, Canada N2L 3G1.
Email: mchoward@watarts.uwaterloo.ca.

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Robert W. Dimand and John Geanakoplos (eds). *Celebrating Irving Fisher: The Legacy of a Great Economist*. Malden, MA and Oxford, UK: Blackwell, 2005. Pp. xv + 456. ISBN 1 4051 3306 6 (pb.). US\$24.99.

J. E. King*

By almost any standard Irving Fisher (1867-1947) was the greatest American economist of the first half of the twentieth century. In terms of citations, for example, he outperforms his nearest competitors, Wesley Mitchell, J.B. Clark and Frank Taussig, in the ratio 9:3:1:1, 'with the ratio growing over time' (p. 3). This volume contains the papers presented at a commemorative conference at Fisher's university, Yale, in May 1998 (just a year too late for the fiftieth anniversary of his death), together with three previously published papers by another great Yale economist, James Tobin. There is a very strong Yale flavour to the entire book, which first appeared as the (bumper) January 2005 issue of *The American Journal of Economics and Sociology*. It is a handsome volume, and remarkably inexpensive. It begins with a brief but moving tribute from Fisher's grandson, the geologist George W. Fisher, which is preceded by several photographs of the old man himself. Aged 35, there is a distinct resemblance to Bernard Shaw; in his sixties and seventies he looks more like a lankier version of Sigmund Freud, with just a hint of Leon Trotsky.

A brief editorial introduction is followed by Tobin's authoritative *New Palgrave* entry on Fisher and a series of contributions on his approach to general equilibrium theory, starting with two big-hitters, William Brainard and Herbert Scarf, whose paper is critically appraised by Donald Brown, Felix Kubler and the physicist K.R. Sreenivasan. Fisherian macroeconomics is then assessed by Robert Hall, Tobin, Martin Shubik, Peter Phillips, John Rust and Robert Dimand. There follow two short appraisals, again by Tobin, of Fisher's unsuccessful introductory text *Elementary Principles of Economics* (1912) and his *The Nature of Capital and Income* (1906). Next, John Shoven and John Whalley, Alan Auerbach and Michael Graetz offer sympathetic appraisals of Fisher's proposal for income taxation to be replaced by an expenditure tax, which seems to have inspired Nicholas Kaldor's better-known 1955 advocacy of the same fiscal reform. Then more big-hitters arrive to discuss Fisher's work on index numbers, with Geanakoplos and Robert Shiller focussing on his suggested inflation-linked bond and its connection to his 'impatience' theory of interest, and Erwin Diewert and Matthew Shapiro dealing with index numbers more generally. The book concludes with a series of papers on Fisher as a health economist, by William Nordhaus, Dimand once more, the economist T.N. Srinivasan, Victor Fuchs, Alvin Feinstein and Martin Zeckhauser.

Taken as a whole this is an extremely interesting book. I doubt, though, whether anyone will want to read it from cover to cover. Some of the papers are formidably technical, not least (and not unexpectedly) the lengthy contributions of Geanakoplos and Phillips, and these will appeal only to specialists. There is also much for the general reader to enjoy, including the Brainard-Scarf reconstruction of Fisher's water-powered general equilibrium machine, which seems to have been a sort of Walrasian forerunner of the famous (A.W.) Phillips hydraulic model of the macroeconomy. Dimand invokes Fisher to solve a Keynesian puzzle: the notion of a 'corridor of stability', he argues, can be used to explain why the economy is not normally 'wildly unstable' but can on occasion (as in 1929) fall right over the edge.

Several of the papers on Fisher's health economics make for informative reading, though few will be convinced by the twelfth of his 'Fifteen Rules of Health': 'Do not allow poisons and infections to enter the body. Among other things this rule means total abstinence from alcoholic beverages and from the use of tobacco' (p. 371). Fisher was a strong supporter of Prohibition, which would probably have rendered him ineligible for HETSA membership.

Perhaps it is unrelated to this small worry, but I could not help noticing that several of the contributors complain that his writings on health are impressionistic and prejudiced, and in particular that they are distinct from and sometimes inconsistent with his economic analysis. On one point, at least, I think Fisher was right and his critics are wrong: he makes no attempt to apply the principle of impatience to health promotion measures, which often yield benefits only after many years and would be severely devalued by the use of even a modest discount rate. Impatience may be necessary to explain how people actually make inter-temporal decisions but it is not for that reason a social virtue – in small children it is usually regarded as a vice – and arguably it should not be used at all by health economists.

Impatience was one side of Fisher's theory of interest, the other side being variously described as 'opportunity' or 'productivity'. Tobin is not alone among the contributors to this volume in expressing strong support for Fisher as a capital theorist, but none of them really gets to grips with his analysis of the 'rate of return' or its very controversial role in the 1960s capital controversies. It would have been good to invite Robert Solow down from Cambridge (Mass.) to defend Fisher, and to fly Luigi Pasinetti in from Milan (via Cambridge, UK) to criticise him (see Solow 1963; Pasinetti 1969). That, though, might have spoiled the New Haven party.

* Department of Economics and Finance, La Trobe University, Victoria 3086, Australia. Email: j.king@latrobe.edu.au.

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<p>Peter Docherty. <i>Money and Employment: A Study of the Theoretical Implications of Endogenous Money</i>. Cheltenham: Elgar, 2005. Pp. vii + 383. ISBN 1 84004 862 7. £75.00.</p>

J. E. King*

Why is involuntary unemployment greater than zero? Why do not the self-correcting mechanisms identified by pre-Keynesian macroeconomists work? Why is the rate of interest greater than the Wicksellian 'natural' rate? Why is the

'Keynes effect' that is central to the neoclassical synthesis not to be relied upon? Why do the answers to all these questions hinge on the endogeneity of the stock of money? Peter Docherty's excellent book provides a very cogent analysis of all these questions, from a broadly Post Keynesian – or, as he puts it, a 'Kaldor-Moore' – perspective.

After a brief but incisive introduction, Docherty turns to the role of money in classical economics, emphasising the work of Smith and Ricardo and the Currency School-Banking School controversy (chapter 2). He concludes, contentiously, that endogenous money was implicit in the arguments of both Schools, and that classical macroeconomics had a distinctive analytical structure in which output and employment were determined separately. Chapter 3 deals with Wicksellian monetary theory, arguing (again controversially) not only that it assumes an exogenous money supply but also that this assumption was central to Wicksell's defence of Say's Law. An earlier version of this chapter was published in *History of Economics Review* (Docherty 1995). In chapter 4 Keynes's monetary theory of output and employment is assessed. Docherty emphasises chapters 17 and 19 of the *General Theory*, which demonstrate, he suggests, that Keynes proved unable to insulate his theory of involuntary unemployment from the neoclassical claim that it could always in principle be eliminated by downward price flexibility: deflation would increase the real value of the money stock and thereby reduce the rate of interest (this is the so-called 'Keynes effect'). Docherty's critique of Keynes carries the further implication that the three axioms identified by Paul Davidson (1994) as the core of Keynes's analytical system are not in fact sufficient to refute Say's Law; they need to be reinforced by an explicit theory of endogenous money.

Chapters 5 and 6 are devoted to the monetary writings of Nicholas Kaldor. Docherty plays close attention to the relatively neglected 1939 article on 'Speculation and Economic Stability', maintaining that Kaldor's views on monetary theory display an essential continuity from this early foray right down to his critique of monetarism in the 1970s and 1980s (see Rochon 2000 for an opposing view). In chapter 7 Docherty provides a detailed exposition and appraisal of Basil Moore's work on monetary theory, which extends Kaldor's analysis by incorporating in it a formal model of bank behaviour. Docherty has reservations about certain aspects of Moore's work, and devotes chapter 8 to a critical assessment of the Kaldor-Moore approach. Suitably revised, he claims, it powerfully reinforces Keynes's theory of output and employment by showing how interest rates may remain above the Wicksellian natural rate. 'Unlike Keynes's version of the liquidity preference theory, this approach is not susceptible to the Keynes effect and so provides the foundation for a long-run explanation of unemployment'. The Kaldor-Moore approach also offers a more satisfactory account than Keynes was able to provide of the way in which investment is financed. 'The result is a causality which runs from investment to saving and so furnishes a genuine monetary explanation of long-run unemployment' (p. 247).

The next two chapters are analytical rather than historical. In chapter 9 Docherty sets out his own model of the banking sector, which is complemented in chapter 10 by an 'endogenous money macroeconomic model' that lives up to its name, its 86 equations being pleasingly devoid of 'microfoundations'. There is a short conclusion that contains some intriguing hints about the implications of the Kaldor-Moore approach for monetary policy.

This book represents a very considerable achievement. It is clearly written, displays very substantial scholarship, and makes a strong case for the Kaldor-

Moore approach, suitably modified, as the analytical core of a genuinely non-neoclassical Keynesian macroeconomics. It is strongly recommended to anyone with an interest in monetary theory and monetary policy, today and in the history of economic thought.

* Department of Economics and Finance, La Trobe University, Victoria 3086, Australia. Email: j.king@latrobe.edu.au.

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<p>A. B. Trigg. <i>Marxian Reproduction Schema: Money and Aggregate Demand in a Capitalist Economy</i>. Abingdon, UK and New York: Routledge, 2006. Pp. xii + 130. ISBN 0 415 33669 4. £60.00.</p>

J. E. King*

The title accurately describes the subject matter of this slim volume, which deals with the role of money, aggregate demand and the Keynesian multiplier in the context of a multi-sector model of expanded reproduction of the type first set out by Marx in volume II of *Capital*. To a considerable extent the book is derived from Andrew Trigg's previously published articles, including one from this journal (Trigg 2000). It is commendably succinct, in small format and with only 101 pages of text (the remaining 29 pages consist of notes, references, index and seven mathematical appendices). Trigg writes very clearly, taking care to explain the historical background from which crucial economic ideas emerged (though there is also a great deal of algebra).

In several respects the arguments are genuinely original and important. This is true, first, of Trigg's formal analysis (in chapter 2) of the multiplier in a Marxian theoretical framework, which follows up the tantalising hints thrown out by Marx himself in volume II. Trigg's second major contribution (in chapter 3) is his application of the 'Kalecki principle' – in aggregate, capitalists get what they spend – to two-, three- and multi-sector economies of the type that Marx introduced in his reproduction schema[ta]. This offers a potential opportunity (which Trigg does not fully exploit) to rebut Ian Steedman's (1992) Sraffian objections to Kaleckian modelling. Third, in chapter 4 he reinterprets the theory of the monetary circuit from a Marx-Kalecki perspective, putting some real flesh on what sometimes seem to be the very bare bones of the Graziani circuit model.

Subsequent chapters offer somewhat less novel, but always very interesting, applications of the underlying arguments to other important topics in Marxian-Kaleckian political economy, including the theory of crisis, in which the

role of money is (quite correctly) emphasised, and Evsey Domar's version of the Harrod-Domar growth model (both in chapter 5). Domar, incidentally, seems to be something of a black hole – or perhaps another unexploited opportunity – in the history of economic thought. Trigg also deals perceptively with underconsumptionism, concentrating on the ideas of Rosa Luxemburg (chapter 6); the falling rate of profit, where he attempts to graft Keynesian elements onto Henryk Grossman's well-known numerical simulations (chapter 7); and the transformation problem, on which he endorses the 'new approach' of Duncan Foley and Alain Lipietz, now more than 20 years old (chapter 8).

Trigg will be criticised by some Marxists for having nothing to say about the newer 'new approach' to the transformation problem taken by Alan Freeman, Andrew Kliman and others over the past decade. I was a little surprised by the lack of any reference to Hyman Minsky, whose financial instability thesis would seem to dovetail neatly with the 'paradox of borrowing' and the associated potential for macroeconomic instability that Trigg stresses throughout the book. And I would have been interested in his reaction to the Steindl-Harcourt criticism of the Kalecki principle: if workers (in addition to capitalists) make discretionary expenditure decisions these days, does the principle apply also to them? Is working-class borrowing an extra source of instability? How would this affect the algebra?

These, though, are minor problems. This is an important book, which belongs in every academic library.

* Department of Economics and Finance, La Trobe University, Victoria 3086, Australia. Email: j.king@latrobe.edu.au.

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<p>Howard R. Vane and Chris Mulhearn. <i>The Nobel Memorial Laureates in Economics: An Introduction to their Careers and Main Published Works</i>. Cheltenham: Elgar, 2005. Pp. 362. ISBN 1 84376 600 0 (hb.). £75.00.</p>

John Lodewijks*

I came to this book with some apprehension. I had been teaching students about the Nobel Memorial Prize Winners in Economics under the guise of an 'Economic Reasoning' course for several years and doubted that these short vignettes or pen-portraits would add anything I did not already know. I was pleasantly surprised to the contrary. The authors lament the disappearance of history of economic thought courses from the training of economists and provide these brief portraits so that students at least get some minimal understanding of the recent history of economic ideas. This mirrored my own teaching objectives.

Mark Blaug contributes an excellent foreword and in typical combative style claims that 'any notion that the Prize is awarded conspiratorially by a small elite is just nonsense' but notes that the Award 'implies invidious comparisons that are extremely hurtful to the near-winners' and some of the decisions were 'deplorable' in his judgment. The authors provide a very useful 'Biographical Overview' of the 55 economists awarded the Prize over the 1969-2004 period that is covered in this book. Of these economists, 65 per cent have been US citizens, 75 per cent had American university affiliation and 65 per cent received their Ph.D. in America. Other correlates of success are that 39 per cent of John Bates Clark medal winners go on to win the Nobel, roughly one in four Nobel Memorial Prize winners are associated with the University of Chicago, and only 14 universities account for 75 per cent of the winners. We might also note that 100 per cent of the winners have been male.

What follows is a necessarily selective coverage of the economists profiled. The first thing that caught my attention was how extraordinarily productive Ragnar Frisch had been in his academic career; even the more remarkable when much of his wide-ranging work was never published in English. Perhaps similarly, because the work of Maurice Allais was published in French, it has not received the acclaim it deserved. The authors state that Paul Samuelson's 'place as one of the greatest theorists who has ever lived is steadfastly assured. He had contributed fundamental insights in nearly every major area of economic theory and has raised the level of scientific and mathematical analysis in economics'. I am reminded of the story Martin Bronfenbrenner told me of his postgraduate studies at the University of Chicago. One day he went up to Jacob Viner and said he was quitting the program. There was this undergraduate sitting in on his classes who was so much brighter than he was that it was embarrassing for him to continue. Viner told Bronfenbrenner to continue his studies for he could still have a successful future in the profession. There was no need to be overawed by Samuelson because he was one of a kind, simply unique, and was going to leave a lasting impact on the discipline.

John Hicks is labelled by the authors as 'one of the most influential and outstanding economic theorists of the twentieth century', while Friedman is viewed as 'one of the most influential and outstanding economists in the history of the discipline'. The entry on Herbert Simon needs to be highlighted, for it reveals someone who is grossly underappreciated in the profession at large. In addition to Simon's Nobel in Economics, he was awarded some of the top prizes in psychology, political science, computing, artificial intelligence, science and operations research. In this age of academic specialisation, he really stands out as the seminal figure who straddles the various disciplines. The authors rightly say that 'Simon was a truly remarkable and talented individual' who 'held professorships in political science, administration, psychology and computer science'. I once had a colleague who was an expert on the economics of rural water resources. When the Head of Department approached him with a proposal that he also lecture on urban water issues he was aghast at the suggestion and said he couldn't possibly stretch himself that far!

The entry on James Tobin repeats the claim that he was 'the greatest macroeconomist of his generation'. The Stigler piece is interesting because it demonstrates Stigler's important empirical contributions, which seem to have been lost sight of in recent critiques of his work. The statement from Modigliani that a market economy '*needs* to be stabilized, *can* be stabilized, and therefore *should* be

stabilized by appropriate monetary and fiscal policies' stands in sharp contrast to the views of later Laureates like Lucas, Prescott and Kydland. Lucas is labelled as 'one of the most influential theorists of modern times'. Solow's entry concludes that he is a 'much-admired, respected and well-liked individual among fellow economists'. We are told that Trygve Haavelmo was a 'modest, even shy man who refused many of the honours that were offered to him'. This stands in sharp contrast to the efforts that some Nobel Prize aspirants have gone to in order to try to claim the Prize.

Of particular interest to an Australian audience is the account of John Harsanyi's early life. Following the German occupation of Hungary in 1944 he was able to escape on the way to an Austrian concentration camp and survived the rest of the war in hiding. In 1946 he was forced to resign his academic post in Hungary because of his anti-Marxist political views and fled the country illegally. Migrating to Australia in 1950, he studied economics at the University of Sydney at night and did factory work during the day. Later he won a Rockefeller Fellowship to Stanford University and undertook a Ph.D. under the supervision of Ken Arrow. (This story is remarkably similar to the experiences of Jan Kmenta.)

The entry on Becker notes that Jacob Viner thought that Becker was the 'best student I ever had', and Viner of course also taught Friedman and Stigler. When it comes to James Mirrlees the authors repeat the implausible claim that 'Mirrlees has had as profound an impact on microeconomics as Albert Einstein had on physics'. The profile of William Vickery is full of surprises in terms of the innovativeness of his policy proposals. One gains a real appreciation of his wide-ranging contributions from this entry. In contrast, the contribution of Robert Merton and Myron Scholes in devising a 'partial differential equation that changed the world' will always be clouded by the Long-Term Capital Management debacle. A number of the Laureates had unfortunate experiences when encountering a certain Mrs Robinson. Amartya Sen's thesis supervisor was 'the totally brilliant but vigorously intolerant' Joan Robinson, while Joseph Stiglitz had a 'tumultuous relationship' with her as his tutor. Stiglitz turned to economics as it allowed him to apply his 'mathematics to important social problems' and 'combine my interest in history and writing. I wanted it all and economics seemed to have it all'. The entry concludes by noting Kenneth Rogoff's 'splenetic attack' on Stiglitz, which makes some of the notorious HETSA exchanges appear like polite conversation.

The work of James Heckman stands out in this collection. I had not before appreciated the depth of his microeconometrics applied to discrimination, education and a myriad of other labour market issues. Similarly, Daniel McFadden states that he has a 'strong appreciation for elegant and innovative mathematics and statistics, but as a matter of scientific priority try to keep my research focused on concrete applications'. McFadden donated his Nobel Prize money to charity. The last entry in our selective coverage is on Edward Prescott, who believes that business cycles or economic fluctuations are 'optimal responses to uncertainty in the rate of technological change' and should not be construed as forms of aberrant behaviour in need of correction by government. Discretionary policy should not be used but instead there must be a rules-based approach to macroeconomic governance. Isn't this where Keynes stepped in seventy years ago?

In reading these entries two things in particular stand out: firstly, the mathematical background of many of the Prize winners and their view of economics 'as a soft science in need of hardening'; secondly, the ability of Vane and Mulhearn to explain clearly and simply the contributions of the economists

covered. This is particularly evident in the coverage of the three financial Laureates (Markowitz, Miller and Sharpe), the economic historians (Fogel and North) and the behaviouralist (Kahneman). Concepts like the Coase theorem, option-pricing, selection bias, non-stationary time series and the Mundell-Fleming model are masterfully explained. This collection has the capacity to surprise the reader. You learn all sorts of new and sometimes admirable things about these economists and about the richness of the profession that is often obscured from students of the subject.

* School of Economics, University of New South Wales, Sydney NSW 2052, Australia. Email: j.lodewijks@unsw.edu.au.

Laurent, J. (ed.). *Henry George's Legacy in Economic Thought*. Cheltenham: Elgar, 2005. Pp. 271. ISBN 1 84376 885 2 (hb.). £58.50.

Gregory Moore*

In our rather mediocre age the edited book of essays is correctly held at a substantial discount for being the repository of half-baked papers that have been summarily rejected from the refereed journals or, what is arguably worse, the product of ill-conceived invitations to acquaintances of the editor. This decline in the fortunes of the collection of essays has been facilitated by the predilection of once reputable publishers to pursue very nearly every objective except scholarship and, at least in Australia, government incentive schemes in which rather ordinary academics are rewarded for the quantity rather than the quality of their research output. It is therefore refreshing to come across a collection of essays on Henry George's legacy in economic thought that is worth reading. It cannot be argued that the essays constitute a landmark development in the field – that would be asking too much – but scholarly standards have been maintained and each contributor has made a solid contribution to the literature. I derived knowledge from each of the essays contained in this book. Every good research library should own a copy of this publication.

The opening essay by the editor of the collection, John Laurent, is a splendid introductory account of the key features and applications of Georgist thought. The traditional editorial duty of providing the context for each of the essays that follow is performed with some skill, while the well-worn Georgist notion (or should I say neo-Millite notion?) of imposing a single tax on the unearned increment in land rents is presented with the minimum of fuss. Non-Australian readers may, however, be somewhat bemused by the fact that most of the historical illustrations of the application of Georgist thought are derived from Laurent's place of residence, Queensland. In fact, the publisher perhaps should have warned those international readers who are more interested in George than Australia – possibly via a suitable sub-title to the publication – that there are strong Antipodean themes in both this and a number of the other essays contained in this collection.

Erin McLaughlin-Jenkins next provides an entertaining essay on T.H. Huxley's much neglected series of articles that appeared in 1890 in *The*

Nineteenth Century. Darwin's bulldog (or 'dragon' as McLaughlin-Jenkins would have it) used these articles to assail second-rate utopian philosophies and, in particular, what he called George's irksome bit of 'fudge'. He declared that Georgist thought was 'More damner nonsense than poor Rousseau's blether'. It is always a pleasure to read such virile excesses, even if second-hand, and it reminds one just why so many Victorians feared meeting the formidable Huxley in an 'intellectual' dark alley. McLaughlin-Jenkins, of course, finds no difficulty in highlighting the more ill-conceived of Huxley's arguments and attributes these violent exercises in intellectual pugilism to Huxley's ailing health and his fear that individuals without scientific or scholarly credentials, especially those from the left, were gaining a popular audience. The essay acts as a valuable extension of Roy Douglas's earlier 1979 paper on the same subject that appeared in R.V. Andelson's (ed.) *Critics of Henry George: A Centenary Appraisal of their Structures on Progress and Poverty* (Associated University Presses).

In the third essay Rob Knowles presents a very precise and well-crafted account of the differences between the positions adopted by George and Leo Tolstoy on the single-tax scheme. As is well known, George induced Tolstoy to support a single-tax policy, even though the latter mistrusted the state's capacity to use the tax revenue wisely and even though his true goal was an agrarian utopia of peasants owning the land in common. Knowles points out that, although Tolstoy and George were driven by similar Christian ethics, they were reacting to different socio-economic systems and hence were striving to reach different objectives. Specifically, Tolstoy was confronted with the static totalitarian-cum-feudal system of Russia, whereas George was initially faced with the evolving capitalism of the United States. Tolstoy therefore saw the single-tax policy as no more than an expedient first step towards the emancipation of the Russian peasant from the tyranny of the idle landowner, not, like George, as a final policy that would solve all of the world's ills. Knowles builds upon Kenneth Wenzel's writings on the links between Tolstoy and George (such as his 1997 piece in the Georgist-leaning *American Journal of Economics and Sociology*), and, with this and other papers, demonstrates that he was more than worthy of the HETSA prize.

Laurent's second essay in the collection is concerned with the extent to which George drew upon the various evolutionary theories that were then prominent (whether they be associated with Lamarck, Wallace, Darwin, Spencer or Bagehot) as well as the precise nature of George's own evolutionary beliefs. A large proportion of the essay is devoted to tracing the influence of Herbert Spencer's evolutionary ideas on George. This is not surprising, given that George himself devoted an entire monograph to Spencer's renunciation of his youthful support for land nationalisation, namely, *A Perplexed Philosopher, Being an Examination of Mr. Herbert Spencer's Various Utterances on the Land Question, with Some Incidental References to his Synthetic Philosophy* (1893). Laurent's key conclusion, which is nestled within an interesting survey of recent literature devoted to evolutionary economics, is that George believed that the human race progressed via social evolution rather than biological evolution. George believed that the biological constitution of 'man' had remained largely unchanged over the millennia and that progress has instead been executed via the transmission of customs, habits and traditions from one generation to the next.

The fifth essay in the collection, penned by Warren Samuels, Kirk Johnson and Marianne Johnson, concerns the debates between George and G.D. Campbell, the 8th Duke of Argyll, over the issue of land ownership. The essay begins with a

rather long account of the economic theory of rent and the institutional framework of land ownership. This introductory analysis is often insightful, but dominates too much space, tries to cover too much ground and occasionally presents the sort of rather tired historical details upon which our more earnest honours students dwell. The editor should have taken a stronger editorial hand to this essay, which is easily the longest in the collection. The reader is, however, more than rewarded when Samuels *et al.* move on to the Argyll-George debates. Historians of thought will be particularly interested in this section of the essay, since, to my knowledge, the Duke of Argyll has not been the subject of a full-length study from an economist's perspective, even though he played a dominant role in the late Victorian debates over land nationalisation and peasant proprietorships. The essay should be read if only for this reason.

Laurence Moss, the current editor of the *American Journal of Economics and Sociology*, next provides an account of the George-Hotelling-Vickery-Stiglitz Theorem, otherwise known as the Henry George Theorem. This theorem posits a solution to the public goods problem that does not entail government intervention, namely, that the costs of a public good may be funded indirectly by the improvements in the surrounding land rents that arise from the construction of the public good. Moss is as precise as he is lucid in his exemplification of this theorem and concludes that the provision of public goods, by real estate developers and others, is one of the most stunning accomplishments of private entrepreneurs in the postwar US economy. It is, indeed, one of the mysteries of our discipline that the Henry George Theorem has yet to be integrated into the first-year texts (or, for that matter, those horrible little manuals on 'How to Make a Million Dollars from Real Estate'). The theorem is, after all, not rocket science, but, as Moss is inclined to say, a brute fact.

The seventh essay in this collection is John Pullen's provocative piece on the subtle distinction between private ownership and private possession within Georgist thought that appeared in 2001 in the *American Journal of Economics and Sociology*, where it elicited not one, but three rejoinders. (Although strangely none of these academic ripostes is cited in this collection.) It seems that Pullen, like an old and reliable Clydesdale, has moved from the top paddock of Malthusian studies to the bottom paddock of Georgist thought without missing a graceful stride. He investigates the extent to which the taxation of the annual rent of land amounts to the nationalisation of land by default. According to Georgists, the confiscation of the rent, rather than the land, effectively transforms the owners from private proprietors into private possessors. Possession, like ownership, provides the security of tenure required for the possessors to undertake capital improvements, but, due to the land tax, does not give them the right to the unearned increment derived from the growth in land rent. Pullen believes that this is largely a play on words: the landlord is still effectively the owner, only now with the condition that he/she does not have the right to the rent stream. It is private property with restrictions. He concludes that George made a tactical error in failing to make this clear, as it would have made the single-tax policy more palatable.

Pullen's second essay in this collection is devoted to analysing the potential problems with implementing George's single-tax scheme. There is no space to consider these problems here. All that needs to be stated is that Pullen canvasses the associated complex, head-hurting issues with such lucidness that the current reviewer could understand the arguments with ease. Knotty issues ranging from the Lockean interpretation of property rights to the 'land-rich-and-income-poor' problem are

presented in an unadorned, but thoughtful, narrative. It is also refreshing to see a collection devoted to Georgist thought that includes an essay that directly and objectively confronts its potential pitfalls. The collection thereby at once ceases to be either a Georgist apologetic or an anti-Georgist text that dismisses the single-tax scheme without sufficient justification. Every undergraduate studying Georgist thought (and indeed every self-proclaimed neo-Georgist) should read this essay.

The ninth essay in this collection, by Terry Dwyer, concerns the way in which George's non-utilitarian, natural rights-based approach to considering economic issues may be employed to gain insights into the appropriate means by which to regulate infrastructure monopolies. Dwyer displays a clear distaste for the neoclassical economist's current tendency to advocate the light-handed regulation of monopolies that are won via a tender process. Although I was a little discomforted by the strident certainty with which Dwyer occasionally concluded his arguments about the almost impossible-to-solve policy issue of overseeing monopolies, his narrative rhythm is strong and his arguments are informed. The greatest compliment I can pay to Dwyer's essay is that I immediately integrated a number of his insights into my undergraduate lecture notes on the regulation of monopolies.

In the penultimate essay Frank Stilwell and Kirrily Jordan advocate the introduction of a neo-Georgist land tax in Australia as a solution to almost every social ill, whether it be housing speculation in Sydney or Australia's Byzantine tax system or the destruction of the environment or economic recessions. I found this exercise in advocacy a little too self-confident for my liking. Even though Stilwell and Jordan consider some of the early (rather feeble) criticism of the Georgist approach, they do not tackle its central problems. They make a single reference to the paper earlier in the volume in which Pullen confronts the failings of the Georgist programme, but they patently chose not to allow any of these considerations to delay the delivery of their policy advice. Still, it is a credit to the authors that, to my own consternation, I found a few of the arguments contained in this essay not only reasonable, but persuasive.

Philip Day's concluding essay maps out the enduring legacy of George in Australia. Day illustrates the powerful impact of Georgist thought in shaping policy in Australia, but predicts that its future influence is somewhat bleak given the schism-prone nature of the Georgist movement, not to mention the naivety of some of its members in judging what is required to win over sceptics. Day, by contrast, is clearly a realist in these matters and provides a more nuanced account of the worth of the Georgist ideals. This, however, simply makes his final, sweeping judgement all the more bewildering. He states that a 'powerful case can be argued that virtually all the symptoms of the widespread malaise lamented by the social commentators – [a long list of social ills follows] – derive from cultural acceptance of the unearned private exploitation of the community's natural resources in the ruthless pursuit of economic efficiency at the expense of social efficiency' (p. 261). Although I am certainly more sympathetic to Georgist thought after reading this book, I could never imagine a time in which I could articulate Georgist sentiments such as these. If only the source of the many social problems in our ineffable world was so easily identifiable!

* College of Business, University of Notre Dame – Australia, PO Box 1225, Fremantle WA 6959, Australia. Email: gmoore@nd.edu.au.

Peart, S.J. and D.M. Levy. *The Vanity of the Philosopher*. Ann Arbor: University of Michigan Press, 2005. Pp. xvi + 323. ISBN 0 472 11496 4. US\$40.00.

Gregory Moore*

Sandra Peart's and David Levy's *The Vanity of the Philosopher* (2005) may be interpreted as the latest product of a research project that first crystallised with Levy's *How the Dismal Science got its Name: Classical Economics and the Ur-text of Racial Politics* (2001) and that Peart and Levy subsequently promoted on www.econlib.org under the title of *The Secret History of Dismal Science* (2001-2). The two authors further developed this research project in papers delivered before the numerous societies that our North American colleagues are inclined to attend (and the usual institutional suspects may be rounded up here), as well as in articles published in most of the chief outlets devoted to our sub-discipline, such as *JHET*, *EJPE*, *EJHET*, *HOPE* and *JEM*. It is, indeed, an understatement to assert that Peart and Levy have ridden this research project hard, and, given the bewildering number of related documents that these authors have since posted to various web-pages, it seems likely that they will push its (tiring?) legs a little further. It is also apparent from the way in which this research has unfolded – which itself reflects the tremendous changes that have recently taken place in the way academics disseminate their ideas – that a number of the chapters that constitute the book under review have already appeared in the public arena. Readers of *The Vanity of the Philosopher* should therefore be warned that they are likely to come across passages that they have previously perused in different forms and forums. They should also be prepared for the kind of repetition and the occasional breaks in narrative rhythm that usually arise when a number of stand-alone essays are laced together within the stiff boards of a book.

I can, for all this, declare that all good university libraries should add this publication to their collections and, further, that all historians of economic thought who are serious about their craft should consider purchasing a personal copy. For, although the writings of Peart and Levy are characterised by repetition, a tendency towards over-statement, a certain selectiveness in the deployment of quotations, an occasional lack of finesse in closing an argument and sometimes insufficient corroboration of rather grand conjectures, it is also the case that they contain a driving hypothesis that has merit and, what is more striking, has left a mark on our sub-discipline. This central hypothesis, which presumably has reached its most precise and considered articulation in the book currently under review, is that the classical economists' belief in human homogeneity and their presumption that humans have the same capacity to make rational decisions (a presumption that the authors call analytical egalitarianism) was challenged in the middle of the nineteenth century by a number of individuals who believed in human difference and who presumed that humans have different capacities to make appropriate decisions (a presumption that the authors call analytical hierarchism). The individuals who laid down this challenge, and hence who championed analytical hierarchism, were initially the Victorian sages, such as Thomas Carlyle and John Ruskin (who were the main targets in the early Peart and Levy publications), and subsequently a number of racist anthropologists and pioneering eugenicists, such as

James Hunt, William Rathbone Greg and Francis Galton (who are the main targets in more recent Peart and Levy publications, including in the present text).

This driving hypothesis, which is eminently reasonable if not historically accurate, underpins a number of more speculative sub-hypotheses, not all of which, at least in my eyes, necessarily follow from the parent hypothesis and not all of which are delineated here. In fact, I restrict myself to considering what I perceive to be the three main sub-hypotheses that spin out of the parent hypothesis.

First, there is what may be called the ‘vanity of the philosopher’ corollary. The supporters of analytical egalitarianism (that is, the classical economists) attributed any observed differences between individuals to luck and history and, more importantly, were convinced that these individuals had the same potential or ‘educatibility’. As Adam Smith stated: ‘The difference between the most dissimilar characters, between a *philosopher* and a common *street porter*, for example, seems to arise not so much from nature, as from habit, custom and education’ (quoted in Peart and Levy 2005, p. 3). The supporters of analytical hierarchism, by contrast, attributed any observed differences between individuals to their innately dissimilar natures, with some natures being distinctly inferior to others. Peart and Levy use this dichotomy to conclude that the supporters of analytical egalitarianism did not see themselves as superior to the individuals they were studying, while the supporters of analytical hierarchism did indeed see themselves as superior in this way. The former, as *philosophers*, believed that they were no better than the *street porter* and hence at least baulked at presuming that they were better placed to second-guess the *street porter’s* preferences, while the latter, as *experts*, saw themselves as not only better than the inferior *street porter*, but better placed to make choices on the *street porter’s* behalf. The so-called experts, in short, suffered from what Peart and Levy call the ‘Vanity of the Philosopher’, which explains the title of the book under review, and paternalistic policies are interpreted as being a product of this sense of superiority.

Second, there is what may be called the ‘post-classical analytical hierarchism’ corollary. Peart and Levy argue that the late-Victorian economists who succeeded the classical economists rejected analytical egalitarianism for analytical hierarchism. They deliberately use the phrase post-classical in this context, since they maintain that the movement from a belief in human homogeneity to human difference was by no means confined to the neoclassical economists. In their words: ‘our argument in what follows is that economics moved from a doctrine of analytical egalitarianism in the classical period to one of analytical hierarchy in the postclassical period’ (*ibid.*, p. 5). And again: ‘this chapter has demonstrated that, for a time at least, the classical economists’ postulate of homogeneity was overthrown, and racial theories prevailed in economics’ (*ibid.*, p. 84). Peart and Levy believe that the ideas of the early eugenicists, such as the aforementioned Greg and Galton, were particularly influential in inducing the post-classical economists to support analytical hierarchism. They further maintain that the concepts of human homogeneity and equal competence were resurrected as the bedrock of economics only once Lionel Robbins began to exert influence at the LSE and the Chicago school came to the fore in the 1950s.

Third, there is what may be called the ‘sympathy’ corollary. Peart and Levy argue that the classical economists, as supporters of analytical egalitarianism,

viewed all individuals as worthy of sympathy, while the post-classical economists, as supporters of analytical hierarchism, viewed those individuals who they believed were innately inferior as sufficiently unfit and parasitical to be unworthy of sympathy. Peart and Levy further argue that, when a person is sympathetic to another person, he or she is willing to give up something to that person in return for something else. Thus one person may yield wealth to another person in return for approbation. This, the two authors maintain, plays an important role in driving reform policies that entail the redistribution of wealth, since this redistribution may be presented as the act of trading wealth for non-wealth rewards. They then claim that the shifting view of sympathy through time explains why the classical economists, especially John Stuart Mill, took this type of sympathy-driven trading into account in their policy formation, while the post-classical economists did not. Peart and Levy add, as a rider, that sympathy was strangely not resurrected in our discipline with the resurgence of analytical egalitarianism that accompanied the rise of the Chicago school.

Now, because the present narrative is no more than a book review, it is not possible to critically assess these three inter-linking ideas in any detail. I certainly do not have the space to convey the richness with which the ideas are presented. There is, for example, no space to consider the interesting way in which Peart and Levy delineate the alliance between the Evangelicals and the classical economists that formed to confront those who supported analytical hierarchism, nor to analyse the skilful way in which the authors deploy illustrations by George Cruikshank and John Tenniel to highlight the prejudices of those who advocated analytical hierarchism. Indeed, the list of the insights and tools that the two authors have deployed with skill to shed light on their research project is endless, and I leave it to the buyer of the book to scrutinise them for him or herself. Instead, with the limited number of words left at my disposal, I wish to take issue with the second sub-hypothesis or what I have called the 'post-classical analytical hierarchism' corollary. The claim that economics moved from a doctrine of analytical egalitarianism in the classical period to one of analytical hierarchy in the post-classical period is, I believe, the least convincing of the arguments presented in the book, or, to be more precise, the argument that the authors push to such a level of over-statement as to warrant questioning. In fact, I would argue that the more appropriate research question that Peart and Levy should ask is why analytical hierarchism had so little impact on post-classical economics, especially the analytical core as opposed to policy applications.

The fact of the matter is that the classical assumption of agent uniformity, as reflected in economic man, was retained as the central tool in the construction of economic theory in the various schools of neoclassical economics (that is, the schools that dominated the post-classical period). If anything, the classical doctrine of human homogeneity was further entrenched in the discipline of economics via the mathematisation of the subject matter that transpired in the Victorian period when Cambridge wranglers from the Mathematical Tripos, French mathematicians and Italian engineers migrated to the study of economics. It is simply more difficult to portray human difference in tight analytical frameworks that defined the post-classical period than in the meandering narratives that defined the classical period. Agents became uniform rolling balls that collided with fixed obstacles in frictionless space. This perhaps explains why the evidence provided by Peart and Levy for the 'hierarchical' infection of the neoclassical schools is so thin. They allude to John Maynard Keynes's well-

known support for eugenics, William Stanley Jevons's predilection to portray the Irish as sub-human, Alfred Marshall's assertion that the lower classes had a different time preference compared to the professional classes and, which I found the most insightful of the observations made by the two authors, F.Y. Edgeworth's twist to utilitarianism on the basis that some individuals have a greater capacity for enjoyment than others. This is all very good, and I would not be surprised if one and all of the post-classical economics were racist to the core. But, at the end of the day, the impact on the theoretical core of economics was virtually nil.

Peart and Levy also have the habit of using the clever rhetorical strategy of devoting the majority of each chapter to outlining the views of racist anthropologists or proto-eugenicists and then punctuating the chapter with one or two pages in which they illustrate the neoclassical economists' use of human difference. In other words, unsuspecting readers are so swept along by the long, scholarly narrative that relates to the non-economists (and it is indeed impressive scholarship) that, when they are finally confronted with the link to economics, they ignore its slender nature. Chapter five, for example, quite astonishingly ends with no more than a three-page account of Bagehot's and Marshall's well-known claim that excessive abstraction in classical economics may be attributed to Ricardo's Jewish heritage. Surely Peart and Levy could muster more support than this to suggest that these scholars were preoccupied with race. Furthermore, much of the evidence that the two authors either could or do muster to support their case is slightly tarnished (but by no means inadmissible) because allusions to human difference in the various neoclassical texts may be attributed to the influence of the many historicist laws of economic development, which were then very popular, rather than to eugenic or Social Darwinism influences. This is important because, within most historicist laws of development, agents are portrayed, in the classical tradition of analytical egalitarianism, as mutable entities who are equally capable, but different due to an accident of time that places them in a certain historically determined institutional setting. They are not different due to innate, insurmountable genetic wiring. It is, in short, often difficult to work backwards, with certainty, from an allusion to difference within a text to either institutional historicism or eugenic theorising.

I strongly advocate that HETSA members read the *Vanity of the Philosopher* for themselves. It is an important publication in our field. As a postscript, some members may be disappointed that relevant articles from our own society's journal are not cited in this text. I here, of course, refer to Terence Hutchison's rather testy exchange with Mike White over the period 1993-1994 on the issue of Jevons's racist attitude to the Irish (see issues 19 and 21) and Peter Groenewegen's 2001 article devoted to Carlyle's attack on the 'Dismal Science' in 'The Nigger Question' (see issue 34).

* College of Business, University of Notre Dame – Australia, PO Box 1225, Fremantle WA 6959, Australia. Email: gmoore@nd.edu.au.

Ann M. Woodall. *What Price the Poor? William Booth, Karl Marx and the London Residuum*. Aldershot, UK and Burlington, VT, US: Ashgate. 2005. Pp. 246. ISBN 0 7546 4203 8 (hb.). £50.00.

Anitra Nelson*

‘Make poverty history’ is only the most recent in a series of ideas and movements that have developed over the last few centuries to address the plight of the poor. Each such movement has its founders, leaders, and some initial sense that their analysis and strategy will succeed in radically diminishing, if not entirely eliminating, poverty. This is probably the most useful context within which to refer to Ann M Woodall’s *What Price the Poor?*, a work that strains to compare the beliefs and activities of William Booth, the founder of the Salvation Army, with the theory and practice of the famous communist thinker, Karl Marx.

Of course, Marx is often referred to as one of the three founding fathers of sociology, along with Max Weber and Emile Durkheim. However, curiously, the latest work in the Ashgate ‘Rethinking Classical Sociology’ series draws its title and central question from quips on salvation and charity by characters in the George Bernard Shaw play *Major Barbara*. The main themes of this series include questioning the canons of traditional sociology from the point of view of newly emerging methods of analysis, such as biography (the personal is the political) and literature, and to challenge theory with practice. Therefore the comparison drawn here, between Booth – Woodall’s real interest – and Marx, purports to be founded on the fact that both developed their ideas substantially as a response to their experiences and understandings of the ‘London residuum’, that is, all those on negligible or nil incomes, the working poor and poor marginalised by incipient capitalism in Britain during the latter half of the nineteenth century. However, the reader is left with a strong sense in which the main argument is really, or by default, that Booth at least did something for the poor while Marx merely wrote theories on their condition. The logic of this argument and the evidence offered are both wanting.

Booth believed in a supernatural force and in his own capacity to lead people to religious salvation and a modicum of material security through charity. In Booth’s mind conversion was tightly linked to social solidarity and material support. Marx doubted that personal efforts could effect the necessary permanent alteration to a collective system of rewards and punishment based on monetary disciplines related to market-based activities (and production for the market). Therefore Marx concentrated his activities on analysis and persuasion, collective and forthright challenges to the capitalist system. The positions of Booth and Marx were starkly different in terms of their interpretations, analyses and recommendations for what needed to be done.

Therefore, it would have made more sense and have been fairer to compare Booth’s activities with his own theories and/or wider and truly comparable religious theoreticians rather than in the light of Marx’s ideas. As it stands, much of the text comprises banal generalisations, qualifications, and modifications because of the discrepancies between the two men’s understandings and responses to the not-quite-same world within which they emerged as agents of social change. In fact, especially from sociological perspectives (given that sociology bases its insights on social relations and interpretations of reality) the dissonance in the

personal histories – the cultural, religious, national, and intellectual backgrounds – of the two men, has clearly made a comparison onerous.

Five of the nine chapters in the book are devoted to Booth, just two to Marx, and the other two refer to both with an emphasis on Booth. The standard of the discussions on Marx is low and concentrates overwhelmingly on what Marx wrote. A comparison of what Marx personally did and what Booth did would have made interesting reading. However, Woodall's knowledge of Marx's personal activities is a fraction of her knowledge of Booth's, and this cannot be explained by lack of primary sources. One can point even just to the English translation of copious works – including letters, journalism and pamphlets – in the Marx-Engels *Collected Works* (50 volumes, 1975-2005, published by Lawrence and Wishart, London, in collaboration with International Publishers, New York, and Progress Publishers, Soviet Union). This collection alone offers considerable material to demonstrate Marx's many social activities, the extent to which he and the rest of his household in varying degrees experienced relative poverty, were involved in assisting the poor (such as refugees), and were active in resistance movements through workers' parties, demonstrations, strikes and unions. Instead, Woodall refers all too often to secondary sources that are notorious for their conflicting assessments of Marx. Also she mentions relations, such as that between Marx and Mikhail Bakunin, in an all too cursory and non-subtle way, to little effect on her arguments. Indeed, both Booth and Marx are discussed by way of a patchwork quilt of religious, sociological and literary reference points that fail to develop into a clear framework or two within which to better understand them.

Indeed, the interpretations, activities and strategies of Booth and Marx are not judged from a point of neutrality but rather the underlying rationale is to laud Booth's practical activity in the light of Marx's purportedly overly theoretical position. For instance, Woodall's third chapter (on Booth) concludes: 'The plight of the residuum also had an impact on Marx's thought just as it did on Booth's although the knowledge was gathered in different ways and led to different solutions being offered, economic theory rather than practical engagement' (p. 87). In fact, Marx had engaged in the world in a practical way, so much so he was exiled for his activities, and yet continued to be active in workers' and scholarly circles as well as to write and publish. Indeed, for Marx the theoretical engagement was a practical one too, especially inasmuch as he challenged popular as well as esoteric economic ideologies/discourses of the social forms of production and exchange.

Finally, in terms of the relevance of this work for the readership of *History of Economics Review*, it is important to point out that the economic data offered are primarily to illustrate the extent and nature of the poverty suffered at the time. There is no rigorous discussion or appraisal of economic data, sources or methods of analysis. The discussions of economists and economic 'facts' are superficial. Again this constrains the assessment of Marx's deep analysis of capitalist society and its social ills. Additionally, there is a strange theme of quality and quantity throughout Woodall's work, with clumsy and half-hearted reference to Hegel and Marx's dialectical concepts of the movement of quality/quantity, and leading to throw-away lines such as: 'The statistics were the important element in Marx's view of macro-redemption' (p. 138).

Furthermore, the work has not been proofread or edited well. There are formatting problems which disturb the flow of the work, such as automatic paragraphs after almost every indented quote. An odd word here and there appears underlined for no obvious reason. There are hyphens in place of dashes, sometimes

abutting the text on one side with a space on the other. A reference (p. 8) to the *Communist Manifesto* does not italicise the title. There are grammatical errors, such as one sentence (p. 7) that begins: 'There were something in the condition....' For these reasons alone one could not set it for students to read.

In summary, the strength of this work lies in its exploration of a theme of enduring interest not only to economists, sociologists, philosophers and other social scientists but also to all of us who wrestle with the meaning of life and weigh up our responsibilities towards others. Its weaknesses lie in the failure to analyse the bases of poverty and enter the discourses on strategies to challenge poverty in more structured, logical and methodologically coherent ways. The editor of the series points out that such a comparison has not been attempted before. Perhaps that is for good reason. The broad discussion in chapter 6, including a short discussion on the Christian Socialists, indicates that a more useful analysis might have been to situate an assessment of Booth's work within the various Christian traditions associated with liberation theology. I suggest that a focussed view of more easily comparable subjects and themes would have improved the standard and utility of the discourse.

* 8 Ada Street, Katoomba, NSW 2780, Australia. Email: anitra.nelson@bigpond.com.

Philip Day. *Land: The Elusive Quest for Social Justice, Taxation Reform and a Sustainable Planetary Environment*. Malden, MA: Blackwell, 2005. Pp. 120. ISBN-13: 978 1 4051 4977 8 (casebound); ISBN-10: 1 4051 4977 9 (casebound); ISBN-13: 978 1 4051 4978 5 (pb.); ISBN-10: 1 4051 4978 7 (pb.). Price not stated.

John Pullen*

This work is one of a series entitled Studies in Economic Reform and Social Justice, edited by Laurence S. Moss, of Babson College. Professor Moss, who edits the *American Journal of Economics and Sociology*, has provided a Foreword to this work. It is an augmented version of a work with the same title, published in 1995 by Australian Academic Press.

As explained in an appended biographical sketch, Day has had a wide experience in town planning and local government. Graduating in law from the University of Queensland after war service, he entered the Commonwealth Public Service before studying town and country planning at Sydney University and joining the NSW Department of Local Government, where he was involved in town planning appeals throughout NSW. He later worked for the NSW Department of Decentralisation and Development at a time when the interventionist Australian government under Prime Minister Whitlam was vigorously promoting growth centres and land commissions. He was the main author of two major reports of the NSW government on decentralisation.

This was followed by an appointment as Director of Town Planning with the Brisbane City Council, succeeded by a position with the Department of

Regional and Town Planning at the University of Queensland, where he later became Head. He served two years as Director of the Australian Institute of Urban Studies and has edited the journal (*Queensland Planners*) of the Royal Australian Planning Institute for 16 years. His previous publications include *Planning and Development: The Philosophy and Practice of Development Contributions* (1982) and *Land Value Capture* (1992), as well as numerous media and journal articles.

This invaluable experience over 50 years has enabled Day's *Land* to incorporate a challenging set of observations and recommendations on town planning and land taxation. From the point of view of historians of economics, the main interest is the fact that its central theme is Henry George. It provides a lucid summary of the concept of unearned increment (adopted by George from John Stuart Mill), and of the arguments for land value taxation. As the title indicates, the emphasis and rationale are as much ethical as economic. It shows how modern circumstances – in particular, the enormous increases in land value arising from rezonings and from the beneficial effects of taxpayer-funded public works – have combined to reinforce ever more strongly today the case made by Henry George for public recoupment of the unearned increment.

However, from the perspective of the historian of economic thought, the work has an added fascination, because Day, despite adhering to the basic Georgist view on unearned increments, is prepared to differ from the standard Georgist position on some significant issues, so much so that his position might well be described as Neo-Georgist, although Day does not employ that term. Day's *Land* is therefore a classic example of a Kuhnian 'paradigm shift' in the history of an idea, as he moves away from strict adherence to the conventional wisdom of the Georgist 'scientific community'. It remains to be seen whether readers will interpret the shift as a Kuhnian revolution away from George, or as a Neo-Georgist variation on a theme by George.

On the central Georgist theme that land value taxation will be the 'single tax' that provides sufficient revenue for government, Day says that 'the notion of land rental sufficing as the sole source of public revenue or "single tax" is inapt and misconceived' (p. 49) and 'is an unnecessary over-simplification' (p. 79). In mitigation, Day adds that this over-simplification was 'more commonly propagated subsequently by others than by George himself' (p. 79), but I am not so sure. George may have been persuaded by others to accept the 'single tax' idea, or he may have invented it himself, or he may have been equally involved in the evolution of the concept – the documentary evidence is inconclusive – but there is no doubt that he enthusiastically adopted the idea. It became an influential political slogan that raised tremendous support through his writings and public lectures. The 'single tax' term or concept was used at most if not all of the 48 lectures and 9 Sunday sermons that he gave in Australia in 1890.

Day at one time was a trustee of the Henry George Foundation of Australia but became disillusioned with the Foundation's 'reiterative endorsement of the single tax' (p. 113). Day also deviates from pure Georgist principles on the question of the contribution made by landowners/developers to the increase in land value. A strict Georgist would argue that the whole of the increase in land value is caused by forces and events that are independent of the landowner – such as the growth of population, the increasing affluence and purchasing power of the community, favourable town planning decisions, and beneficial public works. Day's heretical argument is that 'at least in some circumstances, some *part* of increased land value can be attributed to the quality of development constructed by individual

landholders rather than being *wholly* attributable to public planning decisions or to population growth and general community development' (p. 87; original stress). He develops this point in an end-note where he states that in some circumstances developers do increase the value of the land, and that it is 'prima facie difficult to reconcile this with the Georgist contention that all increases in land value are created by the community....' (p. 92).

Day's book is therefore an absorbing contribution, not only because of its observations on the failures and absurdities of our town planning system, but also as an incisive commentary on the strengths and weaknesses of Georgism, and as a possible catalyst in an evolutionary transition from Georgism to Neo-Georgism.

* School of Economics, University of New England, Armidale NSW 2351, Australia. Email: jpullen@une.edu.au.

Gustav Schachter and Saul Engelbourg. *Cultural Continuity in Advanced Economies: Britain and the U.S. Versus Continental Europe*. Aldershot: Ashgate, 2005. Pp. viii +367. ISBN 0 7546 4476 6. £55.00.

John Singleton*

Students in search of an overview of economic policy in Britain, the United States, France, Germany or Italy over the last three centuries will find a quarry of information in this book. As an attempt at comparative analysis, however, it falls well short of my expectations. There are four main chapters plus a brief introduction and conclusion. Three chapters deal, in turn, with the eighteenth, nineteenth and twentieth centuries. Within these chapters, there is a separate section on each country. The fourth substantive chapter, on railroads and the state in the nineteenth century, also proceeds country by country.

The overall thesis, as set out in the introduction, and restated in the conclusion, is that the United States and Britain have been more inclined than the continental European countries to rely on market forces. France, Germany and Italy have tended towards state intervention and collectivism. This is what the authors evidently mean by cultural continuity. As a condensed description of the economic role of the state in the Anglo-Saxon and continental worlds, this approach has some merit. But, instead of a sustained and convincing analysis, Schachter and Engelbourg offer a narrative history of policy-making in each country. They are singularly coy as to what they really mean by culture and values. Their argument boils down to little more than the observation that the Anglo-Saxons and continentals have different ways of thinking about and doing things. This is very unsatisfactory.

What surprised me most about this book was the bibliography. There is no reference to the work of some leading scholars of long-run economic development such as North and Thomas. When discussing economic culture, it would have been worthwhile referring to the research of, say, Mark Casson. Although I claim no special expertise in the economic history of the United States, France, Germany or

Italy, I do feel qualified to comment on Schachter and Engelbourg's treatment of Britain. They rely rather heavily on the scholarship of a much older generation of economic historians, including Ashton, Clapham and Court. The work of Crafts and Harley on the industrial revolution is ignored. Pollard (admittedly not of the current generation) is cited as the joint author of a book on the history of shipbuilding, but not as one of the leading authorities on European and British economic history. No space is found to discuss, or even acknowledge, the extensive work of Middleton on British economic policy in the twentieth century.

Some very odd footnotes caught my eye. On p. 110, Ireland is described as 'totally unlike' England, Scotland, and Wales in the nineteenth century. This deserves a prize for sweeping generalisations. On p. 316, moreover, Thatcherism is described as the 'best twentieth century representation of a Social Darwinist philosophy'. Better than Nazism or fascism? Read as anything more than an introductory survey, this is a simplistic, frustrating, and very disappointing book.

* Faculty of Commerce and Administration, Victoria University of Wellington, PO Box 600, Wellington, New Zealand. Email: John.Singleton@vuw.ac.nz.