

Willie Henderson. *Evaluating Adam Smith*. Albingdon, U.K: Routledge. 2006. Pp. xvii + 164. ISBN 0-415-33668-6. £70.

James E. Alvey*

Willie Henderson has published a short book (145 pages of text) called *Evaluating Adam Smith: Creating the Wealth of Nations*. The subtitle tells us what the book is about: how Smith created, *through his writing*, the canonical work called the *Wealth of Nations* (and, Henderson would add, the discipline called political economy). Smith's carefully constructed texts were designed to be persuasive (2006, pp. 20, 49, 78, 92-108, 110, 137, 142-3) and Henderson is interested in examining how Smith achieved that objective.

Henderson was, until recently, the Professor of Continuing Education and Director of the Centre for Lifelong Learning at the University of Birmingham, UK. In 2006 he became the Director of the Alworth Institute (an international relations research institute established by the Alworth family) at the University of Duluth, in the USA; the shift to this new job reflects Henderson's interest in another area, namely, economic development (especially in Africa). There is some influence on the content of the book, however, from his former occupation in education/pedagogy: Smith's *Wealth of Nations* is, at least some of the time, discussed as if it were a textbook (2006, pp. 80, 82, 117-18; see also p. 108). I doubt, however, that Smith designed it as a 'textbook'. It is clear that some of the *Wealth of Nations* was designed to be a policy manifesto.

In the book under review there is a short preface and eight 'chapters' (the average chapter length is just over 18 pages). I used inverted commas because the units of the book are only slightly connected papers. Chapter 1 serves to introduce the work; it is here that some attempt is made to connect the chapters together. Chapter 2 considers the sources of Smith's ideas disclosed in Chapter 2 of Book I of the *Wealth of Nations*. Chapter 3 discusses Smith's possible use of irony in some passages in the two books that Smith authorised for publication: the *Wealth of Nations* and the *Theory of Moral Sentiments*. Chapter 4 focuses on the work of Maria Edgeworth, the novelist, and tries to show how this early reader seems to have 'read', or interpreted, Smith's *Wealth of Nations*; it tells us little new about Smith. Chapters 5 and 6 can be seen as a pair. Chapter 5 demonstrates Smith's use of examples in the first three chapters in Book I of his economic *magnum opus*. Chapter 6 returns to the first of these chapters from Book I, but this time it is examined from the perspective of Smith's use of hedging (such as the use of caveats and equivocations). The final two chapters are another pair. Chapter 7 shows how Smith built up a persuasive argument in Chapter 1 of Book III of the *Wealth of Nations*; Chapter 8 moves on to consider Chapter 2 of Book III from various perspectives, with the aim of showing how Smith develops a persuasive story.

Henderson's book does mention a range of Smith's writings but it focuses heavily on the *Wealth of Nations*; his 'close readings' focusing on Chapters 1-3 of Book I and Chapters 1-2 of Book III. As Henderson's is such a short book (books published these days tend to have about 200 pages of text), it could have been improved by adding two more chapters. The first chapter could have been devoted to Book IV of the *Wealth of Nations* (rather than considering it in just a few lines on pages 80-1), which is a highly charged attack on mercantilism; the second chapter could have addressed Book V, where Smith's discussion of public policy is

concentrated. Further, in a more general book on Smith's writing, the *Theory of Moral Sentiments* (see Chapter 3) and the *Essays on Philosophical Subjects* could have been discussed more thoroughly.

The title of Henderson's book is misleading, as there is little discussion, let alone 'evaluation,' of what most people take to be the *substance* of Smith's works. Henderson admits that '[t]he works [i.e., chapters in his book] presented in this monograph are not primarily concerned with evaluating historical or current themes in the interpretation of Smith's writings on economic and related topics' (2006, p. 15). The depth of knowledge of Smith shown by great Smith scholars, like Jerry Evensky, is missing from Henderson's book (see 2006, p. xvii). Nevertheless, on occasion, Henderson does raise, or indirectly allude to, interesting substantive issues. Two examples will be mentioned. He refers to Smith's possibly ironic discussion of the need for inheritance to be based upon a 'plain and evident' principle rather than 'the doubtful distinctions of personal merit' (2006, p. 139 citing Smith 1976, p. 383). Shortly afterwards, Henderson cites Smith's claim that '[t]he male sex is universally preferred to the female' and comments that 'Smith does not seem to notice that he started the paragraph with a different set of "manners and customs" under the natural law of succession' (a law in which the inheritance of property is divided equally) (2006, p. 139 citing Smith 1976, p. 382-3). These are interesting points but the discussion of them is disappointing.

Let us return to what Henderson does attempt to do. He says that his book 'focuses on the analysis of Smith's ways of writing economics and on reading strategies that help to reveal aspects of the writing such as text patterns, hedging, the use of narrative and the use of metaphor and irony' (2006, p. xiv). Later, he adds that:

The concern here is with Smith's writing as writing. ... This is a study that is text-based rather than interpretation based. It is not about encouraging an involvement with the secondary literature on Smith nor is it, primarily, a review of the ways in which that literature has dealt with Smith and his ideas. It is about reading Smith writing on economics and related topics ... (Henderson 2006, p. 25)

What contribution does Henderson's book make? First, what does it contribute as a work on the history of economic thought or the history of ideas? Perhaps Chapter 2 adds a little to the literature by showing some detailed comparisons between Smith's wordings and those of earlier writings of David Hume and Bernard Mandeville on matters raised in Smith's discussion of the division of labour. Second, how does Henderson's book shape up as a work about Smith's rhetoric or writing style? Henderson's book has the flavour of a sampler or introduction (p. xvi). It seems certain to be superseded by a new book on Smith's rhetoric by a professor of rhetoric (McKenna 2006). Third, let us narrow the second field to the 'economics as rhetoric' or 'economics as discourse' approach developed by economists interested in texts as literature. The leader of this school is Deirdre McCloskey. How does Henderson's book shape up in this tradition? I have to be cautious here. Perhaps it should be judged by the insiders in this group of economists who have taken a 'literary turn' (for the list of McCloskey's followers, which includes Arjo Klamer and Henderson, see McCloskey 1998, p. 188). Vivienne Brown is another author associated with a literary and postmodern interpretation of Smith. Brown, through her book and advice, seems to have been very influential on Henderson (see his acknowledgments at the front of his book and in the endnotes). After the passing of time, and having read Henderson's book in the meantime, I now regret some of the severe criticisms that I made against her

book in this journal (Alvey 1997). Apart from being underwhelmed by the content of Henderson's book, I thought that, in a book devoted to Smith's art of writing, a premium should be given to *demonstrating* good writing; Henderson's book requires a lot of copyediting correction.

* Department of Applied and International Economics, Massey University, Palmerston North, New Zealand. Email: J.E.Alvey@massey.ac.nz.

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<p>Massimo M. Augello and Marco E. L. Guidi (eds). <i>Economists in Parliament in the Liberal Age (1848-1920)</i>. Aldershot, UK: Ashgate. 2005. Pp. xvii + 315. ISBN 0-7546-3965-7. US\$100/£55 (cloth).</p>
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Riccardo Faucci*

This book constitutes an international research exercise that expands upon the collective research already completed by Massimo M. Augello and Marco E. L. Guidi on the role of economists in parliament (2002, 2003). Together with a comprehensive illustration of the case of Italian economists in parliamentary forums written by the editors, some well-known specialists in the history of economic thought in different countries illustrate the role of economists in the assemblies of Portugal (A. Almodovar and J. L. Cardoso), Belgium (G. Erreygers and B. Mosselmans), Spain (S. Almenar), Britain (R. Backhouse), France (Y. Breton), Germany (H. Hagemann and M. Roesch), Greece (M. Psalidopoulos and A. Syrmaloglou), Japan (J. Kumagai), and the United States (B. W. Bateman).

As the editors clarify in the first chapter, this kind of research requires the contributors to be competent in a range of disciplines. The various authors must, in particular, be able to present the historical context in which the 'parliamentary' economists operated. This gives rise to the problem of how to define the 'liberal age'. Given the specific objectives of this exercise in institutional history, it may be considered as the age in which the main Western states and Japan adopted a constitution (written or, as in the British case, not written) that established a representative government and determined the separation of powers, the extension of the franchise and, above all, the central role of parliament in political life.

For most European countries this age coincides with the dates in the title of the book under review. This is not, however, the case for all countries. Although 1848 is a crucial date for France, Italy and Germany, in Italy only the kingdom of Sardinia maintained its constitution after 1849 and therefore had a parliament, while in France Napoleon III cannot be considered a 'liberal' monarch, and in Germany the Frankfurt assembly had a short and unhappy life. As far as the other countries are concerned, in Britain a most significant starting point would be 1832 (the year in which Lord Grey extended the suffrage) or rather 1846 (the year in which Britain adopted free trade). In Greece, the liberal age began in 1862 with the deposition of King Otto I of Wittelsbach. In Belgium, the starting point of the liberal age coincided with this country's independence from Holland in 1830. The very troubled political history of nineteenth-century Spain presents a range of alternative dates—1836, 1844, and 1876—any one of which could be considered as valid starting points. Finally, a separate consideration should be made for the two non-European experiences illustrated here. In Japan, the liberal age (with very peculiar features, such as the maintenance of the belief in the divine character of the Emperor) could start in 1881, during the so-called 'Meiji restoration'. In the United States of America, the starting date could coincide with the 1787 Constitution or, alternatively, with the end of the Civil War in 1865. Analogous difficulties are raised by the selection of the finishing date for this period. If 1922 signifies the end of the 'liberal age' for Italy, due to the Fascist rise to power, for most countries the First World War (either the beginning or the end of it) can be considered as more appropriate. For France, whose history is marked by the succession of 'Republics', the proposed finishing date perhaps coincided with the 1929 economic crisis.

A second problem, which is also highlighted by the editors, arises from the difficulty of choosing suitable criteria to determine which 'economists' should be included in the research exercise. Most of the case studies (such as Spain, Britain, and Greece) refer to economic experts in a broad sense. The Italian case study overseen by the two editors, by contrast, considers a narrower category of economists, largely coinciding with academic economists.¹ This restricted sample is also reflected in the earlier research undertaken by Augello and Guidi (2003), where they selected 27 university professors of economics who sat in the parliament between the unification of Italy and Fascism. The choice of such a narrow category of economists may be attributed to the singular trajectory of the discipline of economics in Italy, where the process of institutionalisation began earlier and was more pronounced.² Indeed, this phenomenon was underlined by Joseph Schumpeter (1994, p. 856), who stated that, in Italy after the unification, 'though the worldly means at the disposal of Italian economics were modest, there was a large number of ill-paid professorships'. According to Augello and Guidi (2005, p. 198) this institutionalisation process was due to the huge demand for economists' services. It is more probable, however, that this demand, far from being created by the market, was simply the consequence of the large number of Law courses. In fact, soon after the unification, nobody dared eliminate the smaller universities, frequently of ancient origin, but without a reasonable number of students. Furthermore, the *étatiste* uniformity of the curricula facilitated the proliferation of professorships. The result was that most economics professors who taught in Italy soon after the country's unification were so minor, that Schumpeter (1994, p. 853) could reasonably claim that 'the most benevolent observer could not have paid any compliments to Italian economics in the early 1870s'. As a result,

many of the 'Italian economists in Parliament, 1860-1922' considered by Augello and Guidi (2005, pp. 225-8) are quite unknown.

Also, in contrast to the opinion of the editors, it seems that 'the role played by Parliaments in promoting the [process of] academic institutionalisation of political economy' (2005, p. 21) was not altogether relevant.³ This process was, instead, driven more by the *separation* of economics from politics. The leader of the 'new' wave of economists in Italy, Luigi Cossa (1831-1896), was never a member of parliament. He did not encourage his students to engage in the political struggle, but rather to focus on their academic tasks and thereby follow the German university model. Consequently, very few of the relevant professional economists—defined as those individuals with an appropriate training in economics, who publish scientific papers and so on—who were born between 1850 and 1870 were also MPs. It is true that among those few we can find the likes of Maffeo Pantaleoni (born 1857) and Antonio De Viti de Marco (born 1858). However, it is also true that their parliamentary speeches did not deal with relevant topics in economics.

The obstacles that the economists had to overcome in order to be elected during the Liberal age were also considerable. Francesco Ferrara attempted in vain to be elected in the Piedmont Parliament in the 1850s. Elected in 1867 when he already was finance minister, he was not re-elected in 1880. In 1881 he was appointed a 'lifelong Senator', but thereafter he did not participate in parliamentary life. It is worth noting that, in spite of their efforts, neither Vilfredo Pareto nor Enrico Barone was ever elected.

In fact, the synergies between activities within the Italian parliament and the activities of economists were quite weak during the whole period. The actions needing to be taken to be elected were exclusively political. Candidates needed to be friends of the parliamentary leader of the time, such as Camillo Benso Conte di Cavour, Bettino Ricasoli, Marco Minghetti, Agostino Depretis or Giovanni Giolitti. Furthermore, the economists who had a genuine interest in politics considered the intellectual level of parliament (and the moral level as well) to be very low. This attitude proved to be well grounded when the political-economic scandals of the 1890s (such as the fall of the Banca Romana) came to light. Severe criticism of 'Parlamentarismo' for being a deviation from a genuinely Liberal political life was widespread.

Yet, the Italian case can be considered as an expression of the economists' natural leaning toward an involvement in politics. This leaning is part of the Italian intellectual tradition of 'militanza' (militancy). Since Italian intellectuals were increasingly disappointed with the actual working of the liberal institutions, many individuals (economists included) engaged in a lively discussion about the foundations of democracy and liberalism, as well as the relevance of these ideologies for Italy. Vilfredo Pareto, Maffeo Pantaleoni, Antonio De Viti de Marco, Gaetano Mosca, Luigi Einaudi and Umberto Ricci are just a few names among the thinkers who animated such debate in the journals.

The role of university economists in parliaments outside Italy was also often weak. In Portugal no economics professors entered Parliament during the liberal age. In Belgium, the three most well-known nineteenth-century economists (Emile de Laveleye, Waltère Frère-Orban and Gustav Molinari) did not belong either to a university or to the parliament. In Britain, owing to the backward professionalisation of economics (which of course did not forbid the development of economic science in that country!), only four out of 24 parliamentary 'economists' in the period 1848-1914 were also professors.⁴ Note that the main

representative of mid-nineteenth-century British economics, John Stuart Mill, was an MP for three years only. Finally, in the United States, almost no professional economist sat either in the House of Representatives or in the Senate during the nineteenth and the twentieth centuries (for an explanation, based on the American anti-intellectualist tradition, see p. 292).

The third issue to consider is the extent and form of the economists' involvement in parliamentary life, both through their participation in the debates in the main assembly and in their more obscure contributions to the parliamentary commissions and/or committees. Interesting illustrations of the latter form of participation arise in Britain, where economists such as Alfred Marshall—never an MP—participated in a substantial way.

The substance of the contributions made by economists is also not always straightforward. Many themes of economic policy recur in the parliamentary debates. The economists shared an overall attitude favouring free trade and *laissez faire* (apart from the German case). Their position was not so univocal on other social measures, such as the reduction of work hours, reform of taxation, the unity or plurality of the money issue, and so on. But the positions taken on these measures mainly turned on political choices, not on economic theories, which were kept outside the parliamentary hall. Many economists also shared contrasting positions on such topics. It is therefore difficult to follow Augello and Guidi (2005, p. 22) when they speak of 'pluralism' as a feature of late nineteenth-century economics. Everybody who has in mind the writings (including their correspondence) of Walras, Pantaleoni, and Pareto knows quite well that these authors considered economics as well as politics as a perpetual terrain of struggle: *mors tua, vita mea*.

To conclude, *Economists in Parliament in the Liberal Age (1848-1920)* is a very rich and informed volume that can be suggested not only to professional economists, but to general historians of liberal institutions and to historians of culture as well.

* Department of Economic Sciences. Università di Pisa. Curtatone and Montanara n.15 – 56100 Pisa. Email: faucci@dse.unipi.it.

Notes

1 The only exceptions in the aforementioned book were Fedele Lampertico, who never taught at the university, but was a member of the Accademia dei Lincei and received a *honoris causa* doctorate at the University of Dublin for his merits as an economist; and Marco Minghetti, author of a book on economics that was translated into French.

2 Only in Spain was the presence of economics professors in parliament almost as dramatic.

3 Obviously, the importance of parliament in vulgarising the current economic 'doctrines' (free trade, *laissez faire*, State intervention, and so on) was relevant, but this is a completely different problem.

4 Backhouse may be criticised for being rather restrictive in his selection of economists who served in parliament. Many participants in the parliamentary debates on India, the trade depression, home rule and inter-colonial trade in the late nineteenth century were real experts in economics.

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Mark G. Hayes. *The Economics of Keynes: A New Guide to The General Theory*. Cheltenham: Edward Elgar. 2006. Pp. xvi + 257. ISBN 1 84720 082 6. £65.

J. E. King*

This is a most unusual and thought-provoking book. Not a history of economic thought in any strict sense, it is described by the author as a ‘travel guide’ to Keynes’s great book. It is essentially a work of critical reflection, drawing principally on Mark Hayes’s personal reading of the *General Theory* with only limited discussion of the secondary literature and very little reference to the evolution of Keynes’s own ideas, before or after 1936. The six chapters correspond broadly to the six parts of the *General Theory*, and deal successively with ‘Two Theories of Employment’, ‘Definitions and Ideas’, ‘The Propensity to Consume’, ‘The Inducement to Invest’, ‘Employment, Money and the Price-Level’ and ‘Policy Implications’. There is also a Prologue and an Epilogue. Almost exactly half the book is taken up by appendices to five of the six chapters and to both the prologue and epilogue; I do not remember having come across an appendix to an epilogue (or prologue) before.

Hayes acknowledges the assistance of Victoria Chick, and his style of writing is rather similar to hers: clear and elegant but also extremely dense, making it almost impossible to summarise the argument without doing serious injustice to it. But a reviewer must try, even though Hayes is also very difficult to categorise. He has a certain affinity with the Post Keynesian approach to the *General Theory*, at least in its ‘fundamentalist Keynesian’ variant (Chick, Paul Davidson, Sidney Weintraub), and also shows some sympathy for the Old Keynesians. He has very little time for the New Keynesian doctrine that market imperfections and wage-price rigidities are responsible for all macroeconomic problems. In the final analysis, though, Hayes is very much his own man. His is a profoundly Marshallian interpretation of Keynes. ‘It is well worth reading Marshall’s *Principles*’, he suggests, ‘especially Book V, before turning to *The General Theory*’ (p. xv).

Five central themes are stated on page 1, which is a sort of prologue to the prologue. They concern equilibrium, competition, money, expectation and liquidity. First, Hayes attributes to Keynes the proposition that ‘Employment is in continuous “daily” equilibrium corresponding to the point of effective demand, although equilibrium does not mean that all available labour and capital-goods are employed and factor markets clear, nor that expectations are fulfilled’. Second, competition is ‘the motive force which holds the system in equilibrium. Agents take prices in each market to be independent of their own actions. The degree of competition, however, is not the same as the [inverse of the] degree of monopoly’. Third, money ‘is an integral part of the theory of value and employment, and not a veil’. Fourth,

‘Decisions to produce, consume and invest are based on expectation. Effective demand corresponds to the state of expectation at any time. The long and short term are not the same as the long and short equilibrium periods. The future is unknown, and long-term expectations are fundamentally uncertain’. Finally, liquidity involves both convertibility and ‘invariance of value to changes in the state of expectation ... so that money is more liquid than bonds, and both are more liquid than capital-goods’. These propositions, to repeat, summarise Hayes’s interpretation of Keynes and (I suspect) his own thinking on all these issues. His views on money and liquidity are probably less contentious than the positions that he takes on the other three topics. Many Keynes scholars will agree with the broad thrust of Hayes’s thinking on equilibrium, competition and fundamental uncertainty but with reservations on important points of detail, for example his criticism of Jan Kregel’s well-known gloss on Keynes’s approach to time and expectations (pp. 22 n.11, 61 n.4). Hayes has certainly provided a great deal for Keynesians of all descriptions to think about.

On many other questions his analysis is perceptive and enlightening. Consider three examples. First, Hayes disposes of the Pigou effect, brutally and succinctly, on the grounds that it applies only in a world with no debt and no bank money (pp. 127, 178). Second, he gives short shrift to Weitzman’s argument that involuntary unemployment is inconsistent with pure competition since, without significant economies of scale, the unemployed could escape the dole queue simply by employing themselves (a misleading claim that won over a theorist as acute as Nicholas Kaldor). Keynes’s concern was with employment, not self-employment. ‘Increasing returns may partly explain the existence of a wage-dependent labour force, but it is not the question addressed by Keynes’ (p. 60 n.1). As Marx would have said, the *General Theory* is about capitalism, not simple commodity production. Third, Hayes disposes of the alleged need for ‘microfoundations’ to underpin the Keynesian consumption and saving functions. ‘Keynes offers no equilibrium theory of the level of consumption, and his claim that the marginal propensity to consume is less than unity is the simple corollary of the existence of a market value for output’ (p. 127). This is demonstrated formally in Appendix 3.3.1 (pp. 132-5). Although Hayes (again) does not use the terminology, his analysis could be viewed as providing macrofoundations for microeconomics, in the sense that it underpins any utility-maximising model of individual consumption and saving behaviour.

These are important points, and give some indication of the significance of *The Economics of Keynes*. There are, of course, also some weaknesses. Hayes’s treatment of policy questions is unsatisfactorily sketchy, for example, as is his discussion of the broader social and philosophical issues raised by Keynes’s theory of employment. But then the same could be said of the *General Theory* itself.

Regarded as a tourist destination, Keynes’s book probably resembles Venice more closely than any other city. You can go there again and again and leave as fascinated, and as confused, as you were on your very first visit. Just when you think you know exactly where you are (in a large piazza, or very close to the Grand Canal) you take a wrong turn and suddenly find yourself in maze of narrow, twisting alleyways with no obvious mean of escape ... until you consult your travel guide. Hayes’s guide to the *General Theory* is very much better than most, and is strongly recommended to all past, present and prospective tourists.

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

Laurence S. Moss and Andrew Savchenko (eds). *Talcott Parsons: Economic Sociologist of the 20th Century*. Malden, MA and Oxford, UK: Blackwell. 2005. Pp. xxviii + 226. ISBN 4051-5529-9 (HB). US\$65.00.

J. E. King*

In March 1975, when Talcott Parsons drove down from Harvard to Brown University for a seminar on his life and work, some exciting new technology was being trialled. The seminar was videotaped by a technician from the Rhode Island School of Design, who drew on the technical assistance of the Director of the Language Laboratory at Brown and the Audiovideo Director of Rhode Island Junior College. Thanks to their expert supervision the taping 'went exactly as planned' (p. xv). A transcript was made of the proceedings and, published here for the first time, forms the first 52 pages of this book. There follow three brief (and mistitled) 'Contemporary Responses' written 30 years later by the sociologists Robert Holton, Giuseppe Sciortino and Richard Swedberg (pp. 59-74), and five full-length papers providing 'New Directions in Parsons Research and Research in the Tradition of Parsons' by Milan Zafirovski, our own Paul Dalziel and Jane Higgins, John Holmwood, Jens Beckert, and Alexandra Hessling and Hanno Pahl. The entire volume, which includes a 16-page editorial introduction, originally appeared as the January 2006 issue of *The American Journal of Economics and Sociology*.

The seminar took the form of a long and leisurely interrogation of Parsons by members of his audience, which included Brown faculty from sociology and the history of science, graduate students from a number of departments (excluding economics), and some 'faculty visitors', two of whom were economists. These were Mark Schupack, a name unfamiliar to me, and George H. Borts, managing editor of the *American Economic Review* and *bête noire* of radical political economists; sadly both economists were silent participants. There is little evidence that Brown's sociology students were at all radically inclined. Parsons's description of the United States as 'a relatively egalitarian society' (p. 22) goes unchallenged, and although he has to field some tricky questions concerning his treatment of power (pp. 37-40) he does not undergo the expected grilling on his analysis of social class, hostility to Marxism and alleged inability to account for revolutionary social change, all of which were commonplace objections among the insurgent sociologists of the early 1970s. Possibly this movement had ebbed by the middle of the decade; perhaps it had just passed Brown by.

None the less, there is much of interest in Parsons's account of his early life and influences. Having started out as a biology student he was converted to economics by the institutionalist Walton Hamilton. In 1924-5 Parsons studied at the London School of Economics under Bronislaw Malinowski, and then went to Harvard via the University of Heidelberg, where he learned about Max Weber. At Harvard he undertook research into Marshall under the guidance of Frank Taussig and Joseph Schumpeter and began an investigation of the medical profession as a case study in the socially imposed limits on the pursuit of individual self-interest. At some point in the late 1920s Parsons began to lose interest in economics and to see his future in sociology. Unfortunately the motives behind his switch of discipline do not emerge very clearly from the seminar transcript. Questions from economists might also have elicited more information about Parsons's views on

money and its relationship to power (pp. 41-3), on the inappropriateness of physical models for the study of society (p. 56), and on the circumstances under which he was invited to give the Marshall lectures at Cambridge in 1953-4 (pp. 29-31).

In the 'Contemporary Responses', Holton discusses Parsons's treatment of power, Sciortino emphasises his hostility to economic (disciplinary) imperialism and the dangers of the money-power analogy, and Swedberg writes perceptively on the relationship between Parsons and Schumpeter. There is a fascinating reference to the inter-disciplinary seminar on rationality that the Austrian organised at Harvard in 1939-40, with participants including Wassily Leontief and Paul Sweezy. It seems that Schumpeter had Parsons's card marked from the beginning: 'The author has in fact so deeply penetrated into the German thicket as to lose in some places the faculty of writing clearly in English, and some turns of phrase become fully understandable only if translated into German' (p. 72).

The 'New Directions' articles all deal with aspects of Parsons's economic sociology and the troubled relations between the two academic disciplines involved. Zafirovsky claims that Parsons did manage to construct theoretical and methodological bridges between them. Dalziel and Higgins, in a piece presented at the 2002 HETSA conference, focus on the views of Parsons and Pareto on the boundary between economics and sociology, while Holmwood considers Parsons as a critic of orthodox economics (this paper is very heavy going). Beckert argues that he should not have been dismissed as irrelevant to the new economic sociology of 'embeddedness', while Hessling and Pahl apply the Parsonian categories to the global system of finance.

This is a useful volume, with much of interest to historians of economic thought who try to take an interdisciplinary approach to the subject.

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

<p>Ernesto Screpanti and Stefano Zamagni. <i>An Outline of the History of Economic Thought</i>. Oxford: Oxford University Press, second edition, revised and expanded, 2005. Pp. xviii + 559. ISBN 0 19 927913 6 (HB); 0 19 927914 4 (PB). US\$144 (HB); US\$45 (PB).</p>
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J. E. King*

The future of the history of economic thought as a sub-discipline or specialist field in economics is in serious doubt. Roy Weintraub's keynote address at the 2006 ESHET conference in Porto was profoundly pessimistic, reporting the disappearance of history of thought courses from PhD programmes in all leading US universities other than Duke (Weintraub 2006). As noted in 'RQF and HET: Assassin or Corpse' in this issue of *HER*, the list of the subjects of every successful PhD dissertation in economics in the academic years 2004-5 and 2005-6 published in the *Economic Journal* does not include any theses in the history of economic thought; the closest, an application of Adam Smith's theory of moral sentiments, is

classified (presumably at the author's request) under economic methodology (*Economic Journal* 116(512), June 2006, pp. F337-F345).

For a field apparently in terminal decline, the history of economic thought continues to inspire an impressive number of textbooks. In the last five years alone there have been three entirely new texts (Backhouse 2002; Vaggi and Groenewegen 2003; Roncaglia 2005), and second editions of two more, the 1979 volume by E. K. Hunt (2002) and the book under review, the first edition of which was published in 1993. All have their strengths (and weaknesses). While I cannot pretend to offer a comparative assessment of these five, and the competing works from the previous millennium by respected authors like Barber, Blaug and Spiegel, I must record my appreciation of Screpanti's and Zamagni's *Outline*. It is scholarly but accessible, comprehensive but manageable, often thought-provoking but rarely exasperating. Probably few will want to read it from cover to cover, but few will fail to benefit from intensive browsing.

The authors wisely begin not with classical antiquity but with the rise of liberal humanism at the dawn of modernity. They deal with all the usual seventeenth-, eighteenth- and nineteenth-century suspects, but devote almost two-thirds of the book to the twentieth century, taking the story right up to 'the threshold of the millennium' (the title of the first section of the concluding Chapter 12). Heterodox ideas are taken seriously, but not at the expense of skimping on the mainstream. Ideas are placed in their socio-economic and political context, but are not reduced to a mere superstructural reflection of the material base.

Faced with a book of more than 500 pages, I inevitably turned to those areas with which I am most familiar. The discussion of Keynes and the Post Keynesians is generally good, with due consideration being given to Michal Kalecki and to all the principal Cambridge theorists, but their US colleagues are less well treated. There is almost no mention of Sidney Weintraub or Paul Davidson, and eyebrows will be raised at the inclusion of James Tobin as a Post Keynesian (pp. 358-9) and at the attribution to the Post Keynesians more generally of a downward-sloping aggregate demand curve in price level-real GDP space (p. 373).

The lengthy exposition of Marx (pp. 142-61) is clear and reasonably comprehensive, though I was surprised to see the brief reference to alienation coming in an earlier section (p. 137); the term is not indexed. Post-1883 Marxian political economy receives extensive attention in Chapters 8, 11 and 12, with particular interest shown in the contributions of Dmitriev, von Bortkiewicz and Charasoff to value theory, in the work of Paul Baran and Paul Sweezy, and in the analytical Marxists. Some of the classic texts receive very short shrift: Hilferding, Luxemburg and Lenin are covered in one short paragraph (p. 314) and there is no mention of Bukharin. Marxian theories of imperialism are neglected, and there is no suggestion in the Chapter 12 discussion of globalisation (pp. 456-61) that Marxian economists have made any contribution to this important question. It is difficult to see why Baran and Sweezy are treated as 'orthodox' Marxists (p. 448) while Ernest Mandel is regarded as a heretic (p. 450), and I was astounded to read the following: 'As regards value, almost all Marxists have come to accept Sraffa's theory of production prices' (p. 452). In Italy, perhaps...

On the whole, however, this is all very well done. I have no reason to doubt the accuracy and perceptiveness of the authors' treatment of more orthodox writers. For this non-specialist, the assessments of Walras (pp. 180-8) and of twentieth-century developments in Walrasian theory (pp. 380-96) seem unusually

lucid and balanced. It was good to see methodological aspects of the general equilibrium research programme receiving a searching examination.

There are, inevitably, some problems. The translation creaks a little at times, usually where the definite article is involved, and the generally very helpful reading lists at the end of each chapter should have been updated more thoroughly for the second edition. The latest reference for Marxian economics between 1900 and 1950, for example, is to a book by Naum Jasny published in 1972; the majority of the sources cited on Post Keynesian economics are pre-1985; and there is nothing on institutionalism later than 1992, so that important recent work by Malcolm Rutherford and Yuval Yonay is ignored. I wonder, too, whether it was sensible to retain so many Italian-language sources when they will be intelligible to at most one per cent of the English-language readership.

In places too much knowledge is assumed: the interpretation of Adam Smith, for example, is subtle and convincing, but will the intended readership—‘the educated person, or, rather, the person who wishes to educate himself or herself’ (p. viii)—understand what institutionalism is, two or three hundred pages before Veblen and his disciples make their first substantive appearance? I think that there is too little on Ricardo, and too much on contemporary anti-Ricardians (Chapter 3). As already noted, the index is not always reliable. Happily it is not the case that the only mention of ‘new classical economics’ comes on p. 3, but only readers who already possess some specialist knowledge could reasonably be expected to locate the subsequent discussion under ‘rational expectations’ and ‘neomonetarism’.

These are irritating but essentially minor blemishes on a generally very good book. Ambitious to the last, Screpanti and Zamagni conclude by speculating on the future of economics. The discipline is currently in a state of ‘post-Smithian’ crisis, they maintain, drawing on post-modernist thought (oddly without any reference to Deirdre McCloskey), and the emerging theoretical pluralism is a thoroughly healthy state of affairs:

This is why we are pleased to see Babel developments in contemporary economic science. We do not know where this scientific revolution will lead us. But we do know what we are leaving behind. And we believe that overcoming *Homo oeconomicus* reductionism is a necessary step in anticipation of the reconstruction of an economic science of which we must not be ashamed. (p. 514)

Screpanti and Zamagni do not, alas, tell us what this might mean for the future of 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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Perry Mehrling. *Fischer Black and the Revolutionary Idea of Finance*. Hoboken, N.J.: John Wiley and Sons. 2005. Pp 374. ISBN 978-0-45732-9. US\$29.95.

Jan Toporowski*

There is a strong case for the view that everything of interest about the Nobel Laureates in Economics of the last two decades has already been proclaimed in the official biographies that accompany their awards. Perhaps for this reason Perry Mehrling chose as the subject of his most recent book not Robert Merton and Myron Scholes, who were awarded the Nobel Prize in Economics in 1997, but their co-author Fischer Black, who could not share in their award because he had died in 1995, and because the Prize is never awarded posthumously. Had he been alive, and shared in the Prize, there is no doubt that a Nobel biography highlighting the 'scientific discovery' of the Black–Scholes option pricing formula would have captured all that was exciting about Fischer Black. It is to Perry Mehrling's credit that he has written a book demonstrating how much more can be said about someone whose life and career was as predictable as his acclaimed contribution to economics was uncontroversial.

Mehrling's attempt to invest Black with heroic qualities is perhaps the only major failure of this book. The son of a small-town businessman, Fischer Black's precocious ability in mathematics and computing brought him on a scholarship to study physics and mathematics in Harvard. There the famous American liberal university education ensured that he acquired no systematic knowledge of any subject in particular, but was able to dabble in social science and philosophy (he returned to the work of Willard Quine later in life). His main interest then was in the possibilities of using computers to develop artificial intelligence. In 1964, he was awarded a PhD at Harvard for a dissertation in applied mathematics entitled 'A Deductive Question Answering System'. Then, as now, applied mathematics offered little in the way of a career, except perhaps for the boffins simulating war games for the Pentagon, but was of interest to banking and financial institutions at work in the post-War financial boom. Computing was expanding hugely the possibilities of organising systematically the securities price data that economists from Irving Fisher through to Alfred Cowles III (founder of the Cowles Commission) believed, if subjected to sufficient processing, would reveal how to beat the market, or at least how it could be predicted. Inevitably Black was drawn to consultancy work, initially for A. D. Little, and subsequently for his own consultancy firm, Associates in Finance, calculating whether and how the market could be beaten.

At A. D. Little, Black first met Jack Treynor. Treynor introduced Black to the Capital Asset Pricing Model as a way of thinking about securities prices.

Briefly the model states that the return on a security, which essentially determines its value, consists of a return that is the average for the market in which the security is traded, plus some multiple of that return that may be greater or less than unity, and that is specific to that security. In some versions of the model that security-specific multiple (known as 'beta', the return relative to the market return) is treated as an index of the riskiness of the security, so that it essentially measures the margin of risk offered by the security (and hence the excessive return) over the return from a risk-free security. In finance theory, the Capital Asset Pricing Model came to be associated with the 'efficient markets hypothesis' or the view that prices in securities markets reflect all the publicly known information relevant to the evaluation of each security. Any relevant privately held information comes into the market through the buying and selling that holders of such private information will undertake.

Black embraced these ideas with enthusiasm and, as Mehrling points out more than once, they remained a constant theme and inspiration for his work. No matter that they involved some peculiar assumptions, such as perfect arbitrage, and zero transactions costs. Black's first major finance assignment, for Wells Fargo Bank, was to develop a 'product' or investment offer that would give customers of the Bank consistently higher than average market returns, without higher risk. According to the efficient market hypothesis this was something that could only be temporarily and unpredictably achieved. 'Noise' traders who speculated on market inefficiencies should, over the long period, lose money to 'professional' traders who made the market efficient. Black nevertheless thought that he had found a stratum of securities whose risk was consistently underpriced. His proposal was rejected by the Bank's fund managers, because the active trading strategy that it entailed would have been too expensive.

The meeting with Treynor had other consequences. In 1969, Treynor became editor of the *Financial Analysts Journal*, a professional rather than academic journal. This became Black's main publishing outlet, allowing him to get his ideas into print without having to undergo the tedium of persuading academic journal editors of the value of his reasoning.

In 1971, Black obtained a visiting position at Chicago University that later was converted into a tenured chair. At Wells Fargo, Black had met Myron Scholes. Together they produced the options pricing formula, for which they became famous, even before Black came to Chicago. It was published in the *Journal of Political Economy*, Black's only paper in a highly ranked academic journal. The success of the formula was assured by personal connections with the Chairman of the Chicago Mercantile Exchange, Leo Melamed. Melamed had recognised the trading opportunity offered by the breakdown in financial regulation that had come with the collapse of the Bretton Woods system of fixed exchange rates. He used the coterie of free market economists at Chicago, headed by Milton Friedman, to argue for the liberalisation of regulations preventing options trading at his exchange. Once options trading started, in 1983, the Black-Scholes formula came into its own, since any option bid or offer price that deviated from the one indicated by the formula pointed to a speculative gain for the trader. Mehrling rightly points out that it was not the first such formula: Samuelson had published a 'Rational Theory of Warrant Pricing' in 1965. But the Black-Scholes formula was more general, and more convenient to use.

Mehrling does not go into the technicalities of the formula, so that the unreal assumptions, such as efficient markets and perfect liquidity, that Black and

Scholes were obliged to make in order to arrive at their equation, are not really discussed in the book. Black did develop a more realistic version with fewer restrictive assumptions. But this attracted little attention, even in this excellent book. All of which goes to show that what the markets were after was a pricing convention, rather than an insight into reality. For a while, the Black–Scholes formula performed well. But as other formulae emerged and came to be used by traders, the Black–Scholes formula performed less well (Toporowski 2000, pp. 98-9).

An implication of efficient markets is that the study of financial markets gives one worldly experience, because all the ‘relevant’ information comes to the market. Most of those engaged in finance at a trading or a senior level are naturally flattered by seeing wisdom in what the markets do, and in particular in trading or strategic decisions. Study of this wisdom and the efficient market hypothesis thus reinforces a delusion that the rest of the world is like the financial markets or, if it is not, then it should be. This was no less true of Fischer Black. His schemes to reform accountancy to orientate it towards giving a ‘true’ value for a company’s stocks, pension finance (sticking to bond holding), or corporate finance, betray an almost touchingly naïve belief in market efficiency, and the efficacy of looking at financial positions as options. Even so, the insight into the operations of financial markets, promised by the capital asset pricing model and the efficient market hypothesis, turns out to be hopelessly unrealistic. Black’s view that the 1987 Crash happened because investors’ appetite for risk increased was a typical, *ex post* rationalising, implication of these theories. After he moved to Goldman Sachs in 1983, Mehrling reports, his quantitative trading schemes lost money. The bank’s fortunes peaked in 1993, thanks to fixed income, currencies and commodities positions, rather than Black’s trading.

So much for the conventional career and ideas. These were more symptomatic of, rather than determining, the rise of finance in US capitalism, that was, and largely remains, beyond the knowledge and understanding of most economists. I pass over the invidious comparison in the book between Black’s second and third wives (it seems to me sufficient for the purposes of the book that he found happiness with both of them in different circumstances); some psychological typing that is not illuminating; and idiomatic syntax that is less than literary (e.g., quotations from Black’s writings introduced by ‘Says Black...’). Where the book caught fire for me was about half-way through, with the chapter on ‘The money wars’. As may be expected from the author of an outstanding book on US monetary policy (Mehrling 1997), Perry Mehrling brings to Black’s work on economic theory outside finance a breadth that one could only wish that his subject had had. Black was largely self-taught in economics. He learned his economic theory by reading journal articles in between consultancy assignments, and through more extensive reading of Hayek during the 1980s. This education brought him right up to the cutting edge of economic theory as it was around 1933. His inability to get his papers on monetary theory, business cycles and general equilibrium published in academic journals was not just because of the faddishness and exclusiveness of journal editors. Nevertheless, this does not mean that Black was merely derivative in his economics. Some of his interventions do raise important issues for the economic theory of his time and for today.

A case in point is his view on money. Even before he had arrived in Chicago, Black had disputed Milton Friedman’s view that the way to control inflation and stabilise the economy is to regulate the money supply. Black’s reasoning is revealing. According to the efficient market hypothesis and the capital

asset pricing model, traders are supposed to respond to changes in securities prices according to whether the traders are risk averse, or are willing to make a play for higher returns in exchange for higher risk. If securities prices rise, investors who are not averse to risk are supposed to leverage up their portfolios: borrowing more in order to invest in rising stock prices. In the same situation, and faced with an increase in the value of their risky assets, risk-averse investors are supposed to sell securities and place the proceeds into risk-free bank deposits or government paper in order to maintain a constant proportion of riskless assets in their portfolios. However, if the supply of bank credit is inelastic, then the resulting market-efficient general equilibrium cannot be obtained. Interest rates will rise, as bank credit expands, and thwart the re-establishment of an efficient equilibrium in the financial markets, that is, an equilibrium that reflects all the relevant information and the attitude to risk of the investing community. In other words, although Mehrling does not say so explicitly, endogenous money, or perfect liquidity—the possibility of costless and instant conversion of assets into current money—is a condition of maintaining efficient equilibrium. But if that is the case, as Keynes or Wicksell might have pointed out, then the rate of interest cannot be determined in the ‘real’ economy by the balance between saving and investment. In general, Mehrling points out, Black was hostile to any active monetary policy because it interfered with the general equilibrium established by what he regarded as the community’s attitude towards risk.

Black’s views on business cycles, much as his mature political views, were warmed-up Hayek, although Mehrling is too polite to say so. In Hayek’s monetary business cycle, economic instability arises as different industries adapt to changed monetary conditions, with lower interest rates stimulating over-investment in more productive industrial processes that take longer and involve more industrial specialisation in the production of final goods. Black’s criticisms of the ‘real business cycle’ theories of Kydland and Prescott were an apt reminder of the realities of industrial production, but were unfortunately timed just as industrial economics was being displaced by finance theory. Black believed that the widespread assumption in real business cycle theory of equilibrium between flow variables ignored the active part in economic decision-making played by disequilibrium between stock variables, capital stocks in particular. Black nevertheless adhered to general equilibrium in a way that Hayek never did. Hence his belief that the markets were in equilibrium during the 1987 Crash.

Finally, Mehrling highlights Black’s critical attitude towards econometrics. Black defined econometric models as those whose structure is defined by data, rather than theory. In his view this led in virtually all cases to serious misspecification, with the result that estimated coefficients ‘are virtually meaningless’ (pp. 118-19). In his second, and final, book his starting point was ‘stylised facts’. He listed twenty-three such facts, which he proposed as the subject of economic analysis. These were headed by ‘unlimited growth’, ‘persistent inequality’ and ‘growing specialisation’ (pp. 281-3. See Black 1995).

Throughout the book, Mehrling allays the reader’s doubts about the intellectual significance of Fischer Black by showing his work as an honest intellectual response to increasingly dishonest times. This gives his book a fascination that cannot be attributed to any of the grey personalities he describes. One of the major strengths of North American economics is its continuing investment in the popularisation of economic ideas, as exemplified in the work of people like Douglas Dowd and Paul Sweezy, and journals like *Challenge* and

Monthly Review. We had that too, as evidenced by John Stuart Mill's 'People's Edition' of his *Principles*. But we seem to have lost it with the shackling of academics to an insecure treadmill of teaching to improve the career paths of our students, and researching to impress other academics in the discipline, so that popular economic discourse is taken over by journalists with little interest in the consistency of ideas. Perry Mehrling has shown that it is possible to write a research-based book that can and will be widely read. It should be recommended to students in preference to those boring old finance textbooks.

* The School of Oriental and African Studies, University of London, and the Research Centre for the History and Methodology of Economics, University of Amsterdam. Email: jt29@soas.ac.uk.

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