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Reviews

Understanding Dennis Robertson: The Man and His Work, by Gordon Fletcher (Edward Elgar, Cheltenham, and Northampton, USA, 2000), pp. xii + 433.

Dennis Robertson's contribution to the development of macroeconomics in the first half of the twentieth century is often regarded as having been of fundamental importance. Keynes, for instance, in a letter to Robertson, written shortly after the publication of the *General Theory*, acknowledged that 'I certainly date all my emancipation from the discussion between us which preceded your *Banking Policy and the Price Level*.'

In this outstanding biography, Gordon Fletcher seeks to understand Robertson's work in economics by exploring connections between the character of the man and his work as an economist. This, of course, is what intellectual biography should strive to do.

We already know something about the life and work of Robertson from the biographical essays by J.R. Hicks and Paul Samuelson, and from the work of J.R. Presley. Fletcher now adds considerably to our knowledge. We learn that Robertson was remarkably successful as a student, first at Eton, where he was captain of the school, and from where he won a scholarship in classics to Trinity College, Cambridge. After obtaining a First in Part I of the Classics Tripos, he changed to economics and secured a First in Part II of the Economics Tripos. He was elected President of the Cambridge Union, and President of the University Liberal Club.

Robertson's Director of Studies was J.M. Keynes, who also acted as supervisor of his dissertation submitted for a Fellowship at Trinity College. This was published in 1915, as *A Study of Industrial Fluctuation*. This is a major work in trade cycle analysis and is considered by some to be Robertson's most enduring contribution to economics. Drawing upon the work of Continental writers such as Aftalion and Tugan-Baranowski, and following some hints from Marshall himself (it was Robertson who used to say, 'It is all in Marshall'), he highlighted the importance of real factors, such as inventions and the state of agricultural production. These factors seemed to him to be far more significant than monetary influences, and this view put him at odds with Ralph Hawtrey, for whom the trade cycle was uniquely a monetary phenomenon.

Much of Robertson's *Study* is devoted to an examination of empirical data drawn from a wide array of industries. The argument, in essence, is that real forces serve to trigger a rise in investment, which drives the economy forward. Because of time lags, investment spending is inclined to overshoot actual requirements; once this is realised, investment will diminish, and economic activity will slacken. Monetary policy could mitigate the effects of a slump, but Robertson did not believe that it could completely stabilise the economy. Following the view adopted in 1909 in the Minority Report of the Poor Law Commissioners, Robertson believed that more might be expected from accelerating (or delaying) expenditure on public works, than from monetary policy.

Even so, he did not believe that policy should be aimed strictly at securing short-term stability. Modern societies were prone to cyclical instability, the result of the bunching of new technology. Short-term adjustments to policy might inhibit the dynamic processes associated with economic growth. Therefore he was apt to tolerate temporary unemployment in order to secure longer-term gains in productivity.

Following war service, in which he won the Military Cross, Robertson returned to Cambridge, where he was invited by Keynes, the editor of the Cambridge Economic Handbook series, to write the textbook *Money*.[1] In this work, Robertson attempted to bring money into his explanation of the trade cycle. But the inclusion of money into the explanation of the trade cycle was not successfully achieved until the revised edition of

[1] It was with his book *Money* that Robertson began to use quotations from *Alice in Wonderland*, which became a hallmark of all his later writing. Fletcher devotes considerable space to interpretations of Robertson's use of these quotations, concluding that they provide important clues to his personality.

Money in 1928, in which he incorporated some of Keynes' thinking in the *Tract on Monetary Reform* (published in 1923), and some of his own thinking in *Banking Policy and the Price Level* (published in 1926).

Banking Policy and the Price Level is a difficult work that uses obscure terminology. It explores the relationship between investment and saving, and the implications of divergences between the two for price stability and the trade cycle. The framework is conceptually dynamic, using sequence analysis in an embryonic and elementary form. While Robertson was writing this book, Keynes was beginning to write his *Treatise on Money*, and each acknowledged the assistance of the other. This proved to be the high watermark of their collaboration, and of their friendship. Both thereafter began to deteriorate. The breaking point came in 1936 with the *General Theory.* There was some reconciliation between the two on a personal level during World War II, particularly at the Bretton Woods conference, when Keynes relied on Robertson's consummate drafting skills. But their different approaches to economic theory rendered it impossible for them to restore the close friendship that had existed before the 1930s.

In the meantime, Keynes had surrounded himself with a coterie of younger colleagues – including Richard Kahn and Joan Robinson – some of whom became increasingly antagonistic toward Robertson, and he toward them. In these circumstances, Robertson left Cambridge in 1938 to take a chair at the London School of Economics, returning to Cambridge in 1944 to fill the Chair of Political Economy on the retirement of Pigou.

At the heart of the schism between the two men was the debate over the determinants of the rate of interest. Robertson disagreed fundamentally with Keynes's liquidity preference theory, while Keynes opposed Robertson's adherence to the loanable funds theory. This debate in Cambridge was continued after Keynes's death by his followers, and by Robertson and his supporters. Robertson also felt aggrieved that Keynes had been altogether too dismissive of the contributions of earlier writers, especially Marshall and Pigou.

Of this dispute between Keynes and Robertson, Fletcher stresses the point that it was Robertson who provided the major opposition to Keynes's economics until the arrival of monetarism. Fletcher is inclined to take Keynes's side in the dispute, arguing that Robertson had failed to understand the significance of the fallacy of composition as it applied to the paradox of thrift. Whereas Keynes, in the *General Theory*, had shown that an increase in saving, at less than full employment, could reduce income and employment, and hence the propensity to save, Robertson continued to adhere to the traditional belief that a rise in saving would lead to an increase in investment. Of Robertson's contention that liquidity preference was the lynchpin of Keynes's new system, Fletcher argues that the idea of liquidity preference came very late in Keynes's thinking, and therefore cannot hold the pivotal position that Robertson had claimed for it. This conclusion will not satisfy everyone.

The falling out between Keynes and Robertson left the latter isolated, frustrated and disillusioned. The falling out is the occasion of Fletcher's focus on the personality of Robertson, which provides the central theme of the book. Robertson, according to Fletcher, inherited a deeply ingrained sense of duty, and there existed throughout his life a struggle between the demands of duty and the desire to escape from its demands. Until perhaps as late as the publication of the *Treatise on Money*, Robertson's ideas on macroeconomics were ahead of those of Keynes: Robertson was prepared to move some distance into new territory. But piety for his forebears restrained him from moving too far ahead of them. Keynes, on the other hand, possessed few inhibitions and had leapfrogged Robertson by the mid-1930s. Robertson believed that Keynes had moved too far ahead of mainstream opinion, and in doing so had been led into making egregious errors.

In short, and drawing upon biographical and literary evidence, Fletcher contends that Robertson's temperament and outlook on life exerted a profound influence on his approach to economics and on the development of his economic ideas. 'The conclusion to be drawn', Fletcher, writes, 'is that the curious half-way house appearance of Robertson's theory derives from his practice of seeking to harmonise new insights and new departures with established "truths". The first was a product of his leaping intellect; the second the outcome of a temperamentally determined need for unbroken links with his origins . . . back to some secure past time or golden age' (p. 294).

This is a novel and arresting interpretation of Robertson's economics. Many will contest it. But there can be no denying Fletcher's originality, or his scholarship; the book is thoughtful, insightful and comprehensively researched. Its material is well organised, especially given its wide scope, covering not only Robertson's life and work, but also extensive literary and character analysis. It is a book that can be highly recommended.

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Uncertainty, Production, Choice, and Agency by Robert G. Chambers and John Quiggin (Cambridge University Press, Cambridge, UK 2000), pp. xvi + 373.

This is a book with a message. It wants to convince the reader of the superiority of the state-contingent approach for the analysis of production under uncertainty. Combining their expertise from duality theory and from the theory of decision-making under uncertainty, the authors argue that new and interesting insights can be gained by looking at production under uncertainty in a consequent state-space based approach.

The state-contingent approach is not new in economics. It is used in general equilibrium theory to study the allocation of state-contingent commodities in the context of general production sets and preference orders. Yet, at this level of generality, little more than existence of market clearing prices and Pareto-optimality of the competitive equilibrium can be established. In order to deduce more specific results on risk-sharing or asset prices, special assumptions about preferences and technologies are needed.

In financial economics or in contract theory it is common to represent technological uncertainty by stochastic production functions relating state-contingent output combinations to inputs or, equivalently, by induced probability distributions over commodities. These specifications of technologies preclude tradeoffs between state-contingent outputs at given input levels. Yet many production processes allow the producer to vary the mix of state-contingent outputs by allocating a given set of inputs in different ways. Rearranging inputs between different activities may offer a choice between low, but stable, output across states and riskier, but higher, state-contingent output. Such choices cannot be modelled by stochastic production functions.

Limitations resulting from the choice of a particular production model are unnecessary, as the authors convincingly argue. Applying well-known results from duality theory, one can deduce stochastic cost functions and their properties, even with quite general production sets. Moreover, notions characterising risk preferences of consumers, like certainty equivalent and risk attitudes, find natural analogues in a state-contingent theory of production. The book is divided into two parts of five chapters each, entitled 'Theory' and 'Applications'.

Chapter 1 sets the stage, arguing for the state-contingent view of production under uncertainty as superior to the more common approach of modelling production uncertainty by a probability distribution over outcomes. Chapter 2 offers a careful discussion of production sets in the context of state-contingent commodities. Meaning and interpretation of the 'usual' assumptions on technology sets do not remain unaltered under uncertainty. The assumption of free-disposal, for example, which is generally accepted under certainty, implies the feasibility of riskless, though possibly inefficient, production plans.

Cost functions, the main instrument for the analysis of general state-contingent technologies, presuppose profit-maximising firms and a set of complete state-contingent markets.

Chapter 3 therefore deals with the firm's objective function, identifying quickly state-contingent profits as the firms' objective. Maximin-type preferences, expected utility, mean-variance and rank-dependent expected utility are reviewed as preference functionals representing different risk attitudes. Probabilities over states are viewed as a purely subjective concept, implicit in preferences like risk attitudes. Chapter 4 turns to stochastic cost functions and revenue cost functions. Duality theory is applied in order to derive properties of cost functions.

The second part of the chapter introduces measures of production risk. Relative and absolute riskiness of technologies are defined and good and bad states are distinguished.

In Chapter 4 a tension becomes visible which permeates many applications in later chapters. The subjective view of probabilities over states adopted is in conflict with the characterisation of a technology's riskiness. The inherent riskiness of a technology is defined with reference to the expected revenue of state-contingent production vectors. Yet, which probability distribution shall one use to compute the expected value? The authors make it quite clear that they view the riskiness of a technology as a subjective feature of the producer. But which subjective probability distribution shall one choose for producers with preferences that are not probabilistically sophisticated? Which probability distribution shall one use if there is no single owner of a firm, but a group of shareholders? If different owners of a firm have different subjective probability distributions they will disagree about the riskiness of production vectors and about the inherent riskiness of the technology.

In the following chapters this problem is resolved by assuming, implicitly or explicitly, that all agents share the same probability distribution over states. Chapter 5 provides a typical example. It begins with the case of a risk-neutral producer where the problem of the adequate subjective probabilities of states is 'naturally' resolved. Later on, when producers with maximin or the Schur-convex preferences are considered, the authors continue to work with the subjective probability distribution of the risk-neutral firm.

The chapters of the second part treat different economic scenarios, mostly from agricultural economics. Chapter 6 studies a firm subject to production and price uncertainty in a world with forward and future markets. In Chapter 7, the producer can buy production insurance with or without loading factors. Insurance against production risk for a firm whose production activities also generate pollution is the topic of Chapter 8. Providing incentives for the prevention of pollution requires insurance contracts with actuarially unfair premia.

Chapters 9 and 10 apply the state-contingent approach to a principal agent problem under asymmetric information. With a state-contingent production technology, the free disposal assumption suffices to generate an optimal contract which is increasing in income. In contrast to the traditional stochastic production function approach, monotonicity of the optimal contract follows quite naturally in this case. Chapter 10 extends the principal agent problem to the case where the principal can manipulate the outside option of the agent.

The strength of the book lies in its clear and convincing argument that it is worthwhile and feasible to analyse general production sets under uncertainty by means of duality theory. Standard results from the economics of uncertainty and information can be derived as easily as with the more standard restricted stochastic production function approach. Moreover, as the application to the principal-agent problem shows, the state-contingent approach yields some intuitive properties of contracts without special assumptions.

A weakness of the book is the occasional loss of focus. For example, in Chapter 4, there is no need to go through well-known duality results in all notation-intensive detail. Or in Chapter 8, we really do not need to learn about an algorithm for the solution of the hidden-action problem. The chapters in the second part of the book are obviously separate studies of problems in agricultural economics which use the state-contingent approach, but would have benefited from thorough pruning. Many of the results and model variations presented in these chapters may be of interest for a reader who is familiar with the debates among agricultural economists; for a general economist their importance is not always clear.

Instead, one would have wished for a more detailed discussion of issues central to the approach. Riskiness of production as defined in this book relies on statistical concepts which require a commonly accepted probability distribution over states. Most economic theory under uncertainty assumes that agents optimise based upon a common probability distribution. So there is no problem in the traditional context of expected utility maximising agents with a common probability distribution over states. Yet, just as many other results from contract theory and the economics of uncertainty do not easily allow for a purely subjective view of probabilities, many concepts proposed in this book need careful reconsideration in such a context. In the concluding section, the authors state as an 'immodest goal in writing this book' their intention to 'start the project of rescuing stochastic production analysis from its moribund state'. In this regard they have succeeded and, most importantly, they have done so in writing a book which is well worth reading.

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APEC and the Construction of Pacific Rim Regionalism, by John Ravenhill (Cambridge University Press, Cambridge, 2001), pp. xii + 294.

Professor Ravenhill's thorough overview of the Asia–Pacific Economic Cooperation (APEC) process concludes that it has proved deeply disappointing. He is right to do so, in some important respects. But he undervalues some achievements, partly because of inadequate appreciation of what was, and what should have been, expected of the process. This book's strength is the assessment of the evolving attitudes of various members; its main weakness is its failure to explain why APEC set itself the wrong agenda from 1994 to 2000.

Ravenhill sets out to assess APEC from an international relations perspective, but becomes obsessed with one very narrow aspect of it; namely the liberalisation of border barriers to trade. But, as Chapter 4 in the book explicitly recognises, the broader objectives of cooperation include:

- changing the incentives for collaboration
- changing actors' interests and identities
- acting collectively in wider forums, such as the WTO
- changing perceptions of appropriate strategies for development by sharing experience among a diverse group of economies.

APEC was designed to promote all of these objectives, not to be just another forum to promote trade liberalisation. Chapter 2 sets out the motives for creating an intergovernmental process to help Asia–Pacific economies act upon shared interests. These included their growing interdependence and the threats to the international trading regime based on GATT/WTO.

Ravenhill sets out why it was difficult to initiate substantive cooperation among such a very diverse group of economies. It took 20 years of preparation, and most analysts were surprised that it was possible to convene APEC's first ministerial-level meeting in Canberra in November 1989. He also notes, correctly, that many participants, especially from ASEAN nations, were cautious about the process, partly for fear of undermining ASEAN itself, and partly because of fear that the United States would dominate the process and use it as another 'lever' to prise open markets. The Australian government conducted extensive discussions over many months to allay these fears and achieve consensus on the context and mode of cooperation.

The initial 12 members had decided, well before 1989, that 'opening to the outside world' was the way to promote sustained economic development. This view was not based on theory or ideology, but on clear evidence of the relative performance of trade-orientated and protectionist economies, and this led to an acceptance that reducing obstacles to international trade and investment was desirable, albeit sometimes politically difficult.

The agreed mode of cooperation was reflected in the agreed principles of APEC, set out initially at the Canberra meeting, and formalised in the 1991 Seoul APEC Declaration. These principles stress that APEC is to be a consultative forum, focusing on areas where cooperation was perceived to be mutually beneficial. APEC was to encourage the flow of goods, services, capital and technology, but the principles did not call explicitly for reduction of border barriers, let alone for their elimination. In other words, APEC was to be a voluntary process, much more like an Asia–Pacific version of the OECD, than a mini-WTO.

That was the only viable option for economic cooperation in this region. Events have demonstrated that it is still the only viable option. Ravenhill accepts this reality and sets out the nature of an agenda consistent with voluntary cooperation. As he explains, these are coordination games, where participants perceive the potential for mutual benefit from collective action. A voluntary process should not seek to resolve adversarial or prisoners' dilemma games which require participants to act against their perceived interests.

APEC's leaders understood this at the outset and set up a forum for playing out coordination games, such as harmonising administrative procedures and mutual recognition of various standards. On the trade policy front, APEC sought to sustain, and possibly accelerate, the ongoing trend of unilateral liberalisation; while recognising that some 'sensitive sectors' would not be fully liberalised in the near future, if ever, on a voluntary basis. At the same time, APEC intended to defend and strengthen the WTO, which provides the context for confident 'opening to the outside world'.

This was a realistic set of objectives and APEC has achieved some useful things in its areas of comparative advantage. It has been effective in many aspects of trade and investment facilitation, which are perceived as positive-sum games. For example, APEC has done a lot to reduce the costs of clearing customs (with considerable economic gains, since these costs greatly exceed tariff rates for the vast majority of traded goods). APEC also helped East Asian governments to reject a retreat to systematic protectionism in response to their serious financial crises in the late 1990s. Ravenhill does not give any credit for these achievements, while castigating APEC for not achieving the impossible.

Ravenhill notes that, when APEC was established, it would have seemed 'preposterous' to expect that APEC would deliver fully free trade in the foreseeable future. That is quite true. But he then fails to explain why APEC subsequently set itself the target of free and open trade and investment by a fixed date. Moreover, APEC leaders created the expectation that the APEC process itself would be the vehicle for meeting this target. Free and open trade and investment by 2020 may yet be feasible, but can only be achieved in the WTO.

So, why did APEC commit itself to what it cannot do? A large part of the blame lies with the Eminent Persons Group (EPG) which operated between 1993 and 1995. Its reports sought to turn APEC into a trade organisation, downplaying the significance of coordination games, while advocating a focus on adversarial or prisoners' dilemma games. Essentially, the EPG recommended that APEC neglect its comparative advantage in order to concentrate on what it was never designed to do and what had no comparative advantage at all over the WTO.

The main weakness of the book is that Ravenhill repeatedly describes the EPG's input as intellectual leadership. It may have been leadership, but it was hardly intellectual. It would have been more useful to address how an eminent group could have got it so wrong, and why APEC leaders accepted such advice.

Ravenhill notes the dominance of the EPG chairman, Fred Bergsten from the United States, but does not explain that the recommendation for APEC to become a trade organisation with a fixed date for full liberalisation of all trade was Bergsten's dream. It was strongly opposed by the rest of the group, which included Professor Ippei Yamazawa from Japan, Dr Narongchai Akrasanee from Thailand and Dr Noordin Sopiee from Malaysia.

The Australian Government of Paul Keating swallowed the Bergsten view, hook, line and sinker, acting as the main advocate for setting the Bogor goal of free and open trade and investment by 2010 for developed and 2020 for developing economies, respectively. Following the acceptance of this goal by President Soeharto it was adopted by APEC leaders in 1994.

The stage was thus set for disappointment and division, unfortunately along essentially racial lines. APEC economies kept up their 'opening to the outside world' in important areas of trade and investment facilitation where they perceived mutual benefit, but refused to liberalise their remaining 'sensitive sectors' within the APEC process. However, successes were undervalued and opportunities for productive economic and technical cooperation were too often missed.

A growing obsession with the liberalisation of 'sensitive sectors' led to the so-called early voluntary sectoral liberalisation (EVSL) experiment, when all pretence of voluntarism was dropped. That in turn led to the debacle of the Kuala Lumpur meeting in 1998.

After that, Asian members became less interested, not only in action within APEC, but also in defending an open non-discriminatory trading system. The current fashion is preferential trading arrangements, which are in stark contradiction to APEC's basic principle of open regionalism as well as to the fundamental non-discriminatory principle of the WTO. That is, as Ravenhill points out, a crucial failure of the APEC process.

The constant efforts to depart from voluntarism have also revived a strong interest in an East-Asia-only grouping. As Ravenhill notes, the ASEAN + 3 (China, Korea and Japan) is a renaissance of the East Asian Economic Caucus proposed by Malaysia in 1990. This is weakening the prospects of *trans*-Pacific cooperation and Australia has itself to blame.

Can APEC be fixed? Ravenhill correctly concludes that further efforts to transform APEC away from a voluntary process would be counter-productive. The preferable alternative is to adopt an agenda 'consistent with the type of institution that members are willing to sanction'. In a voluntary process, these are cooperative arrangements where mutual benefits are available and compliance is self-enforcing. Since 1999, APEC has moved in that direction.

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Is There Progress in Economics? Knowledge, Truth and the History of Economic Thought, by Stephan Boehm, Christian Gehrke, Heinz D. Kurz, and Richard Sturn (eds.) (Edward Elgar, Cheltenham, 2002), pp. xxii + 410.

Is There Progress in Economics? consists of 23 papers originally presented at the meetings of the European Society for the History of Economic Thought (ESHET) held at the University of Graz in 2000. The conference theme became the book's title question. As the editors say in their foreword, the subject of progress in economics was clearly an appropriate theme for the first ESHET conference of the new millennium.

Conference volumes, particularly those originating from broad-based society meetings, are notoriously uneven. Often there is wide variation in the intellectual quality of the contributions, but even with adequate quality control, difficulties invariably arise from a lack of coherence; papers are frequently bunched together in relatively ad hoc ways with little but the inertia of section headings to hold them together. In this respect, *Is There Progress in Economics?* should be given relatively high marks. First, the quality of the papers is quite high. and second, the editors did a relatively good job of selecting, arranging, and editing the contributions so that the volume really does focus on the question in its title. The editors' introduction also contributes to the overall effect by attempting to tie all of the papers together into a reasonably tight bundle.

This said, I would note that I modified both 'good job' and 'high marks' with the word 'relatively.' This is because the volume does have a few papers that focus exclusively on specific debates within the history of economic thought and make little or no effort to connect those debates with the broader theme of progress in economics. While these papers may represent significant contributions to their own particular sub-fields, they seem a bit adventitious and would probably not be of interest to those who would pick up the book to find out what historians of economic thought have to say about progress. Although several of the papers fall into this category, these contributions are effectively camouflaged so that the book still comes off as very focused. This is accomplished in part by putting the papers that are most clearly about progress right up front, the ones that involve some discussion of – but do not focus exclusively on – progress in the middle, and ending with the three papers in Part VIII which neglect the general question entirely. (They involve an in-house debate about the character of the classical long period equilibrium.)

Given the number and variety of papers in the volume, it is beyond the scope of this review to try to say a little something about each of the various contributions. Rather than attempt such a summary, I will focus my discussion on just two of the papers, in fact the very first two: Chapter 1 by Donald Winch and Chapter 2 by Mark Blaug. These two papers lead off the volume and set the tone for much of what follows. Together they constitute Part I. Part II contains three other papers addressing the general question of progress in economics (Hamberger, Birner, and Cremaschi), followed by four papers in Part III (Backhouse, Mäki, Pasinetti, and Streissler) which comment on the previous chapters. Most of the 11 papers in the next three sections, Parts IV to VII, are also concerned with the question of progress, although they approach the subject from a different direction. Rather than focus on the question of progress in general, these papers take a case study approach; they examine particular sub-fields in economics – most contemporary, but some not – and attempt to address the question of progress within these various sub-fields. There are many important papers in these sections, but I would like to note two as particularly significant: Philippe Mongin's paper on normative economics (Chapter 10) and Stephen Meardon's examination of the new economic geography (Chapter 13).

According to Winch in Chapter 1, the question of progress is very important for the history of economic thought. But it is not important because historians need to differentiate epistemically good economics from epistemically bad economics; it matters to us because it mattered to them. As Winch puts it: 'Progress matters to us as historians because it mattered to many, perhaps most of those about whom we write' (p. 17). Historians of economics do not need to take sides on the question of whether economist A's work constituted empirical or theoretical progress over the work of economist B, but do need to take sides on the question of whether A (or protagonist C) thought it constituted progress and why they thought so. In this sense, historians are concerned with progress, but it is only the type of progress that mattered to the relevant historical agents, not progress by the standards of contemporary economists, economic methodologists, or some particular philosopher of science. Progress matters, but it is always embedded and contextual.

Mark Blaug disagrees. While he no longer seems to require the prediction of Lakatosian novel facts, he continues to insist that a criterion for the growth of economic knowledge is essential for the history of economic thought and that the proper one is empirical progress – 'a greater capacity accurately to predict the outcomes of economic action and, hence, to control these outcomes at least to some extent' (p. 22). Why is such a universal standard for demarcating knowledge from other types of scholarly output necessary for historians of economics? Because without it, Blaug argues, historians would lack the most important single tool for appraising the work of previous economists. In particular, historians would be unable to criticize historical developments like the formalist revolution: the 'disease' that has gripped the profession 'since the end of World War II' (p. 34). Formalism – characterised by the Arrow-Debreu general equilibrium theory in microeconomics and rational expectations, new classical theory, and real business cycles in macroeconomics –

is the 'worship of technique' (p. 36) that directs inquiry toward 'solving intellectual puzzles that economists themselves invented instead of addressing problems encountered in the real world' (p. 35). For Blaug this is clearly the wrong road; historians have an obligation to say that it is the wrong road; and universal standards for scientific progress are necessary for such protestations.

As I said, these two positions effectively set the tone for the discussion of progress by most of the other authors in the volume (those concerned with progress in general as well as those focusing on one particular area of economic inquiry). Almost everyone appears to think that progress is an important subject for historians of economics, but for some it is the more Winch-like concern for relatively local, historical, or context-dependent criteria; and for others it is the more Blaug-like desire for universal epistemologically inspired demarcation criteria. While both authors, and many of the others who consciously or unconsciously end up siding with one of their views, offer rousing defenses of their positions, I am afraid I must ultimately side with the historians rather than the methodologists; that is, with Winch's local and contingent, rather than Blaug's universal, standards. Both are difficult to identify, but surely determining the standards employed by a particular person, or in the air at a particular point in time, is far more feasible than identifying a demarcation criterion that successfully separates 'knowledge' from all other things that humans say. While it may be difficult to come to agreement about the relevant local historical standards, it is not impossible; while the last hundred years of philosophy of science makes it quite clear that no such agreement exists at all regarding universal standards for the demarcation of scientific knowledge in general. On this question, as with so many others, it seems that practicality, humility, and history all end up on the same side. Of course this is just the opinion of one particular reviewer; it is also my opinion that this is a very good book – well-edited, containing well-written and well-researched papers, and for most part concerned with the progress in economics – but I suspect this latter opinion will be far less controversial than the former one.

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Economics and Language: Five Essays, by Ariel Rubinstein (Cambridge University Press, Cambridge, 2000), pp. 128 + viii.

Before one opens the cover of the printed version of Ariel Rubinstein's profound and austere Churchill Lectures in Economic Theory, with postscripts focusing on technical aspects of the lectures by two economists (Tilman Börges, Bart Lipman) and a logician (Johan van Benthem), one sees an image of the author's father in Jerusalem in 1947 buying bread from a local baker. This 'simple' act of exchange is accompanied by vigorous gesticulation. We observe trade in the midst of talk, and talk in the midst of trade.[1] And just what are we to make of this? On reflection it is a peaceful image. So begins Rubinstein's re-opening of a project mostly dormant since the days of Adam Smith, using the same rational choice considerations to explain aspects of language which we use to explain the exchange of goods.

Also akin to Smith's approach, the rational choice considerations which are used to model the decisions of ordinary language users are the same in structure as the rational choice considerations of the modelers themselves. There is in Rubinstein's account no heterogeneity of motivation which so disfigures so much modern economics wherein the subjects of economic theory pursue the private good of happiness, whereas those who theorise about these subjects are presumed by themselves to pursue the public good of truth.

Each of the chapters has a problem:

1 Why in ordinary language are linear orders so common, that is, why do we arrange things on a line and not in a circle? (A linear order is formed by 'taller' when I am taller than my son and he in turn is taller than his; I am *therefore* taller than my grandson.)

[1] This is what Rubinstein wrote to me about the image: 'I was looking for a picture of people debating. Art experts told me that there is none in the impressionist tradition. So I called my favourite Israeli photographer and asked for a photo, he searched and came back with the message: "all debates of Israelis involve some violent body moves." So, I did not know what to do and my wife, Yael, suggested I put this picture, that was *not* meant to be connected to the book, but from the mere facts that (1) the person buying the bread is *my* father; (2) I love both the picture and the issues of the book (not the book itself) very much.' Historians of economics will recognise that Rubinstein has encountered the thesis that trade pacifies in a surprising context!

2 How is it that 'Be careful' is understood as a warning?
3 How is 'It is not raining very hard' understood to mean 'it is raining, but not very hard'?
4 Why do economists resist working with lexicographical preference orderings?
5 Is the 'strategy' in game theory used rhetorically?

I will confine my comments to a non-technical interrelation of the first and fourth problem. The fifth problem takes off from the McCloskey's famous discussion and ought to be looked into by anyone who is persuaded that game theory has easy applicability to the real world.

Rubinstein starts with a factual claim: transitive linear orders are enormously common in ordinary language – taller, stronger, faster, smarter, prettier, fatter – relative to intransitive circular orders. We find circular orders in games which are meant to pass time – rock breaks knife, knife cuts paper but paper covers rock – and where 'efficiency' would have a football game played with 22 on one side and 0 on the other. Rubinstein's argument is a linear order has the efficiency property of being 'indicator friendly' which is to say that 'linear orderings are the most efficient binary relations for indicating every element in every subset'. (13) They can point out directions with less cost than any other order. Language is not meant only to pass the time amusingly.

I think the supposition that 'real orders' – my phrase, not Rubinstein's – ought to be transitive is what lay behind the shock which greeted the Arrow-Black demonstration of circular majorities in voting. Perhaps if the Athenian democratic practice of election by lot had been remembered, the profession might not have been so puzzled by Buchanan's observation that intransitivity is the point of democracy. Perhaps also the supposition that ordering ought to be linear can explain why Jenkin's image of exchange as a circular order, something akin to a dance without a leader, had so little impact (Levyand Peart 2001–02, #5).

We leave the considerations of the subjects of economic theory and address the considerations of economic theorists. 'Why does the utility function $(\log x_1 + 1)x_2$ in a two-commodity world lie within the scope of classic textbooks whereas lexicographical preferences do not?' (56) This I propose is the central question about the role of economists in the real world. Why is it that we have renounced lexicographical orderings?

Both Rubinstein's textbook utility function and lexicographical orderings are linear orders so they will make competing claims which can be translated to the considerations of ordinary language. Think about these two orders as guides which the economic theorist brings with him to talk to ordinary people about their choice. How many ways are there for an ordinary person to be attain happiness? Can the ordinary person maximise utility in diverse manners?

The textbook utility function has, and the lexicographical order does not have, indifference. Indifference allows the ordinary person to attain the goal of utility maximising in a diversity of ways. When the order is lexicographical, the theorist's way is the only way. There is only one way to attain some agreed upon level of utility. Here is the choice facing theorists: which sort of ordering do we offer ordinary people in exchange for goods. Here is the way of Adam Smith:

> The man of system . . . seems to imagine that he can arrange the different members of a great society with as much ease as the hand arranges the different pieces upon a chess-board. He does not consider that the pieces upon the chess-board have no other principle of motion besides that which the hand impresses upon them; but that, in the great chess-board of human society, every single piece has a principle of motion of its own . . . (1759, VI.II.42) (Smith 1759).

Here is the way of Frederick Nietzsche:

> A thousand goals have there been so far, for there have been a thousand peoples. Only the yoke for the thousand necks is still lacking: the one goal is lacking. Humanity still has no goal.
>
> But tell me, by brothers, if humanity still lacks a goal – is humanity itself not still lacking too?
>
> Thus spoke Zarathustra. (Nietzsche, 1954; p. 170)

Before there was Nietzsche, there was Carlyle who labelled the political economy launched by Adam Smith the 'dismal science' precisely because it had renounced the hierarchical way implicit in the lexicographical. (Levy and Peart 2001–02). But this history does not answer Rubinstein's 'why?' But the historical fact suggests that answering Rubinstein's question is necessary for understanding the place of economists in the world of ordinary people.

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The Big Problem of Small Change, by Thomas J. Sargent and François R. Velde (Princeton University Press, Princeton, 2002), pp. xxi + 405.

How did the market economy evolve from a pure commodity money system to a fiat money system? This impressive book attempts to explain a significant part of the transition. By showing how Western economies solved the problem of providing small change. The argument in a nutshell is that there are technological impediments to any commodity money system providing a variety of denominations. It required innovations in both monetary technology and monetary theory to complement a commodity money system with a token coinage. These innovations made possible the token coinage, and so were a crucial step on the way to fiat money. The book tells the story of the impediments and how they were overcome, a story that is of interest both as a piece of monetary history and as an example of the evolution of a fundamentally important economic institution.

The book begins with a model of a commodity money system, and then uses the model to tie together the history of monetary technology, the history of monetary thought, and the history of monetary systems. This review will follow the same path.

The model is of a monetary economy with coins of two denominations, high and low, and a restriction that some goods cannot be bought with high denomination coins. The mint buys metal at a price (MP – measured in units of account per unit weight) and produces coins of given weight and fineness. The coins are given a value in unit of account which implies a unit of account of minted coins per unit weight (ME). The difference between MP and ME is the mint's net revenue per unit weight, a combination of costs of production and pure seignorage. One objective of the monetary authority is to choose values for the four parameters (the MP and ME for each coin) that are consistent with concurrent circulation of both coins.

The model suggests: (i) that there are a range of 'exchange rates' between large and small coins that are consistent with equilibrium; (ii) that the possibility that agents will duplicate coins limits the seignorage rate that can be imposed; (iii) that exogenous factors such as changing values of precious metals, or differential wear can cause a shift from an equilibrium with concurrent circulation of both coins to a 'shortage' of small coin; and (iv) that debasement of a small coin or increase in valuation of a large coin can eliminate such shortages.

Thus the model makes sense of a variety of the patterns in the history of money over the period between Charlemagne (~800AD) to the nineteenth century. It also suggests a way to resolve permanently the problem of small change, the 'standard formula' described by Cipolla: issue a limited number of small denomination, convertible, token coins on government account.[1] But if the answer is so obvious, why was it not implemented by – if not Charlemagne – the Venetians in the thirteenth century, who endured myriad difficulties as they tried to expand the range of denominations? The answer, according to Sargent and Velde, is a combination of not having the technology to prevent counterfeiting – a necessary precondition for the standard formula – and not having a monetary theory that would permit it. They argue that there have been three technology regimes, such that the difficulty of counterfeiting increased with each regime shift. Coins were first minted using a hammer and die technology, and were very easy to counterfeit. From 1550 screw press technology was applied to minting coins, and were still fairly easy to counterfeit. Finally, in the early 19th century, mints began using a steam-driven minting technology that created coins that were very difficult (i.e. expensive) to counterfeit.[2]

[1] The model permits other solutions, for example, a floating exchange rate between the two coins, or only small coins in circulation. In essence these solutions reflect the necessity to abstract from the complexity of the monetary problems. I, at least, am happy to accept that there were constraints that aren't modelled explicitly that precluded either solution as an efficient outcome.

[2] While accepting this basic regime structure, George Selgin has challenged the notion that steam power was the essential characteristic of the third regime.

Turning to the history of thought: under Charlemagne the penny was the unit of account and the medium of exchange, and the Roman view that money should be valued by its intrinsic value was consistent with the monetary standard. But the evolving monetary system created a number of problems for this view. When different states reduced the silver content of the penny, did a debtor owe a fixed quantity of silver or a fixed number of pennies? Did a state have the right to alter the silver content of the penny? The authors argue (p. 100) that despite these challenges, the Roman view remained the *communis opinio* until around 1500. Then, during the Renaissance, scholars established three new principles in monetary theory: coins can have value in excess of their intrinsic value, they could in fact have zero intrinsic value, and the price level would depend on the quantity of coin in circulation. According to Sargent and Velde these principles opened the door for the issue of token coin and subsequently fiat money.

Having set the stage, the authors use their model, and their understanding of the history of monetary technology and contemporary monetary theory, to interpret the evolution of monetary systems across Europe. To a large extent their analysis builds on the careful data collection of others (e.g. Cipolla (1982), Lane and Mueller (1985) and Bernocchi (1976) for the Italian city examples) but they have done a considerable amount of primary research and the breadth of their examples is remarkable. Furthermore, the historical examples support the usefulness of the model that they have introduced as a framework for the analysis. We see the difficulties of the multi-coin standards – the complaints of shortages and the debasements as the monetary authorities attempt to redress the problems. The authors show how the improved technology of the early modern period, combined with the innovations in thinking about money, lead to experiments in providing small denomination tokens that culminate in the successful introduction of the 'standard formula' in Germany in the 1830s.

So far, so good: the story as told is broadly compelling. But let me raise three interrelated caveats. None alone alters the significance of the details of the story, but together suggest a somewhat different bottom line.

First, the issue of '*small*' change: The reader could be forgiven for thinking that the authors are assigning a pivotal role to the equivalent of nickels and dimes in the history of money. In part, this is so, but if it were only 'nickels and dimes' I don't think there would have been such a '*big*' problem. The Big Problem was that some of this change was not so small: consider the 'blanc', a fifteenth century coin of less than 50 per cent silver; it was valued at 10 pence. or about a third of a day's wages – this is closer to \$10 than 10 cents.[3]

This leads me to the second caveat: the 'convertible tokens' that made possible the variety of denominations with a commodity anchor, and paved the way for fiat money, were not only token coins, but – possibly more significantly – token bank notes. The introduction of multiple coins (large silver and gold to supplement the Carolignian penny coinages) occurred in the twelfth century when European trade expanded. The economic expansion of the eighteenth century was arguably equally dramatic, and the monetary needs of this expanded economy were met in part by the spread of privately issued convertible bank notes. As with token coins – and in part using the same arguments – monetary theory had to adapt to consider the role of bank notes in the money supply.

Thirdly, the use of privately issued bank notes to supply high denomination monies makes clear that the 'standard formula' with its emphasis on government issue and 'limits' is not the only solution to the problem of multiple denominations, and may not be the optimal solution. Convertible notes could be issued by the private sector (and were in many countries), and competition would limit the quantity endogenously.

Some readers will ask whether it was necessary for the authors to deploy the algebraic apparatus they use to organise their story. There are clear costs and benefits: the cost is that the apparatus may deter readers who would otherwise get a lot out of the story; the benefit is that the rigor of the argument makes it more compelling to an economist. But I tend toward their interpretation of history. A 'small coin' is defined only in the context of the model. and questions such as 'Is the standard formula the only solution?' (the optimal solution?) similarly require a model to be formulated and answered. Thus, I see the model as an essential part of the argument.

I end where I began: this is an impressive book. It makes a contribution on a number of levels. On

[3] Or do Sargent and Velde take the blanc to be a large coin? There were indeed smaller coins, but there were also larger silver coins, as well as a range of more valuable gold coins. The model and technology suggest that there will be difficulties with any two coins implying that in this context the blanc was a small coin.

the micro side, the case studies that the authors examine will be useful components to national economic histories, but the key to the merit of the book lies in its broader contribution to the history of money. Money is a social institution that is fundamental to the economic world in which we live, and understanding how this institution evolved – the nature of the ideological and technological constraints that influenced that evolutionary path – is a necessary input into any analysis of the future of the monetary system.

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Retail Pricing Strategies and Market Power by Gordon Mills, (Melbourne University Press, Carlton South, Victoria, 2002), pp. x + 341.

This book in many ways recreates the once-grand Australian applied microeconomics tradition of taking some simple theory and using it practically – in this case, to analyse the pricing of retail goods and services. Mills weaves a good blend of theory, description, analysis and policy in a scholarly and informative presentation. A reader with no training in economics could read much of the book and become a better informed consumer, in the sense of knowing how prices are set and how to evaluate competing offers from rival sellers. Students could use the book profitably as a primer in some of the pricing practices engaged in by firms with market power, most notably price discrimination. Academics can use the book as a source of references for work on retail pricing over a lengthy period, and for ideas on future research. It is a book therefore of considerable appeal.

Mills first presents the basic theory that underlies pricing practices like price discrimination, and explains the phenomena of consumer search, price dispersion, product differentiation, brand signalling and unit pricing, largely in the context of supermarket pricing and pharmaceuticals. A chapter also explains bundling (readers without any formal economics education might find the explanation of the concept heavy going), in the context of hamburgers, travel and other services. The reader is then guided through the pricing of medical services, foreign exchange, personal bank accounts, conveyancing, and domestic air travel. Mills shows that price discrimination is present in many everyday transactions, and his exposition is a balanced one, noting often that its welfare effects are not necessarily such that it should always be regarded pejoratively. The final two chapters of the book pose the interesting – and difficult – policy questions of whether governments should regulate price discrimination, and how information is best conveyed to consumers.

The book has many strengths, of which more to come, but two weaknesses. It is irritating to find that the empirical work on which this book, published in August 2002, is based was mainly carried out in 1991. However, no claim is made that the book contains up-to-date analyses of pricing behaviour, and to be fair, Mills tells readers in his preface that the empirical chapters depend on 'detailed and laborious fieldwork, which I have undertaken over the last 10 years or so', and justifies its use on the basis that it 'serves to illustrate general principles that are here to stay'. It is a matter of caveat emptor after that!

On top of this, the samples on which the analysis is based are generally very small. Given these (often judgement-based) sample sizes, it is best to regard the results as explaining some observed past pricing patterns, rather than as the outcome of formal statistical analyses of current retail pricing practices. The conclusions drawn are not necessarily applicable to retail pricing in 2002. In many cases the institutional factors at work have changed markedly. However, in some chapters Mills does provide a brief account of recent developments, especially with respect to airlines, but accurate up-to-date statistics are not always provided. The book is therefore best read for its discussion of the principles of information search and delivery, rather than for a guide to current retail pricing.

But now let me accentuate the book's positive aspects. The different types of price discrimination, and the conditions needed to put them into effect, are carefully explained. The diligent reader of chapters 3 and 4 will look differently at bags of budget apples in the supermarket from now on, and coffee drinkers may rethink their coffee-buying habits and allegiance to name brands, when they realise that often the same (or not much lower) quality can be bought for a much lower price than they pay for the market-leading brand. In addition, there is a warning in Chapter 7 that buyers should not assume that larger packages will cost less per standard measure of weight or volume, especially in the case of toothpaste, flour, snack foods, paper tissues and detergents.

The chapters on individual industries offer different insights, but mainly always around the theme of price discrimination and price dispersion. Some are much more useful than others, at least in a practical sense. For example, Chapter 8 on pharmaceuticals is based largely on 1991 data and policy settings ranging from 1991 to 1997. However Mills' conclusion is doubtless still valid: because of brand price premiums and considerable price dispersion between pharmacies, buyers should shop around for the best price on private and PBS prescriptions. More useful is Chapter 9 on retail foreign exchange. The data are from November 2001 (but only observed for one day), and the findings and their explanation, especially sorting out what the 'buy' and 'sell' rates mean, take the mystery out of retail foreign exchange dealings. The message is to shop around, and ask a few questions. Mills makes a plea for the mandatory posting of accurate information to reduce buyers' search costs, to allow a premium to be charged for higher service levels, and to lead to a more competitive market. More of the same is presented in Chapter 10 on personal bank accounts. However the data here relates mainly to 1991–92, and much has changed in the banking market since then. Nevertheless Mills provides a useful guide on how to compare the merits of different banking account options, as well as pointing out how banks knowingly engage in price discrimination between different customers, and noting how consumers can take steps to avoid it.

Chapter 11 on airline travel is relatively up-to-date and therefore provides valuable information for travellers, in the context of discounting mechanisms, seat availability, and the role played by travel agents. The latter may not provide unbiased information, and Mills recommends that consumers engage in search of their own for the best fare. He argues that price discrimination is rife in the sale of airline seats, although some of the price differentials can be partly cost-justified, and he shows how Qantas and Ansett used price discriminating strategies to respond to the entry of Impulse and Virgin, but absolves Qantas' price discrimination from being a significant determinant of Ansett's demise.

I found Chapter 12 on conveyancing to be somewhat out-dated in its main data base and of less general or analytical interest. Of wider interest is Chapter 13 on medical fees, especially for GP services. Even though the chapter is based on 1991 data, we find clear evidence that GPs charge different customers different fees (stemming from a mixture of charitable feelings toward some patients and revenue-seeking price discrimination), and that fees vary across locations. Mills also finds that billing and fee information was not commonly displayed in waiting rooms. While his pleas for more information are timely, the problem still remains as to what use it would be put, if any, by consumers of medical services.

The final two chapters present Mills' policy conclusions. He looks at how governments have regulated price discrimination and other pricing practices permitted by the possession of market power, both in general terms as well as specifically in the US, the UK and Australia. The treatment is a little unfocused, and the target audience it is not readily apparent. He introduces new material on price discrimination (that initiated by governments, as well as in telecommunications, airlines and banks), but there is little detailed analysis of what has been done about it in specific fact situations in Australia, perhaps because 'price discrimination is a very difficult issue for a regulator'. Mills does, however, identify three key policy issues: (i) price discrimination may lead to greater quantities sold, so to prohibit it could preclude a welfare gain; (ii) the distributional effects of price discrimination should be evaluated for their social acceptability; and (iii) price discrimination can be used anticompetitively by firms to alter market structure in their favour.

The final chapter provides an excellent discussion of the principles underlying consumer search, and of how consumers use the information thus gathered. Sources of price information are discussed, with a focus on the signals given by posted prices. For markets with multiple price components, such as supermarket purchases, Mills considers overseas research on the desirability of the independent publication of price comparisons, and concludes that it is desirable. He also considers products with complex price bases such as home loans with differing interest rate provisions, and bank accounts which offer various combinations of interest rates and timing of interest payments. Other issues touched upon briefly include undisclosed commissions; the influence of undisclosed ownership when sellers make recommendations for one product over another, when consumers are relying on the seller's impartiality to produce the best deals for them; switching costs; whether governments should regulate for the provision of information to consumers (no clear answer); the commercial provision of information; the ways in which information should be provided; and labelling.

Ultimately, however, consumers need to know what to do with retail information, which points to the need for greater levels of consumer education. With education, consumers would at least be better equipped to sift through all sources of information, whether provided by governments, third party providers, sellers or acquaintances. But how best to provide it, where it should be provided, and in what amounts, is the big question. Mills points the way for further research into this important aspect of consumer protection.

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The Economics of Language, by Donald M. Lamberton (ed.) (Edward Elgar, Cheltenham, 2002), pp. xxvi + 330.

Why are we fascinated by 'economics and language'? Is it because of the contrast between the materialistic economics and the humanistic language? Is it because of the feeling that language is fundamental to our behaviour, yet is absent from our standard 'economic models'? Or is it simply because it sounds exotic? Since my early days as an economist, 'economics and language' has fascinated me. But as I grew older I became more of a sceptic. I came to believe there is another reason why so many people are fascinated by this combination of words – it is vague enough that everyone can impute his own meaning to it.

What still appeals to me about 'economics and language' is the idea that we can investigate linguistics using economic methods. For example, we can use game theoretical evolutionary models to explain the evolution of words, meanings and grammar. We can think about the function of pragmatic rules of language as an outcome of an implicit attempt to economise language. We can look for the rationale for our reasoning being organised around certain concepts using some complexity considerations. I am also fascinated by the attempt to model limits on the language of economic agents and to apply such limits in a way which alters the standard conclusions of our economic models. And, like many others, I am interested in the attempt by economists to study the language of economics and to clarify the rhetorical tricks used by the profession. Indeed, some of the above points were touched on in my own short series of lectures, titled 'Economics and Language' (see Rubinstein, 2000).

The scope of the collection under review, *Economics and Language*, is quite narrow. To understand how the collection relates to language, it is useful to draw from the literature on 'club models'. Think about a 'club' as an entity which includes all speakers of a certain language. Each agent is born belonging to one club (of his native language) and has the option of joining other clubs. It is assumed that belonging to many clubs is beneficial for his trading options. However, learning a language is costly. In other words, an agent who knows additional languages will improve his productivity but must bear the cost of learning them. An equilibrium analysis is therefore called for to deal with the array of questions that arise.

The topics discussed in the book are of special interest in multilingual cultures, as well as computer environments. It examines the implications of being a multilingual society on political and economic structures. It relates to issues such as inequality which are rooted in differences in language within a society.

Personally, I found the body of research presented in the collection to be less than exciting. It uses only traditional 'economic variables' and applies 'elementary tools'. Indeed, as the editor says, this is a neglected area, however, it is also a very narrow field which makes the need for such a collection questionable. Scholars who are interested in this field would probably be better off reading a good short survey paper. The topic has been surveyed in a special issue of the *International Journal of the Sociology of Language* (see Grin, 1996).

Further, the production of the book is not the best. The index does not cover the introduction. All papers are put into the same size of page which makes reading some of them a less than comfortable experience. But the big question is whether at this point in time such a book should be published (even if its production were improved and its range of topics widened). It took me only a few minutes to download five of the better papers in the collection from the internet (5, 7, 8, 14 and 19). Would it not be sufficient to publish the reading list on the web with the appropriate links (when available) and a short introduction (or survey), thus saving individuals and libraries the book's cost (through Amazon) of $US125?

There are now 158 titles in the 'International Library of Critical Writings in Economics' printed with golden letter titles on a hard blue cover. Most of the papers within these collections can be found on library shelves or easily downloaded. The prices of the books are . . . well, the reader is invited to judge by himself. I myself made the mistake of editing one of the early collections in the series and was astonished to discover that the book, containing 36 articles, is priced on Amazon at $US285.

Why should a library buy such a book? I do not have any idea! The publication of hundreds of titles in this format must be the outcome of a worrying imperfection in the academic market. Let me use this opportunity to voice my personal opinion that libraries should not buy such collections and to recommend that authors, instead of publishing expensive volumes like this one, publish lists of recommended papers on the web for the free use of the scholarly public.

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How To Argue With An Economist: Reopening Political Debate In Australia, by Lindy Edwards (Cambridge University Press, Cambridge, 2002), pp. vii + 172.

Lindy Edwards, a former adviser to Natasha Stott-Despoja and now a graduate student at the Australian National University, aims to acquaint the general reader with the thinking of economists, who have allegedly taken over the policymaking apparatus of the Australian government – a charge made famously a decade ago by Michael Pusey. Having shown the narrow doctrinal view of the world possessed by economists, Edwards sets out to explain how the uninitiated can challenge economists in their own language.

Regrettably, this book reiterates some tired old criticisms of the economics profession, and would not enable a reader to effectively argue with an economist in a policy debate. Like Pusey and other noneconomists, Edwards misses the point when criticising economics. Edwards argues that there are policy considerations other than purely economic ones which economists ignore. However, economists do not ignore these considerations, but rather deal with them under the rubrics of community service obligations and market failures.

The book does have a number of positive features however. Edwards makes her arguments within the framework provided by Maslow's hierarchy of human needs, which does in fact remind us that economics can only help us satisfy basic material needs. We cannot rely upon it to meet higher-order needs such as the need to be loved or to live up to our own expectations, although we cannot rely on government policy in these matters either. Quite obviously, economics can give us a bigger GDP, but it cannot save the world.

One of the highlights of the book is in Chapter 3, where Edwards describes how government is actually run, as a conflict between bureaucrats and politicians. The bureaucrats, though they control significant fiefdoms, have to kow-tow to the ministers and their often youthful and inexperienced staffers, while the ministers and staffers 'shuffle in their leather seats at the façade of their superiority' (p. 20). Also illuminating and amusing is the concise summary of the power structure in the Canberra bureaucracy, with the top bureaucrats in the central agencies (Prime Minister and Cabinet and Treasury) lording it over the lesser functionaries in the so-called 'line agencies' and all but the most senior politicians.

As good as it is, however, Edward's description of the workings of government in Canberra actually runs counter to her main argument. To point out that 'high-level decision making occurs at the level of broad concepts' (p. 28) is precisely what many economists would maintain is a serious shortfall of government. Policy proposals should not be passed on the basis of ideals or vague notions of what is fair, equitable or efficient, but only after a rigorous cost-benefit analysis. It is the rigorous analytical framework of economics that is most lacking in the upper echelons of governments across Australia.

Another negative is that in making the claim that there is such a thing as society, Edwards has in addition to becoming tiresome, failed to acknowledge the reality that there is no sensible way to aggregate the preferences of individuals to come up with the optimal combination of private and public goods and services. Edwards commits the same sin that economists are often accused of making in assuming a 'representative agent', by talking about 'the punter', the average Australian and what he or she thinks. But is there a representative member of the community in a community of individuals, and how is Edwards sure she is adequately representing the view of 'the punter'? How do we really know what people want unless they actually vote for it at the ballot box?

Edwards also makes the claim that economists are too busy worrying about technical as opposed to allocative efficiency. However, much of recent microeconomic reform, especially tariff reform, has promoted more efficient prices, as has Alan Fels' work in relation to monopoly pricing. Edwards is therefore clearly incorrect in asserting that economists have neglected allocative efficiency.

The idea of economic rationalists having committed a coup d'état and taken over the Australian government is farcical. The fact is that the government still substantially subsidises basic public services, beyond the level 'economic rationalists' would accept as justifiable on the grounds of community service obligations and market failures. Even making allowances for inertia in the political process, the current federal government has strayed from a pure economic rationalist line on many fundamental policy issues. Two issues which readily come to mind are immigration, where the government is acting against the classical liberal belief in the free movement of persons, and the first-home owners scheme, a blatant Keynesian stimulus to the construction industry.

How to Argue with an Economist is in many ways a political document. I do not think economists will gain anything from it. You will have heard all of these criticisms of economics before. Nonetheless, it could be useful for fresh economics graduates beginning careers in the public service, who might appreciate an insight into the criticisms that will be thrown at their discipline. Economists interested in evaluating more rigorous critiques of neoclassical economics may be interested in reading either John Quiggin's (1996) *Great Expectations* or Brian Toohey's (1994) *Tumbling Dice*.

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Literature, Money and the Market from Trollope to Amis, by Paul Delany (Palgrave, Basingstoke and New York, 2002), pp. v + 243.

Literature, Money and the Market from Trollope to Amis discusses 'Land, Money and Identity in Trollope', 'The Market for Women', 'Money, Marriage and the Writer's Life: Gissing and Woolf'; 'Conrad and the Economics of Imperialism: *Heart of Darkness*', '*Nostromo*: Economism and its Discontents', 'The New Literary Marketplace, 1870–1914', 'English Literature and Rentier Culture', 'Paying for Modernism', and 'T.S. Eliot's Personal Finances, 1915–29'. As this list of chapter headings indicates, Delaney's concern is with literature's relationship with economic circumstances in England in the nineteenth and twentieth centuries, and his method is the inspection of useful samples.

Sometimes the vista is critical: he discusses the representation, within works of fiction, of economic factors. Delany notes particularly the ways in which Trollope, Gissing, James and Conrad, responding to the development of a 'consumer society', expressed their distrust of 'market forces'. At other times, the vista is contextual: he discusses the impingement of economic factors on the lives and productive powers of individual writers such as Virginia Woolf and T.S. Eliot. The background is the evolution of the mass market as a consequence of such forces as the Victorian education acts, technological advances in the production of books and magazines, and the emergence of vast publishing conglomerates.

One argument of the book is concisely rendered on p. 16:

> 'Rail as they might against the market, authors belonged to it; and their natures were subdued to what they worked in'.

A more precise elaboration appears at the end (p. 191):

> 'It may seem a depressing prospect that the future of English literature should be so controlled by these market institutions . . . But if any margin of innocent cultural production remains, sheltered from global market forces, it is surely a shrinking one . . . Literary diversity may be restricted to what is adaptable to the market, yet diversity may increase in absolute terms. And diversity may be the reliable quality that future readers can expect: no longer the *Bleak House* or *Middlemarch* that sums up a culture, but books that express one of many contending perspectives . . .'

But some problems with the hypothesis of increasing diversity come to mind. In the 1890s, numerous British magazines published short stories and 'sketches' (brief descriptive essays or narratives), in addition to serialised novels. Today, far fewer magazines offer space for short fiction, and a writer of tales rather than full-length novels would have great difficulty in finding a publisher. In that respect, there seems to be a reduction in diversity.

Generally, however, Delany makes his case persuasively. The discussions are lucid and well-informed, with a deft balance of the general and the particular. There is a pleasant absence of theoretical jargon. He is capable of taking a hard-headed look at some modern classics. For instance, he points out that Virginia Woolf's celebrated essay, *A Room of One's Own*, which argued (in 1929) that female writers needed private space and an income of £500, 'might also be seen as a way of justifying one's own enjoyment of such a sum, and of claiming a special civility and literary quality for those whose cabins were above the £500 a year watermark'. As evidence, Delany points to her hostility to major modernist works produced by Lawrence and Joyce, writers whose income was much lower than that. Even *Jane Eyre*, Woolf reasoned, would have been less 'deformed and twisted' if Charlotte Bronte 'had possessed say three hundred a year'. Woolf failed to see that her own relative affluence was producing critical deformation.

The most entertaining part of the book concerns the apparent hypocrisies of modernists, who, while generally condemning the cash-nexus and commercialism, were often shrewdly (sometimes cynically) adroit in their financial speculations and opportunism. The income of Leonard and Virginia Woolf eventually proved to be handsome. Leonard criticised imperialism, but invested more than 40 percent of his and Virginia's capital in such imperial ventures as Shell Oil, Federated Selangor or Ceylon Para. T.S. Eliot approved a campaign, headed by Ezra Pound, to raise enough money to buy Eliot out of his job at Lloyd's bank. Delany remarks that 'Pound did not know that Eliot was already endowed . . . with a private income larger than his own'. Eliot had soon gained a generous salary at Lloyd's (a location which, to Pound, would seem 'the Vatican of the great economic swindle') which was augmented by money from shares in the Hydraulic Press-Brick Company of St Louis, not to mention an earlier gift of 'engineering debentures' worth £3000 from Bertrand Russell. The eminent philosopher, being sexually attracted to Eliot's wife, may not have been motivated solely by an enthusiasm for experimental poetry. Pound himself, notoriously, sought patronage from Mussolini and supported the fascist cause of World War II.

Perhaps more could have been said by Delany about the related ironies in the development of modern literature. Conrad, the redoubtable opponent of 'material interests', proud of his noble background and critical of democracy, was sustained in his work by charity and the British taxpayer. In 1902, when Conrad's maid earned £20 per year, Conrad was granted £300 by the Royal Literary Fund; in 1904, the year of *Nostromo*, the 'Royal Bounty Special Service Fund' donated £500; and in 1908 there came a further £200 from the Royal Literary Fund. Furthermore, in 1910 Conrad was awarded a Civil List Pension of £100 per annum (£50 less than W.B. Yeats would be awarded in the following year), and this pension continued until 1917, when the prospering author was in a position to renounce it. Conrad regarded the cinema as 'a silly stunt for silly people', but the Hollywood film industry, by purchasing the rights to film various novels of his, helped him make the rapid transition from debt to affluence. Great authors often need a hand which will not only feed them but which can also be vigorously bitten.

Delany suggests that in Conrad's case, 'imprisonment within economic necessity, with no real prospect of writing himself into the clear, contributed to his brooding consciousness of irrationality and doom'. Exile from a partitioned Poland, and from that country's class of *szlachta* (the gentry-cum noblemen), must also have contributed to that sense. J.B. Pinker, Conrad's literary agent, was remarkably generous in providing Conrad with huge loans. Delany says: 'Unfortunately, Pinker's willingness to fund Conrad only encouraged him to plunge further into debt, and to look on his financial prospects with deeper despair'. This seems rather uncharitable to Pinker, whose advances enabled Conrad to continue to produce brilliant work, even though there was no guarantee that Conrad would ever be sufficiently marketable to repay Pinker. The author, incidentally, outlived his long-suffering agent.

In short, Delany's book is proficient and thought-provoking, but by no means exhaustive. It suggests plenty of routes to be explored by other researchers in the field of literature and economics. As I've indicated elsewhere, economic theories have been sufficiently infiltrated by myths to erode their differentiation from the realm of fiction (Watts, 1990). Story-telling of one kind of another seems inseparable from the human desire to make sense of the world that we inhabit. If literary authors grumble about the economic system of which they are inevitably a part, their grumbles may serve worthwhile ends. Their hopes may sometimes, like William Morris's, be Utopian; nevertheless, as Oscar Wilde remarked, 'A map of the world that does not include Utopia is not worth even glancing at'.

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