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Reviews

A History Of Post Keynesian Economics Since 1936, by John E. King (Edward Elgar, Cheltenham, UK, 2002) pp. 328

Making sense of the tangled history of Post Keynesian economics (PKE) is not an easy task: it is a tale with many characters, changing ideas and issues at different times and levels (methodological, theoretical, policy orientated etc.). John King has attempted the task in a sensible way, and his treatment is skilful.

He starts with a delimitation of PK economists that is purposefully chosen, broad and fuzzy enough to include almost anyone who had any influence on the development of this 'school' (p. 5), and tells the story chronologically and thematically, capping the presentation with an assessment of the present state and possible future of PKE. King's emphasis is on ideas more than on institutions and his focus on macro- rather than microeconomics.[1] Repetitions have been kept to a minimum, while the omissions are compensated by ample references to the literature (this history and King's, 1995 annotated bibliography of Post Keynesian writings naturally complement each other).

The narrative hardly lends itself to a brief summary. Suffice it to say that it begins from the immediate reactions to the *General Theory* from friends and future 'enemies' alike (the critical target of PK economists being, for a long time, the IS-LM representation of Keynes's ideas, as well as the marginal productivity theory of value and distribution), it continues by reporting Kalecki's contribution and its reception by Joan Robinson and other radical dissenting economists, discussing the development of growth theory and the controversy on capital theory. The methodological discussions in the 1970s and the developments relating to the idea of uncertainty are also examined, and the non-Cambridge strains of Post Keynesianism are presented. In the three final chapters King enquires into the affinities, differences and mutual criticisms of the three main lines of PKE (the 'fundamental Keynesians', the Kaleckians and the Sraffians), compares it to other forms of dissent from mainstream economics, and eventually evaluates if and how there has been progress in PKE since 1936. King's account seems to be fair: he does not take sides in an obvious way between these approaches to PKE, with the result that the story unfolds quite smoothly and is very informative on a broad range of issues.

In spite of the clarity of the exposition, the various issues seem to me to have been juxtaposed rather than integrated. This may well reflect the impossibility of identifying a unique common thread that unites individual approaches into a specific line of development of PK ideas. King, in fact, insists on the individual characteristics of the various subschools and on the specificities of national developments, and there are surely many arguments in favour of a reconstruction retaining the diversity and the individuality of the scholars pigeon-holed as PK economists. Yet I have the impression that beneath the 'twists and turns of PK thought' (Sheila Dow's words), there is a theme that is in the background of the whole narrative: the opposition to 'mainstream' economics. This theme does find a place in King's discussion, and is often manifested in his choices of terminology ('Post Keynesians and other heterodox schools of economic thought', 'against the mainstream' or 'Post Keynesians and other deviants'), but the issue of a critical target common to all PK factions is only considered 'in addition' to 'six central messages of Keynes's vision' (Thirlwall)[2], which would be taken by most PK economists as

[1] There is an account of how PKE was gradually marginalised into a 'ghetto' by means of the exclusion from mainstream journals and academic positions (Ch. 12). The issue of the future of PKE reduces to the possible ways of escaping this seclusion – on which King does not seem to be too optimistic: he prospects, in fact, a medium-term fate in terms of a survival as an embattled minority, *Survival*, because there *is* something in the methodology and analytical baggage of PKE; *minority*, because there is no collapse of mainstream economics in sight; and *embattled* because the issues dividing its different strands are far from being resolved.

[2] 'The propositions that output and employment are determined in the product market, not the labour market; involuntary unemployment exists; an increase in savings does not generate an equivalent increase in investment; a money economy is fundamentally different from a barter economy; the Quantity Theory holds only under full employment, with a constant velocity of circulation, while cost-push forces cause inflation well before this point is reached; and capitalist economies are driven by the animal spirits of entrepreneurs, which determine the decision to invest' (pp. 5–6).

a minimum platform. While there is much to say for the strategy of emphasising the constructive part of PK contributions more than the critical attitude, it also seems worthwhile to have a look at the other side of the story, as Keynes himself expounded his positive contributions by means of a criticism of the 'orthodox' view.

When Harrod criticised Keynes for his attacks to the classical schools, Keynes felt he had to intensify his attack rather than abate it:

> My motive is, of course, not in order to get read. But it may be needed in order to get understood. I am frightfully afraid of the tendency, of which I see some signs in you, to appear to accept my constructive part and to find some accommodation between this and deeply cherished views which would in fact only be possible if my constructive part has been partially misunderstood. . . . If I had left out all the parts you object to about the classical school, you would have simply told me that you were largely in sympathy and liked it. But my attack on the classical school has brought to a head the fact that I have only half shifted you away from it. Your preoccupation with the old beliefs – and much more so in the case of most other people – would prevent you from seeing the half of what I am saying unless I moved to the attack. (Letter to Harrod, 27 August 1935, in Keynes, 1973, p. 548)

Some of Keynes's most important followers also proceeded in this way. Thus, it has been said of Joan Robinson that she was one of those scholars who express themselves at their best by targeting someone else, and whose thought better finds its final form in a process of differentiation from the ideas of other people, to the extent of easily degenerating into polemics (Becattini, 1977, pp. 8–9).

Keynes, his pupils, and surely also later Post Keynesians, were fully aware that the establishment of the new ideas could only pass through the elimination of the old ones. But an issue immediately arises. If heterodoxy is characterised as the negation of some orthodoxy, the first task is to define what is orthodox. But this is a distinctly embarrassing assignment. A self-appointed defender of orthodox economic theory wrote, in the same year in which the *General Theory* was published, that he 'found it increasingly difficult to define exactly what it is that I am supposed to be defending', and eventually resorted in turn to a negative definition: having identified three types of economic thinking of widespread importance in the United States – orthodox, institutionalist and Marxian economics – he chose to pigeon-hole as orthodox 'that economics which is neither institutionalist nor Marxian' (Estey, 1936, p. 791). How to avoid running in circles?

Unable to find a single text or author incarnating all the features of orthodoxy, Keynes had to *construct* the notion of 'classical theory', 'which seem to include everyone from Say to Pigou' (complained Robertson: letter to Harrod, 5 October 1935). In a sense, this was a fictitious target for Keynes's attacks. On the other hand, it was not made up out of thin air, but in some way distilled what Keynes thought to be the 'essence' of orthodox theory – whether explicitly stated or resulting from tacit assumptions necessary to make its approach logically consequent. If taken literally, Keynes's construction is highly artificial, and no historian of thought would recognise his portraits as accurate interpretations of Marshall or Pigou (in this sense Robertson's complaint that 'Pigou was misrepresented' and was himself 'regarded . . . as a victim of premature arterio-sclerosis' were justified: letter to Harrod, 28 October 1936). They are, however, far from being useless, as they are essential for understanding Keynes himself, as orthodoxy was implicitly defined in terms of its differences from Keynes's thought and merely illustrated with examples taken from the Cambridge tradition. Keynes's specific criticisms to the 'classics' seem to have been taken up severally, at different stages, by PK economists: King reports in particular of the focus on money and uncertainty, which were first discussed emphasising the superiority upon neoclassical economics in terms of realism or relevance, and were eventually understood as part of Keynes's unorthodox methodology. This approach emerged in the late 1970s, following the research on the newly published correspondence included in Keynes's *Collected Writings* and on other unpublished materials, and led to the feeling that the unifying element of PKE lies in the method (Chs 8 and 9).

The importance of Keynes's methodology, and its distance from the mainstream approach, can hardly be denied. It should also be stressed, however, that this is accompanied by a precise view of the nature of capitalism. In 1934 Keynes pinned down the contrast between orthodox and heretic economists to a single factor: 'On the one side are those who believe that the existing economic system is, in the long-run, a self-adjusting system, though with creaks and groans and jerks, and interrupted by time lags, outside interference and mistakes. . . . On the other side of the gulf are those who reject the idea that the existing economic system is, in any significant sense, self-adjusting. They believe that the failure of effective demand to reach the full potentialities of supply, in spite of human psychological demand being immensely far from satisfied for the vast majority of individuals, is due to much more fundamental causes' (Keynes, 'Poverty in Plenty: Is the Economic System Self-Adjusting', November 1934, in Keynes, 1973, pp. 486–7).

Here Keynes provides a notion of orthodoxy that is not self-referential, one lying deep in the beliefs of economists. Having so identified the line of division, Keynes – who, needless to say, sided with the heretics – identified his fellow travellers: 'The heretics of today are the descendants of a long line of heretics who, overwhelmed but never extinguished, have survived as isolated groups of cranks' (Keynes, 1973, pp. 485–92). The 'cranks', in so far as not belonging to the academic world, have not been able to perceive the strength of the citadel.[3] In contrast, having been brought up within the citadel, Keynes claimed to be in the position of recognising the groundwork of the orthodox argument, and of identifying the assumptions necessary to achieve the desired result. Accordingly, his reflections on money, uncertainty and organicism can (and, perhaps, should) be read in connection with the opposition to the classical approach to the dual notions of equilibrium and crises: as to uncertainty, the whole of chapter 12 of the *General Theory* is written with reference to instability and fluctuations; as to money, Keynes is adamant that 'the conditions required for the "neutrality" of money, . . . are, I suspect, precisely the same as those which will insure that crises *do not occur*' ('A Monetary Theory of Production', 1933, in Keynes, 1973, pp. 410–11); and the 'generality' of the *General Theory* is meant to overcome the limits of the 'postulates of the classical theory [which] are applicable to a special case only and not to the general case, the situation which it assumes being a limiting point of the possible positions of equilibrium' (Keynes, 1936; p. 3).

This induces me to suggest that the issue of the unity, or lack of unity, within the various strands of PKE may benefit from examination in terms of the continuance and development of Keynes's reflections on equilibrium and its stability.[4] I offer this as a provocative suggestion, and am well aware that a number of rational reconstructions have already been offered along different lines (as summarised by King, especially in Ch. 10). Such a characterisation cannot cover the entire range of issues discussed by PKE and includes other unorthodox schools of thought, and cannot therefore be taken as the sole basis for a precise definition of PKE. Yet the recognition that 'the people who come in under this umbrella are a heterogeneous lot, sometimes only combined by a dislike of orthodox or neoclassical economics' (Harcourt, cited on p. 203) provides a not purely 'negative' common feature, but would also have a constructive counterpart; not, of course, at the analytical or methodological levels, where differences characterise the different schools of heretics, but at the deepest level of the *belief* regarding capitalism.

King's book offers abundant material for reflection along these lines as, not surprisingly, the theme of equilibrium frequently comes to the surface. The lion's share obviously goes to Joan Robinson's rejection of equilibrium as a meaningful notion for the analysis of capitalist processes (the evolution of her views is a story within the story). There is also a discussion of Kaldor's position on the same issue, of the debates surrounding the long-run equilibrium interpretation of Harrod and growth theory, of Davidson's conclusion that a monetary theory invalidates Say's law; the issue also covers the 'orthodox' field, with the debates on monetarism and the equilibrium interpretation of the IS-LM representation.

As to instability, King's presentation of Minsky's position is especially interesting in the perspective suggested here. In a letter to Sydney Weintraub, Minsky argued that 'within a cyclical perspective uncertainty becomes operational in the sense that myopic hindsight determines the current state of Keynesian/Robinsonian Animal Spirits: without a cyclical perspective uncertainty is more or less an

[3] Robertson pointed out that by stopping at Mummery and Gesell Keynes gave 'the impression that apart from a handful of dead cranks he was the first person to question the alleged "classical" hypothesis of an automatically and instantaneously self-righting economy. He ought to have gone on to say something serious and appreciative of the work of his contemporaries, – the Swedes, Haberler, myself…K[eynes] found it easier to be generous to cranks than to professional economists, but I think it is not unfair to say that he preferred even his cranks to be dead.' (Letter to Harrod, 4 April 1950)

[4] And with other dissenters, including those related to Schumpeter, strangely neglected by King. Most of the topics discussed by PKE and reported by King overlap, not accidentally, with the features of heterodoxy in Marx, Keynes and Schumpeter: see the summary by Deleplace and Maurisson (1995).

empty bag' (cited on p. 113). This brings uncertainty back to the original context of chapter 12 of the *General Theory*, where it is intrinsically linked to the fundamental issue of the instability of the system. Would this be true of other themes recurrent in PKE as well?

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Financial Econometrics: Problems, Models and Methods, by Christian Gourieroux and Joann Jasiak (Princeton University Press, Princeton, 2001), pp. xi + 513

In recent years there has been a substantial increase in interest in financial econometric techniques in both academia and industry. This is driven by the proliferation of financial time series data, within which empirical evidence suggests that there are persistent and evolving signals. Modelling these signals has proven to be complex, and the flexible econometric models developed are among the most difficult on which to perform statistical inference. *Financial Econometrics: Problems, Models and Methods* thus proves a very timely publication. It provides a clear and concise summary that should prove accessible to postgraduate students of finance, econometrics, applied statistics and mathematical economics. In particular, the choice, ordering and scope of the material are excellent.

The authors are two prominent researchers, and they bring an informed discussion of many of the topics of current interest. As suggested by the subtitle, the approach the authors take is a multidisciplinary one, although with a primary focus on the econometric modelling. Each chapter is clearly structured, first highlighting an underlying financial problem, then containing a discussion of the econometric models available to address the problem, followed by a guide on how to undertake statistical inference. Throughout each chapter the main points are demonstrated by application to several financial datasets. At the end of each chapter there is a short summary of the major conclusions to be drawn and a discussion that places the chapter in context of other chapters within the book.

Following the introductory chapter, Chapters 2 to 4 provide a summary of traditional econometric time series analysis in a univariate and multivariate setting. This is motivated by the study of financial returns and, in the multivariate setting, by the joint modelling of financial returns and volumes. The embedding of such time series models into economic asset pricing and optimal portfolio allocation models is also discussed. Chapter 5 contains a short discussion of cointegration and long memory properties of asset returns. The early chapters make a clear argument for the existence of signals in the conditional means and variances of asset returns. This is explored mainly for the univariate case in Chapter 6. Here, GARCH modelling is covered, along with a short comparison with stochastic volatility models. Also discussed is flexible kernel regression modelling of conditional means and variances, as opposed to parametric models. Overall, these early chapters provide a summary of much of traditional and modern time series analysis for a single financial asset.

The next two chapters are motivated by developments in financial economics and stand out because the rest of the book is primarily concerned with econometric time series modelling. For example, chapter 7 compares forecasting using exponential smoothing with the rational expectations and present value models popular in financial economics. Chapter 8 highlights the increased complexity in selecting optimal portfolios when employing an intertemporal utility function, and includes the derivation of a consumption-based capital asset pricing model.

Chapter 9 continues the summary of financial time series where 6 left off, namely with the use of parsimonious factor models to study the dynamics of high dimensional multiasset return vectors. Models with factors that are static or dynamic and observed or unobserved are all covered, and the exposition is nicely integrated with the univariate stochastic volatility and GARCH models discussed in earlier chapters. Here, the authors discuss the state space formulation of such dynamic models and estimation using the Kalman filter and other filtering algorithms. Chapter 10 continues with discrete time series modelling, in particular for directional movements of stocks.

Chapters 11–13 change tack with a discussion of continuous time diffusion modelling. The popular geometric Brownian motion, Ornstein–Uhlenbeck and Cox–Ingersoll–Ross processes are all covered, as well as Eulerian discretisation, binomial tree approximations, jump processes, Girsanov's theorem, Ito's lemma and derivative pricing in complete and incomplete markets. While there are already a number of excellent texts in financial mathematics, I found the authors' choice of material covered in a compact space the most useful aspects of diffusion modelling from the point of view of an applied econometrician. For example, the continuous time diffusion analogues of popular discrete time series models (such as the GARCH-in-mean and stochastic volatility models) are covered in Chapter 11. Chapter 12 discusses estimation of such diffusion models using maximum likelihood or method-of-moments based estimators. Chapter 13 discusses the volatilities implied by the Black and Scholes options pricing framework, and semiparametric modelling of the implied volatility surface.

The last three chapters of the book collect together recent work on market microstructure, analysis of high frequency data, estimation of value at risk and extreme risk management. They make a nice discussion of topics that are very much the subject of current research.

The material in the book is probably beyond all but the most advanced undergraduates, and it is not really a text in the normal sense, omitting a section on class problems. Nevertheless, I would not hesitate to use it for postgraduate teaching purposes. In this case, the material can be split into five main sections: univariate financial time series (Chapters 2 to 6), intertemporal financial economics (Chapters 7 and 8), multiasset and discrete financial time series (Chapters 9 and 10), diffusion modelling (Chapters 11–13) and topics from the frontier of econometric modelling of financial data (Chapters 14–16). While this is far in excess of what can be covered in a single semester, two-thirds of it can be covered over two semesters in an introductory and an advanced follow-on course.

The book provides an excellent companion to *The Econometrics of Financial Markets* by Campbell, Lo and MacKinlay. This earlier book covered much the same material in a similar multidisciplinary fashion, but Gourieroux and Jasiak use to good effect three or four more years of material and – the main difference – Gourieroux and Jasiak focus is slightly less on financial economic theory, but more on econometric modelling. Both books pursue their stated agendas excellently and will do much to encourage study and research into financial econometrics through the current decade.

Financial econometrics is an emerging field that is difficult to define. It represents the cumulative research of a range of scholars into quantitative problems arising from financial markets. This includes research undertaken by mathematicians, economists, econometricians and statisticians, so that 'financial econometrics' is not clearly defined within any single traditional field. One result is that effective research requires a combined understanding of the financial markets, economic theory, econometric modelling and statistical inference. Another result is that developments are found in academic journals across a number of fields, as well as those dedicated to financial studies. Both of these factors make the current state of the literature difficult to assess and bring into a single coherent volume. Nevertheless, it is this task that Gourieroux and Jasiak set themselves.

The one notable omission in *Financial Econometrics* is a lack of focus on the frequent necessity to revert to advanced statistical inference for some of the more complex econometric models. There is little emphasis on inference beyond the point estimation of parameters and semiparametric surfaces, and absolutely no mention of any Bayesian perspective of the econometric models discussed. Also omitted are the now standard computationally intensive approaches of estimation for time series models, such as Markov chain Monte Carlo sampling schemes, or the EM algorithm. However, a single volume on the 'problems, models and methods' of financial econometrics cannot be fully comprehensive. If some omission is to be made, then there is a good argument for making it the more advanced forms of statistical inference.

Some years ago a prominent econometrician said to me 'financial econometrics has saved econometrics from itself'. I do not concur with this statement, but I certainly agree that financial econometrics has helped reinvigorate econometrics as a whole. What is more, the underlying demand for empirical analysis of financial time series is increasing with the changing face of financial markets, so that I can only see financial econometrics playing a larger role within the discipline. For those interested in studying or researching the topic, I recommend this book as an excellent way to begin. For those already with some knowledge of the area, I also recommend it as a comprehensive, focused and up-to-date summary of the field.

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The Elusive Quest for Growth, by William Easterly (MIT Press, Cambridge Mass., 2001) pp. xiii + 342

This is a racily written, highly readable book that reads like a detective story. It is about a dismal subject, which however, is not correctly identified by its title. It is not about economists', but, more specifically the World Bank's elusive search for growth. Interspersed with anecdotes and personal autobiography, Easterly – now a former World Bank official – has, with wit and honesty, shown up the continuing intellectual shallowness of the economic advice offered by the institution he has worked for since the mid 1980s. Although most of his strictures will not be revelatory to those of us who in our misguided youth worked at the World Bank, it is important for the public debate on international financial institutions that this worm's eye view from the inside now be available. I hope it will be widely read.

This being said, it is fair to ask: what is its scholarly content? Does it advance our knowledge of the economics of developing countries? Here I must here confess an interest. For people of my age and persuasion there is little new in what Easterly says. The critique of 'development economics' he provides in the first part of his book, and which during the McNamara–Chenery years was the intellectual basis of the World Bank's advice, does not go beyond that in a little book I wrote in 1983 (Lal 1983; 1997; 2002) which has acquired some notoriety if not fame. This is not even mentioned. Nor is the more substantial book by Ian Little (1982). Similarly, there is no mention of the large World Bank financed comparative study on the *Political economy of poverty, equity and growth* that Hal Myint and I wrote (Lal and Myint 1996), which is directly relevant to Easterly's subject, and which presages most of his policy conclusions, whilst providing a more robust political economy explanation for why growth has been elusive in the land-abundant continents of Latin America and Africa (as compared with the labour-abundant economies of Asia). Following the practice now common among the young, Easterly ignores relevant past work on the subject, except that of the most recent fad. This may be forgivable in a travelogue but incomprehensible in a scholarly work published by a University press.

Apart from these gripes, I have some serious doubts about the methodology underlying the substantive parts of this book, and thence on some of the panaceas Easterly offers. To be sure, no classical liberal economist will find anything surprising about Easterly's central tenet 'people respond to incentives; all the rest is commentary'. Similarly, his castigation of past and current panaceas of international financial institutions – capital fundamentalism based on the Harrod-Domar model (Ch. 2), education (Ch. 4), population control (Ch. 5), adjustment assistance (Ch. 6), debt forgiveness (Ch. 7) – though not new will still be music to their ears because of the author's provenance.

But, serious doubts remain about the cross-section statistical studies on which Easterly by and large relies to slaughter these sacred cows, and to support others that he favours. As Solow (1994) has rightly noted, whatever one thinks of the 'new' growth theory, an international cross-section regression is not a 'a confidence inspiring project. It seems altogether too vulnerable to bias from omitted variables, to reverse causation, and above all to the recurrent suspicion that the experiences of very different national economies are not to be explained as if they represented different "points" on some well-defined surface' (p. 51). Particularly if the 'points' of observations are such disparately sized countries with an equal weight in the sample as Chad, Malawi, India and China, and when there are serious and fundamental questions about the quality of the underlying national accounts data (see Srinivasan 1994). Nor, as Levine and Renelet (1992), showed are the regressions robust, and their main message 'is not that nothing matters, but that policy matters. [But] the data cannot really tell which policy is bad' (Sala-Martin 1994, pp. 742–3).

This is particularly worrying for Easterly as his strong conclusions about particular policies are based largely on these cross-section regressions. Easterly is clearly in thrall to the burgeoning cross-section regression studies using the Kravis–Heston–Summers data set. Most of the empirical studies he cites (many of which are his own and with numerous collaborators) in support of his claims are of this statistical variety. But I, with many others, have grave doubts about the relevance of the theory, and the robustness of these cross-sectional studies. Given this fragility, there is no escape from the longitudinal historical study of a large number of countries. That is, of course, what the Lal-Myint study attempted. (Also see Harberger 1984).

The most worrying part of Easterly's book, however, is his wholesale swallowing of the 'new' endogenous growth theories. In his chapters on increasing returns and creative destruction he illustrates the abiding fault of many 'development economists', and particularly those at the World Bank, who are impressed by the latest theoretical models, and assume that these have practical relevance, with dire consequences when they are applied (see Lal 1983). Thus, by swallowing the mathematical models with increasing returns that generate all sorts of traps, he reinvents another version of the famous Rosenstein–Rodan model based on the Pareto-irrelevant pecuniary externalities. But all these theories of vicious and virtuous traps are old hat and moreover have been empirically exploded (see Little 1982 and Bauer 1976). While, Rodan wanted coordination of investment in a planned 'big push', Easterly's big idea for alleviating poverty runs as follows: 'If everyone was able to agree in advance that they would make investments until they reached a skill level above the poverty trap threshold, then they would get out of the poverty trap. Unfortunately, the market does not make this coordination on its own, and so poverty persists' (p. 169). There then follows a whole list of dirigiste panaceas, completely out of keeping with the tenor of the rest of the book.

The trouble with the 'new' growth theory is that, it has not rethought the foundations of the theory – in particular the concept of the 'aggregate' production function. It is not much more, as Solow has rightly noted, than the standard neo-classical theory 'with bells and whistles', with the fashionable AK model just being another version of the Harrod–Domar model. A much more enlightening but sadly neglected 'endogenous' growth model that attempts to rebuild the production function foundations is by Scott (1989). This is also applicable to actual countries, as Scott shows for OECD countries and Lal-Myint do for their sample of 25 developing countries. Despite Easterly's title, economists (but presumably not those at the World Bank) do now know as a result of 50 years of experience how growth is generated. The answer is banal. It depends upon the rate of investment and its efficiency, where the latter is crucially dependent on policy (in particular trade policy). These policies in turn are the classical liberal package, and what needs to be explained, if growth in so much of the world remains elusive, is why this well-known package is not universally adopted. For that one needs to know the economic history and political economy of these different countries. Cross-section regressions will not provide the answer. The World Bank, for the 'rent-seeking' reasons Easterly notes, has resisted a whole hearted endorsement of this package. The 'new' growth theory seemingly provides the Bank another play at global dirigisme. While, its current stance, endorsed by Easterly, is for its 'aid' to be given only to the successful countries that have good policies. But, with burgeoning privatised capital markets open to the successful, why would they need the World Bank's 'aid'?

Easterly, thus displays the very quality that has led to the elusive search for growth at the World Bank and by so many 'development economists' – an unhealthy and unworldly respect for the latest current academic fashion. Thus, while I would commend this highly readable book – whose conclusions are by and large sound – to the general reader, I have serious doubts about its scholarly acumen.

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Making Modern Economics, by Mark Skousen (ME Sharpe, Armonk, N.Y., 2001), pp. 495

The 2001 Higgins Memorial Lecture was delivered by Mark Skousen. It was quite a performance. Skousen danced along with his wife to tunes representing the great economists, recounted stories from his CIA days, and told how he had taken up the task of writing a book to counter the influence of Heilbroner's *Worldly Philosophers*, after being frustrated by Murray Rothbard's failure to deliver his own counter-blast, and with Rothbard's low view of Adam Smith. (Rothbard's incomplete two-volume work has been recently published by Edward Elgar.) After all, Skousen's Mormon relatives felt Adam Smith was a Godly man because he was a friend of capitalism (American style, of course). Skousen's really serious argument for capitalism, though, was a graph with an upward sloping line – no labels of axes in sight – which was briefly whisked across the overhead projector by Skousen's wife.

At least I knew reviewing the book would not be dull reading! Skousen avoids recent arguments among historians of economics over Whig approaches to history versus contextual approaches in favour of his own totem pole approach. This is best explained by Fig. (B) on page 8, which shows a totem pole with Adam Smith serenely looking out across the landscape from top, and rather sour looking Keynes and Marx down the bottom. Skousen adds 'today's histories of economics lack a running thread of truth, a consistent point of view which allows the student to realise when an academic scribbler is heading off the straight and narrow path (p. 6)'. We are never left wondering where Skousen stands. Smith is selected as the book's hero and reference point because he 'advocated maximum economic freedom, in the microeconomic behaviour of individuals and the firm, and minimal macroeconomic intervention by the state (p. 7). Although Skousen's agenda is very upfront the remainder of the book is by no means predictable. The Austrians and the Chicago school emerge later as subsidiary heroes, and the twentieth century development of American economics receives more attention than most other histories of economic thought. Just at the point when one starts to feel it is getting a bit predictable Skousen's boxes and biographical snippets enliven the book. For instance 'Famous Economists' Signatures: Can You Tell Which One is the Pessimist?', 'Phrenology' (i.e. examining skulls to determine character), 'Why Did Marx Grow Such a Long Beard?', 'The New Palgrave: A Marxist/Sraffian Plot?' etc. There is lots of gossip about economists' sex lives and personal finances, lots of photos (including Skousen at the tomb of his hero Adam Smith). And never a dull moment.

The book is written with students in mind. But should it be recommended for a course in the history of economic thought? I'm not sure. The standards of evidence and argument are not for student historians of thought to emulate, but it does a great job of raising issues and generating interest in the big questions of economics. It would be a better book to give beginning students of economics, and I'm sure would do more for enrolments in upper level economics programs than the current first year textbooks used in most Australian universities. There is actually a lot of economic theory in a history of thought book like this one, and students taking a first year course based on the book would not go away ill equipped for higher level studies. And students dissatisfied with Skousen would then take upper level units in the history of economic thought to do this properly.

Overall, a flawed but enthralling and almost unputdownable book. Bring on the reasonably priced paperback edition.

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Economic Theory and International Trade, by Alan D. Woodland (ed.) (Edward Elgar, Cheltenham, 2002), pp. xvii + 329

This volume collects 20 papers on economic theory as applied to trading economies. The papers were collected to mark the occasion of Murray Kemp's 75th anniversary. All of the authors are former colleagues, students or coauthors of Murray Kemp.

The papers are organised into four parts: Classical Trade Theory, International Trade Policy, Market Structure and Economic Dynamics.

This volume can be viewed in two ways. First, and unusually, as a collection of papers by authors associated with Murray Kemp that tells us a great deal about the output of one of Australia's greatest economic theorists and his considerable influence on economic theory. Second, as a collection of papers on the theory of international trade and trading economies that informs us about developments in trade theory.

From the first viewpoint, the volume demonstrates the truly remarkable lifetime output of the subject of the book. One dimension of his output is that Murray has worked with a very long list of coauthors. Indeed, I do not know of any economist who has a longer list, and they are almost all well-known trade theorists or general theorists. Many of them are from Japan, Korea, China or Vietnam, which is a huge personal contribution to Australian cooperation with East Asia. Another dimension is that the scope of the areas of economic theory in which he (and his coauthors) has worked is extremely wide; in addition to virtually every area of the theory of international trade (including trade in capital, migration of workers, foreign aid and foreign exchange markets) and international trade policy, it includes basic contributions to such areas as economic dynamics (the Correspondence Principle), social choice, general equilibrium, environmental economics, economic growth, history of economic thought and even a smattering of articles on macroeconomic theory. This output makes him one of the most important trade theorists of the twentieth century. I have eight of his books on my shelves, a number exceeded by only one other economist.

This productivity has not ceased since retirement. The Introduction contains a list of publications in the 10 years after retirement at age 65. They come to 42 articles or chapters of books edited by others, plus one new book and another book of essays that is mainly reprints, and three new papers. (There is no double counting in this numbering.) What an example of post-retirement productivity! As one who is due to retire within a few weeks, let no one ask me to emulate this feat.

From the second viewpoint, the collection of papers is an impressive one. A feature of the collection is the high technical quality of the contributions. Together they cover almost all of the areas of contemporary research in international economics.

Naturally in such a diverse collection, each reader will be more interested in some rather than others. As an indication of the richness, let me comment briefly on a few that are of particular interest to me. The paper by Yew-Kwang Ng deals with the effects on national welfare of a growing population. It comes up with the provocative finding that a subsidy on population growth may be Pareto-optimal and benefit the existing population, even in the absence of external benefits. The paper by Michihiro Ohyama applies the theory of international trade policy to the rules of the GATT/WTO. One result of particular value and interest is the extension of the famous Kemp–Wan Theorem – which is probably the most quoted of all Murray Kemp's papers – from the case of a customs union to the more common case of a free trade area. The paper by Albert Schweinberger is a generalisation of previous research by Hamada and others on the economics of special economic zones. Such zones are examples of piecemeal reform and the earlier research showed that foreign investment attracted to these zones is immiserising. The author produces a quite general condition for welfare improvement. The paper by Henry Wan is a simple and neat model of trade and growth, based on learning by doing based on national output. In this world, trade sometimes but not always benefits growth.

Other papers will equally appeal to other readers. International trade theorists should consult the rich offerings in this volume to follow current research at the frontier of international trade theory.

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Reflections on the Great Depression, by Randall E. Parker (ed.) (Edward Elgar, Cheltenham, 2002) pp. xii + 230

Understanding the Great Depression remains the 'Holy Grail of macroeconomics'.[1] Randall Parker, who together with James Fackler (1994) has himself ventured after the Grail, has had the good idea of interviewing a number of distinguished economists belonging to the generation that experienced the Depression and got their training before the War. They make an impressive list of the idols of my generation: Paul Samuelson, Milton Friedman, Moses Abramovitz, Albert G. Hart, Charles

[1] The felicitous phrase is Ben Bernanke's (1995) and quoted by Parker.

Kindleberger, Anna Schwartz, James Tobin, Wassily Leontief, Morris Adelman, Herbert Stein and Victor Zarnowitz.

Parker furnishes a useful introductory chronicle of events and a survey of research on the Great Depression from Friedman and Schwartz, through to Bernanke, Cecchetti, Eichengreen, Hamilton and Romer. This is grouped under the three headings: 'the Monetary Hypothesis', 'the Nonmonetary–Financial Hypothesis', and 'the Gold Standard Hypothesis'.

What was the cause of the Great Depression? N.R. Hanson (1969, p. 275) once said that in trying to find their way through the 'logical labyrinth' of 'cause' '. . . philosophers and scientists often go in circles, or get completely lost, or come out the way they went in.' In the spirit of the kind of illustrations that Hanson liked, consider the case of a man slipping on a banana peel and breaking his leg. A physicist would explain that the speed with which he was walking, his weight and the low friction coefficient of the banana peel did it. A doctor might find osteoporosis in his bones. A psychologist might argue that a domestic disagreement earlier in the morning caused him not to pay attention where he was stepping. Someone else might point out that if he had taken his usual way to work he would not have encountered the banana peel. The police might look for whoever was guilty of throwing the peel on the sidewalk . . . and so on. In the absence of any of these, the accident would not have happened. 'Clearly, what will count as a cause or an effect is largely a function of what we are looking for, what our problem is, what we consider noteworthy, where our interests lie, the way we express our questions' (Hanson 1969, p. 283).

Kindleberger, the economic historian, and Zarnowitz, the National Bureau inductivist, are the most sceptical of monocausal explanations.[2] But also Paul Samuelson, regretting that it makes for 'lousy theory' but respecting the need to 'describe the facts correctly', thinks of the Great Depression 'as a concatenation of a multiplicity of factors'. At the opposite end of the spectrum one would find economists who think in terms of impulse-propagation models and believe that the Holy Grail can actually be grasped. None of Parker's interviewees go quite that far, although Friedman and Schwartz come close, being reluctant both to give much weight to finance beyond money and to concede that defence of the gold standard offers much in the way of excuses for the Fed's conduct of monetary policy. Even when they argue very strongly for a particular interpretation of events, almost all of the respondents bring out episodes or events that enrich the reader's appreciation of the complexities of the historical process.

Since this book was written, a new front has opened up in the battle among contending interpretations of the Depression. Cole and Ohanian (2001) find that, in the context of a calibrated intertemporal general equilibrium model, money and banking shocks can account only 'for a small fraction of the Great Depression.' Their conclusions would thus dispose in one fell swoop of the Monetary, the Nonmonetary–Financial, and the Gold Standard hypotheses in all their various variants. It is a fair guess that the people speaking in this volume would attribute the negative findings to the reliance on the general equilibrium framework and probably see it as a return, in modernised form, to the kind of economics that they remember from their youth as so clueless in the midst of the Great Depression. Samuelson and Tobin, the still unabashed Keynesians, would surely exclaim 'Say's Law rides again!'

Certain themes naturally recur. 'Beware of a bubble economy', warns Abramovitz, when asked about the lessons of the Great Depression. Most of the others agree. Yet, no-one is quite sure what to do about them when they occur. Several explicitly shy away from adding asset prices to the intermediate targets of Fed policy.

Although those on the right and those on the left obviously hold different opinions about the permanently enlarged role of government that they date to the 1930s, there is virtual unanimity on the New Deal itself. The bank holiday, the deposit insurance legislation, the Works Progress Administration and Civilian Conservation Corps they all view positively, but Abramovitz speaks for the entire group when he testifies that '. . . we were all strong economists enough not to like the NRA and for the same reason, we didn't much care for the Agricultural Adjustment Administration either.'

Distribution was and has remained an issue of more importance to the generation that speaks here than to the current generation of economists. Herb Stein, for example, whose views on the causes of the depression are basically those of Friedman and Schwartz, names Henry Simons's *Economic Policy for a Free Society* as having had a lasting influence on him and quotes Simons from memory: '. . .

[2] Kindleberger, however, having espoused 'Kindleberger's Law of Multiple Causation', turns right around and proclaims himself 'a debt-deflation man'.

extreme inequality in the distribution of income, as of power, is unlovely.'

From some of these interviews the reader who has not made a specialist study of the Great Depression has much to learn. The one with Anna Schwartz may be the meatiest of them all. In contrast, Leontief (at age 91) offers no reflections on the Depression beyond noting that fluctuations are non-linear processes (and not fit for input–output analysis). Several provide the increasingly unusual combination of good economics and good reading. In this regard, I would particularly single out Kindleberger and Adelman for honourable mention.

Parker obviously conducted these interviews in large part for his own edification and enjoyment. One cannot begrudge him that. But his conversational engagement makes him less than the ideal interviewer. With A.G. Hart he had such a good time that he basically failed to draw out Hart's views on 'debts and recovery'. A more experienced interviewer would have been more self-effacing. Parker, a free marketeer,[3] occasionally lets his own values and opinions intrude in a manner that may have prevented him from eliciting more nuanced views from his subject.

In the brief span of time since these interviews were conducted, several of these great old economists have passed away. One is grateful to Parker for his initiative in undertaking this project.

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[3] Many government programs, he interjects, are 'hocus pocus' (p. 124) and the New Deal 'opened the door to socialism' (p. 119).

50 Years a Keynesian and Other Essays, by Geoff C. Harcourt (Palgrave, Houndmills, UK, 2001), pp. xxi + 364

50 years a Keynesian is the latest collected volume of essays from the prolific pen of Australia's Geoff Harcourt. Dating from the 1990s (most from the latter half of the decade), the essays are testament to an economist whose analytical powers are complemented by the singular experience of five decades of mixing it with the leading figures of our discipline. Harcourt's well-known ability to synthesise and distil complex ideas means these essays are both highly informative and very useful pedagogically. But, as one has come to expect from Harcourt, they are also delightfully entertaining reading.

The essays in *50 years a Keynesian* are divided into six parts, covering: the ongoing relevance of Keynes; intellectual biographies; tributes to near contemporaries; review articles; a survey of Post-Keynesian thought; and general essays. Some of the most penetrating biographical sketches emerge in the survey and articles, and some of the most illuminating theoretical discussions in the portraits of the protagonists.

As the title of this volume makes clear, Harcourt approaches economics from a Keynesian perspective. Keynes is, he writes (p. 341), one of his 'great heroes' whose life provided a 'resounding *yes*' to the question posed by Cambridge sage G.E. Moore as to whether it was possible to both *do* and *be* good. But Harcourt is by no means an uncritical observer of the path taken by Keynesian economics. Keynesians in the 1960s (especially in North America) were, he says (p. 43), 'grossly overconfident'. Their presentation of the Phillips Curve as a kind of á la carte menu that enabled society to choose varying levels of inflation and unemployment was, he believes, especially unfortunate and ultimately opened the door to doctrines hostile to Keynes's vision. Harcourt remains convinced, nevertheless, of the ongoing relevance of Keynesian economics and there is a sense, through many of the essays, that he believes Keynes's time in the sun is once more at hand. This is especially so in the light of what Harcourt sees as four major changes that have been made to the foundations laid by Keynes by scholars inspired by his vision. These, he suggests, are:

1 The establishment of microeconomic foundations that incorporate imperfect competition. Work in this area has united what Harcourt (p. 68) refers to as the 'two Cambridge revolutions' – Keynes's macroeconomic one, and the 'imperfect competition revolution' that was the product of fellow Cambridge luminaries such as Gerald Shove, Richard Kahn and, especially, Joan Robinson.

2 The assumption of an endogenous rather than an exogenous money supply. Harcourt notes (p. 48) that 'Keynes was essentially an endogenous money person for all of his life', but for expositional and tactical reasons had maintained an exogenous money supply in *The General Theory.*

3 The abandonment of the assumption of constant long-term prices with the more realistic expectation of a rising sectoral trend.

4 The greater emphasis on open economy dynamics. Keynes wrote more on global macroeconomics than had any economist before him but, as per exogenous money, for expositional and other purposes *The General Theory* assumed a closed economy.

Harcourt is generous to all the contributors to this dynamic Keynesianism, but he has himself played a major role, especially with regard to microfoundations. Of course, by 'microfoundations' Harcourt does not mean what he refers to (p. 119) as 'the worst of modern heresies', to wit, the extrapolation of the 'representative agent' to the analysis of systemic behaviour. True microfoundations, according to Harcourt (p. 265), reinstate the firm rather than the consumer as the basic unit of analysis, allowing the inclusion of institutional details and 'a view of economic processes as evolving, progressing organic systems, a dynamic view of the nature of our discipline as opposed to the more static allocative one . . .'

Harcourt is also impressed by Keynesian methodology, by which he means approaching economics via real world observations rather than behavioural axioms. Such a methodology was true of the Cambridge tradition generally he argues, 'a "horses for courses" approach, [and] itself an all-embracing structure at the methodological level' (pp. 263–4). This last attribute has the added bonus of being a unifying principle for the all-too-often fractious Keynesian economics family.

Harcourt is a great exponent of the intellectual biography. They allow us, he says (p. 330):

> . . . to begin to see the links between the historical settings of the persons concerned, their class, their racial, educational, philosophical and religious backgrounds, and the issues of the day on which they have worked. By analysing the intertwining of all these aspects, we get a better understanding of the writings and contributions of these economists, of their limitations as well as their achievements, of the particular forms which their analyses take, and, possibly and hopefully, we are also inspired to follow on from where they left off.

50 years a Keynesian contains biographical essays on Joan and Austin Robinson, Lorie Tarshis, Karl Marx and Keynes, but there are also 'tribute' essays to George Shackle, Josef Steindl, Bill Phillips, Piero Sraffa and Hyman Minsky.

It would not be unfair to say that it is the portrait of Joan Robinson that dominates. The principal essay included here is Harcourt's acclaimed 'obituary' that appeared in *The Economic Journal* in 1995. That this appearance was so belated is but one of the injustices Harcourt feels has been meted out by our profession to Robinson, who is on any measure surely its most accomplished female practitioner. Elsewhere (p. 102) he writes approvingly of the recent emphasis on path dependency, but chides that 'rarely is Joan Robinson . . . ever given credit for identifying the issues and setting out the conceptual framework for the subsequent analysis'. He also repeats (p. 210) Robinson's own oft-voiced exasperation with her interlocutors that she wished they 'would stop paying her compliments and answer her questions instead'. A separate essay examines the contribution of both Joan and Austin Robinson to development economics – a field in which (rarely) it was Austin who probably had the better of the argument.

The essay on Marx (written with Prue Kerr) might seem at first glance the 'odd man out' in this collection. In fact, it's a gem, and a marvellous introduction to Marx's life and principal ideas (including, as Harcourt drolly notes, that 'dreaded phrase' – the Labour Theory of Value) that will lighten the heart of anyone attempting to acquaint the modern undergraduate with this now unfashionable topic.

In paying tribute to Joan and Austin Robinson, Harcourt notes (p. 320) their 'ability to structure and communicate in a clear and intelligible way a usable system of thought' – an ability, he says, that 'characterises the greatest members of our trade'. Harcourt's own virtuosity in this context is on display throughout the essays collected in *50 years a Keynesian.* It warrants a place on every economist's bookshelf.

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The Macroeconomics of East Asian Growth, by Yanrui Wu (Edward Elgar, Cheltenham, UK, 2002) pp. 241

The book contains a collection of six empirical works relating to economies in East Asia, and a review article on the causes and impact of the Asian Financial Crisis. It is a creditable assembly of academic exercises, some of which are based on students' theses ably supervised by the author of this book. The chapters in the book are well organised, starting with topics that cover groups of countries and then proceeding to specialised issues associated with individual countries.

Using the frontier production approach, Chapter 2 investigates the impact of openness on economic growth in 16 APEC economies. This approach has enabled the author to compute changes in total factor productivity (TFP) and the decomposition of TFP into technological progress, scale economies, and technical efficiency. The author would have done readers who are less familiar with the literature a favour by showing how the TFP can be disaggregated into components and elaborating on the interpretation of these components. The results indicate that many of the economies studied have negative technical *efficiency* in the 1990s. While acknowledging this, readers will be puzzled to note that all countries have positive technological *progress* in the same period.

Novelty has been exercised to use principal component analysis to derive a measure of 'openness' of an economy based on three ratio variables: export to GDP, import to GDP and FDI to GDP. The weights used in constructing the component are practically the same, implying a simple average of the three ratios will suffice. As expected, 'openness' does contribute to growth via its influence on productivity. The estimated TFP growth in the Asian NIEs becomes more significant, and rather unlike that reported by Young (1992) and Kim and Lau (1994). The results are also congruent with those of Gapinski (1997) where allocative efficiency arising from trade is taken into account.

Chapter 3 considers the impact of stock markets on economic growth in Asian countries. It is expected that such institutions will boost economic growth of an economy by diversifying risk, facilitating investment, improving resource allocation, bringing changes in incentives for corporate control and sourcing of funds for new business ventures. Indeed, panel regression results involving 14 economies seem to support the hypothesis that large and liquid stock markets will aid growth. However, as indicated by the author, the results are sensitive to the sample considered. Furthermore, it is likely that the direction of causation is not one way. Economies with vibrant growth will have positive spillover effects in the development and performance of the stock markets. Another point regarding stock markets in Asia is that they are often very sensitive to the variability of the leading stock markets in the Western developed countries. If Asian stock markets' movement and performance are simply a reflection of the stock markets of the West, it will be not surprising that correlation between stock market performance indicators and the growth of the associated Asian economies will be weak.

Intra-industry trade (IIT) in ASEAN is the subject of Chapter 4. Together with the concerted effort of implementing the ASEAN Free Trade Agreement and the substantial flow of FDI into the region over the past two decades, IIT has grown rapidly. The author has diligently incorporated other measures of IIT in addition to those pioneering measures constructed by Grubel and Lloyd. In particular, the separation of IIT into vertical IIT and horizontal IIT is illuminating. Such inclusion will allow for the consideration of economies of scale and imperfect competition in the IIT analysis. It is likely to surprise many readers that the author has missed an earlier work of Menon (1996) which would provide useful comparisons.

In Chapter 5, the primary focus is on the role of productivity growth in the Taiwanese economy. It begins by providing a survey of the problems and complexities involved in estimating TFP. The sensitivity of TFP growth computation to factor share has been demonstrated by Toh and Low 1996). A similar observation is made by Sarel (1996) and his meticulous procedure in obtaining an accurate estimate of the capital share for the economy has captivated the author's attention. The contribution of this chapter lies not so much in obtaining precise capital share figures but on the determinants of TFP growth. Using regression analysis, the author is able to show the statistical significance of trade, government expenditure and FDI as causal factors for TFP growth.

The rapid growth performance of the Singapore economy is the topic of investigation for the penultimate Chapter 6. Three alternative engines of growth are hypothesised: trade, public policy and investment. Thus three separate equations were specified to explain the GDP performance of Singapore. In the first model, terms of trade, exports of manufactures and number of tourist arrivals are used as explanatory variables. In the second equation, government recurrent expenditure, compulsory saving, and consumer price index are used. The final equation uses government expenditure on education, government development expenditure component, and total private investment. The result indicates that 'the trade model' is favoured over 'the state' and 'investment' models. But it is hard to attribute growth to a single source. All three factors, trade, investment and proactive macroeconomic polices act in concert to deliver the sterling growth performance. Trade, and in particular exports, will not flourish if not for the active measures made in attracting foreign multinational companies to invest in Singapore: good infrastructure, disciplined workforce and appropriate fiscal incentives. It may be better to classify the engines of growth simply into domestic drivers and external drivers. An exercise of that nature is reported in Tan *et al*. (2002)

The final chapter gives a good review of the causes and consequence of the Asian Financial Crisis. Of great interest is how these crisis-stricken economies cope and recover from the devastation wrought by the Crisis. The year 1997 marks the return of Hong Kong to China; it is also a year that signals economic potency and threat of the Chinese economy posing new challenges to NIEs and near-NIEs in Asia.

I suspect the author did not intend this book to serve as a basic text. Nonetheless, it will be a very good reference, not only for researchers interested in East Asia, but also for final year undergraduate students learning the craft of empirical investigation.

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Nugget Coombs: A Reforming Life, by Tim Rowse (Cambridge University Press, 2002) pp. 430

In 1992 when *Quadrant* asked me to review a much-lauded but intellectually unrewarding book I reflected, ruefully, that 'to review a work one must, after all, first read it'. There are some parallels between that labour and my present task.

Nugget Coombs is hard, and generally extremely dull, reading. It is redolent throughout, not so much of a work of literature (which I take biographies to be), as of an assemblage of notes garnered from voluminous reading (producing an 11-page bibliography and 32 pages of Notes). It is a kind of semi-Bourbon work: while it would be wrong to suggest that the writer has learned nothing, he has clearly forgotten nothing.

Rowse himself says (p. 9) that: 'Some biographies tell the reader what made the subject tick. This one does not'. Underlying that honest self-assessment is the condition, which Coombs rigorously maintained as the price of his co-operation, namely that there be no 'intrusion' on his private life. Understandable, perhaps, but its result is a book that leaves the man behind the public figure largely undiscerned. Scholars seeking a ready reference to some aspect of that public figure's life will find it valuable; but a biography, as normally termed, it is not.

My own personal relationship with Coombs was limited, but during his years as Adviser to Prime Minister Whitlam (1972–75) I saw him operating at close quarters. I rather liked him – another Western Australian who grew up mainly in the bush; a student at Perth Modern School and, later, the University of Western Australia, where he too became President of the Guild of Undergraduates, and later awarded a scholarship for study in England. He had been a more than average sportsman, enjoyed a drink, possessed a great fund of anecdotes, and had no ostentation.

Officially, when we worked together on the Review of the Continuing Expenditure Policies of the Previous Government ('The Coombs Task Force') – he as Chairman, I as the Treasury member and, effectively, chief draftsman – I found him easygoing, approachable, quick on the uptake and efficient with his paperwork. The report in June, 1973 – most of whose recommendations were incorporated in the 1973–74 Budget – was probably his most valuable contribution to Mr Whitlam's term in office.

I have, nevertheless, since come to think that in the final analysis Nugget was a bit of a phoney.

Rowse would strongly contest such a verdict; but it is one of his book's merits that, read with eyes (and mind) open to that view it provides a great deal of supporting evidence.

To begin with, and contrary to the 'iconic' image acquired later, he does not seem to have been outstandingly intellectually gifted. Neither at school nor at the University of W.A. (his Hackett Studentship notwithstanding) did he attract favourable academic attention from his teachers (see, for example, pp. 17–19, 28–29, 35). Leslie Melville, commenting on his MA thesis in 1931, noted that 'when Mr Coombs makes use of economic analysis it is usually poor and often wrong'. His fellow examiner, Edward Shann, was little more complimentary. Of Coombs' L.S.E. doctorate, Rowse himself says that 'Coombs' language was almost comically bereft of historical and political vision'.

Of course, as Paul Johnson's *Intellectuals* testifies, it is not necessarily a criticism to suggest that someone was not an intellectual – unless, he is portrayed as one. Importantly, too, offsetting any lack of sheer intellectual firepower were Coombs' notable gifts as a manipulator of people and as a 'networker'. These qualities gave him throughout his career a wonderful knack of being in the right place at the right time. His appointment in 1948 as Governor of the Commonwealth Bank – the high point of his career – clearly owed itself to the personal relationship by then developed with the Prime Minister (and Treasurer) Ben Chifley. As Rowse records (p. 161), 'no less an authority than Giblin said that the Governorship should have been Melville's'. Years later, the same view could be heard privately within the Reserve Bank itself from those qualified to judge – such as my old friend the late Austin Holmes.

A short review cannot explore in detail Coombs' record, during the War and immediate post-War years, as a public servant. Nevertheless, here too Rowse provides materials for the heavy qualification (at least) of the claims made on Coombs' behalf in that regard. For example (p. 124), overseas in 1943 attending an official conference, Coombs was writing 'long letters weekly' to Mr Chifley – not quite, I think, the role of a public servant. In September, 1948 (after appointment as Governor, but while still heading the Department of Post-War Reconstruction) he disagreed with his then Minister (Dedman) over the future of the CSIR. Coombs was, I believe, right; but in pursuing his own ends he clearly went behind Dedman's back in a most deceitful manner (p. 170). Throughout, numerous other such instances attest to a certain capacity for intrigue that sits oddly with the role of public servant.

One footnote to that observation concerns the report of the Royal Commission on Australian Government Administration, which Coombs chaired, and which reported in 1976. Rowse suggests (p. 331) that Coombs formulated there the notion of the 'responsive' public servant. Was this, perhaps, the rationalisation of his own earlier public service *modus operandi*? Whatever the answer, Coomb's Royal Commission report saw the start of the gradual, and later precipitous, slide away from the previous concept of an apolitical public service and to the shambles in Canberra today.

Even to those Ministers who were his patrons, however, Coombs was not keen on giving advice that he thought they would dislike. Thus, although fully informed (by the Secretary to the Treasury, Sir Frederick Wheeler) of the Whitlam Government's 'extraordinary decision in 1975 [actually, 1974] to raise loans' (p. 307) via the agency of a palpable con-man (and later convicted criminal), Tirath Khemlani, he made no real effort to warn Mr Whitlam of the folly of this course. His subsequent (1989) pleading on that matter was specious at best.

Again, when 'on 15 July 1974 Treasury officer John Stone had given a chilling briefing to the Economic Committee of Cabinet' (p. 304) – as events were to show, there was much to be chilling about – Coombs was merely 'worried that senior Ministers were now being panicked by the short-term forebodings of Treasury'. In short, when unpleasant advice had to be given, Coombs either 'went walkabout' or simply changed the subject.

While still Governor of the Reserve Bank, Coombs employed both the authority of his position and, perhaps more importantly, the Bank's substantial cheque book to carve out a leading role in the arts world – opera, ballet, theatre, painting, even architecture, with new RBA offices erected in every capital (see, in particular, pp. 253–85). Yet, once forced to fall back more (though never entirely) on his own resources, the inadequacies became clearer. His notorious 1974 surrender, as Chancellor of the ANU, to the radical student stupidities of that time (p. 317) was a prime example. The late Professor Fin Crisp (Chifley's biographer and an old friend of Coombs') rightly said, and later detailed in his John Curtin Memorial Lecture, that the University had surrendered academic freedom in its response.

That reference above to going 'walkabout' brings me to what some (perhaps including Rowse) have seen as the pinnacle of Coombs' career, but which might equally be seen as a long descent into the taking of positions that were not only intellectually silly, but that have done great harm to those on whose behalf (in this case, Aboriginal Australians) they purported to have been taken. Geoffrey Partington, in his short work *Hasluck* vs. *Coombs: White Politics and Australian Aborigines* has said most of what needs saying on that topic. Here it is enough to note that as early as 1970 the late Trevor Swan (another old post-war-reconstruction colleague of Coombs', and a genuinely gifted one) was expressing his almost despairing contempt for the shallowness of Coombs' views in this area (p. 326).

When all is said, Coombs enjoyed a remarkably full life: one of these days, and aided no doubt by Rowse's research, someone will write a book about it.

JOHN STONE