Reviews

May the Best Team Win: Baseball Economics and Public Policy, by Andrew Zimbalist (Brookings Institution Press, Washington D.C., 2003), pp. xviii + 198.

Baseball is an American obsession. It is widely regarded as the United States of America's national pastime. It is an activity with a penchant for generating increasing revenues. Following a strike in 1994/95 – which lasted 232 days! – the income of Major League Baseball increased at an annual average rate of 17 per cent (p. 4). And, like other professional team sports in America and across the globe, it has become a source of increasing interest for scholars from a number of disciplines, including economics.

Baseball, like other professional team sports, operates as a cartel. The various teams/clubs that comprise a competition/league need to work out the rules of engagement, both on and off the field. American baseball, however, occupies a peculiar place in the annals of professional team sports. It is a cartel which has obtained exemption from the *Sherman Antitrust Act* 1890. In 1922 the Supreme Court, in *Federal Baseball* (259 US 200 [1922]), ruled that the playing of baseball games were state affairs, even though they involved clubs and players crossing state borders, and did not involve trade and commerce *per* the *Sherman Act*. In *Toolson* (346 US 356 (1953)) and, again in *Flood* (407 US 258 (1972)), the Supreme Court, on *stare decisis*[1], upheld baseball's exemption. This is a privilege which has not been afforded to other American professional team sports and other forms of entertainment. Baseball, in the words of the author Andrew Zimbalist 'is an unregulated legal monopoly' (p. 2).

Zimbalist has published a number of books and articles on the economics of professional team sports, especially baseball. Like any good economist, he abhors monopolies. His purpose in writing *May The Best Team Win* is to expose and attack various problems associated with baseball's operation as a monopoly, and offer policy recommendations for their redress[2]. In presenting his (hardly surprising) recommendations, of the need to remove baseball's *Sherman Act* exemption, and to break up baseball into two separate leagues forced to compete against each other (Chapter 7), Zimbalist is acutely aware that 'rational politics' can and will strike out rational public policy (p. 135). In fact, in the case of baseball, it has always been thus. Notwithstanding this, however, Zimbalist maintains it is 'necessary to understand the industry and how it works' and 'discuss what public policy measures make the most sense' (p. xv).

Literature on the economics of professional team sports has long extolled the virtues of competitive balance. A competition characterised by uncertainty as to who will win is more likely to generate interest from fans, broadcasters and sponsors than a competition where it is obvious who will win. A major concern of Zimbalist is the decline in competitive balance that has occurred in baseball since the mid-1990s. This phenomenon is linked to the widening gap in the revenues of clubs, and the incomes they pay players. Zimbalist finds there is a high correlation between payrolls and sporting success (pp. 42–45). Differences in the incomes of clubs are mainly explained in terms of their negotiation of individual deals with broadcasters. 'Big city' clubs, such as the New York Yankees, have a major advantage over 'small city' outfits, such as the Minnesota Twins. Zimbalist is also concerned, and this has been a long standing interest of his, how subsidies are extracted from the public purse in the building of stadiums, which enhance the wealth of millionaire players and billionaire owners.

One of the great strength of *May The Best Team Win* is the way in which Zimbalist clearly unravels the workings of various markets – labour, product, broadcasting and stadiums – and how they combine to make up the industry that is baseball. He provides a detailed analysis of collective bargaining in baseball. Since 1972 baseball has had eight industrial disputes. Zimbalist demonstrates how the Major League Baseball Players' Association has been able to prevail over a divided and disunited group of owners. He examines how the players' association

[1] The principal that the precedent decisions are to be followed by the courts.

[2] In a sense this is true of all his baseball writings – see, for example *Baseball and Billions: A Probing Look Inside The Big Business of Our National Pastime*, Basic Books, New York, 1992.

used collective bargaining to end the reserve clause; a 'crime' which, until possibly recently, the owners have never been able to forgive. The negotiation of baseball's 2002 collective bargaining agreement was the first time since 1972 there was not a strike or lockout.

Baseball is the only American sport which has utilised revenue sharing – redistributing income from rich to poorer clubs – in attempting to enhance competitive balance. Redistributive mechanisms have been a feature of both the 1996 and 2002 collective bargaining agreements. Given an assumption of profit maximisation, economists would predict that uncompetitive clubs would have an incentive to employ 'inferior' players, lowball salaries and reap the benefits of redistributed income. Zimbalist finds some evidence, and expresses disquiet, concerning this effect (pp. 103–105). Despite this, however, he concluded that baseball's 'revenue sharing transfers will help modestly to alleviate . . . imbalance' (p. 116).

The material dealing with broadcasting examines the various ways broadcasters/clubs attempt to package product in 'gouging' income from consumers. Given that technological advances associated with broadcasting have yet to come to an end, Zimbalist's account of the various issues involved should be compulsory reading for policy makers and broadcasting regulators in other nations. Zimbalist simply applies the notion of opportunity cost to demonstrate that local communities, and taxpayers, more generally, will hardly be better off; in fact, in all probability, they will be worse off, in subsidising the building of stadiums and associated facilities for baseball clubs.

Zimbalist wants baseball's antitrust exemption to come to an end, so its various business decisions can be subject to the rule of law like other sports and businesses, if and when they are perceived to transgress antitrust norms. The baseball industry should be forced to present evidence and argument to justify their decision making – something which it alone has hitherto not been required to do. By breaking up Major League Baseball into two competing businesses, Zimbalist is relying on competitive pressures to enhance consumer welfare and to act as a break on the ability of baseball clubs to play off rival cities/communities to subsidise stadium development.

May The Best Team Win provides a very readable account of major issues associated with the recent operation of American baseball. It systematically examines various peculiarities and nuances of the operation of this legal cartel. Its major contribution lies in its analyses of the impact of recent collective bargaining deals, the various revenue sharing mechanisms they contain to enhance competitive balance and the moving feast that is broadcasting rights. *May The Best Team Win* is highly recommended for all those interested in the economics of professional team sports and the operation of cartels.

Braham Dabscheck
University of New South Wales

Fair Division and Collective Welfare, by Herve Moulin (MIT Press, USA, 2003), pp. 289.

Fairness is a concept we all value. 'Give us a fair go' and 'that's not fair' are words spoken by school children and politicians throughout Australia. But if you have to pin down what fairness is, you'll find that it just isn't that simple. We know unfairness when we see it, but articulating what it is, and isn't, is quite another thing.

Moulin does an excellent job of presenting the attempts by economists to deal with questions related to fair distribution and collective decision-making. Starting from the simple notion of 'equal treatment of equals', he takes us through numerous examples of situations and then offers possible resolutions. Which resolution is fair might well depend on the situation at hand.

In the case of bankruptcy where the total claims exceed what is available, the proportional solution seems fair. This involves giving each claimant the proportion of what is available equal to the proportion of their claim in the total claims. If $2 million is available, and I claim $1 million of a total $5 million in claims, then the proportional solution gives me one fifth of the $2 million.

On the other hand, consider an organisation raising funds to help a cancer patient. If it finds itself in a shortfall and must go back to the contributors for more, it might not seem so fair to apply a proportional solution. Such a solution requires each contributor to pay the proportion of the shortfall equal to the proportion of the total collection thus far. Perhaps it seems fairer to hit those who contributed least.

The text is well organised and comprehensive with concentrations on fair division and fair cost sharing, voting and collective decisions, and problems of common ownership. Each chapter highlights the major contributions while keeping the examples basic enough to allow readers with different backgrounds to grasp them. References to more formal treatments are given, along with an extensive list of exercises at the end of each chapter.

To grasp the contributions of Arrow, Nash, and Shapley in collective decision making would require a more in-depth analysis, which Moulin acknowledges. However, this book gives a nice taste of what there is out there, and it will be likely to spark further interest.

Using four basic principles: *exogenous rights, compensation, reward*, and *fitness*, Moulin is able to capture many aspects of distributive justice and to show how conflicts might arise between these concepts. Should I give a flute to the one whose father owns it, the one who needs it, the one who worked hardest, or the one who will play the sweetest music on it? This simple example, and others are used by Moulin to show the inherent conflicts that lie between the basic principles of justice.

In the process of exploring this wealth of examples, one grows to appreciate the contributions that economists have made towards a better understanding of notions of fairness. Yet, with so many options and conflicts, one might wonder where it all leaves us. On the one hand, the concept of fairness is clearly multifaceted and has aspects that lie well outside the current reach of microeconomics. On the other hand, the research covered in this book has helped people to pinpoint and focus attention on some tractable aspects of fairness.

After reading the book, I still have my doubts about the importance of envy-freeness to our understanding of justice and fairness. According to Moulin, ‘this concept is a major contribution of microeconomic theory to the modern thinking of distributive justice, one nearly as important as the Shapley value’. Yet, I don’t believe he argued this case in the book. Indeed, many economics research papers have been written on envy-freeness, but I think this has more to do with the convenience of application in extant models, than it has to do with it being a representation of justice and fairness. I do concede that in some situations, not envying something can be related to fairness. For example, not envying the procedure of review applied to others seems a reasonable requirement of fairness. But this does not prohibit envy from being part of a fair outcome, nor does the lack of envy make an outcome fair.

Overall, the book is delightful to read and should be accessible to a wide range of disciplines. It is likely to produce greater interest and debate about fairness and collective decisions.

J. Jude Kline
Bond University

Does Money’s Origin Matter? The State, the Market and the Euro, Chartalism vs. Metalism in the Theory of Money, Edited by Stephanie A. Bell and Edward J. Nell (Edward Elgar: Cheltenham, UK, 2003), pp. xiv + 202.

This book is the result of conference held at the New School for Social Research in April 1999. It was inspired by an article published a year earlier in *The European Journal of Political Economy* by Charles A.E. Goodhart on ‘The Two Concepts of Money: Implications for Analysis of Optimal Currency Areas’. It contains Goodhart’s original article, eight rhetorically diverse responses to that article, and a summary article by Goodhart as ‘A Reply to the Contributors’. As such, this book is a short, readable, and valuable contribution toward understanding money’s origin and the role that understanding can play in monetary policy analysis and monetary regime design. It raises some important issues with respect to the design and implementation of the Euro and Euro-related institutions – issues that can be important for the survivability of the Euro, even though, at this point, the Euro’s success seems assured.

As described by Bell and Nell in their preface, the ‘Two Concepts of Money’ are ‘Metalism’ (the ‘M-Theory’) and ‘Chartalism’ (the ‘C-Theory’), and define two supposedly distinct schools of thought about money’s origins. In brief, these schools might be characterised as believing money’s origin was primarily private (the ‘M-Team’), deriving its value from its intrinsic worth like with gold or silver coins (hence, from valuable ‘metal’ commodities), versus primarily public (the ‘C-Team’), deriving its value from the authority of the issuer (like the church or state, which has a social contract, or ‘charter’, to enforce). M-Team ‘players’ are said to include well-known economists and philosophers like Aristotle, Locke, Jevons, Menger, von Mises, Brunner, and Alchian, while C-Team ‘players’ include lesser-known and ‘fringe’ economists such as Knapp (Germany), Mireaux (France), many US and UK post-Keynesians, as well as Keynes of the *Treatise on Money*. The C-Team, however, claims support from allied disciplines such as anthropologists, numismatists, and historians, who are claimed to have explored actual evidence, as opposed to the supposedly unsupported ‘creation myths’ of the M-Team.

The M-Team and C-Team are said to differ in other fundamental ways: over methodology (methodological individualism versus institutional approaches); over theory (money as a medium of exchange that reduces transaction costs versus money as a unit of account for the final settlement of obligations to the state [taxes and fines], the church [tithes], the family [bride prices]); over public policy (*laissez faire vs. dirigisme*); and, I might add, disciplinary status ('enthroned mainstream' versus 'bumptious contenders').

Further, several contributors in this book associate the M-Team and M-Theory with the Optimal Currency Area (OCA) Theory of Mundell, McKinnon, and Kenen, as well as associating both with the creation of the Euro and the Euro supporting institutions. Their view is that such newly created institutions might not survive if based on inappropriate beliefs and understandings, such as those afflicting M-Theorists and OCA-Theorists. A few of the book's authors (in particular, Hudson) actually view the Euro-project as a conspiracy against the public good by mainstream economists. More nuanced and well-balanced contributors (including, significantly, Goodhart) are simply a bit nervous about the Euro-project and apprehensive that dangers lie ahead, while a couple of contributors (Helleiner and Guttman) are relatively objective and non-judgemental regarding both the theories of money's origin and the prospects for the Euro.

Contributor paper titles are fairly self-explanatory. Perry Mehrling's 'Mr Goodhart and the EMU' goes beyond that simple comparison to assess Goodhart's body of work as a background to his position on the Euro. Michael Hudson's 'The Creditary/Monetarist Debate in Historical Perspective' is the most polemical in setting out an extreme chartalist position. Eric Helleiner's 'Some Limitations of the Chartalist perspective: A Comment on "The Two Concepts of Money"' argues that, while the chartalist position has historical support, history also suggests that the reasons for state intervention are more complicated than those encompassed by C-Theorists. L. Randall Wray's 'The Neo-Chartalist Approach to Money' sets out what might be characterised as 'hard chartalism'. Edward J. Nell's 'Nominal Money, Real Money and Stabilisation' emphasises the post-Keynesian concern for fiscal policy as a tool for not only stabilising the economy (he advocates a Keynesian style, 'government as the employer of last resort' scheme) but also for stabilising money (seemingly in contrast to Tinbergen's rule). Robert Guttman's 'Money as a Social Institution: A Heterodox View of the Euro' claims that neither M-Theorists or C-Theorists adequately recognise the 'contradictory dual nature of money' in its public good versus private good functions, which 'left unchecked, can wreak havoc with the stability of our economy'. Stephanie A. Bell's 'Neglected Costs of Monetary Union: The Loss of Sovereignty in the Sphere of Public Policy' quaintly resurrects Abba P. Lerner's parable of the 'Economic Steering Wheel' to lament the Euro's current institutional structure, favouring a revamping to a institutional format that would allow the application of Lerner's 'Functional Finance'.

Charles A.E. Goodhart's 'A Reply to Contributors' concludes the volume by stressing the similarity and differences between his views and those of the contributors, who are mostly his admirers. Significantly, Goodhart here appears more mainstream than most contributors. In particular, he is moved to reject the extreme views and polemics of Hudson, especially with respect to the merits of Mundell's contribution. However, Goodhart reiterates his concern about the structure of Euro institutions and, in particular, whether 'the conduct of monetary sovereignty is buttressed by democratic acceptability and legitimacy'. To me, this concern is really about whether money, particularly the Euro, is an accepted convention. How money has come to be a convention – whether by an M-process or a C-process – is of minor importance, as long as it is a convention. Whether left hand or right hand drive on roads came about because it evolved naturally, or was mandated by authority is less important than that it is as close to a universally accepted, self-enforcing rule as possible within any driving region. Similarly, medium of exchange choice by modern individuals has as much (or even more) to do with expected acceptability by merchants in daily exchange as with acceptability by governments in payment for taxes (especially given near universal withholding) or churches in payment for tithes (given the decline in organised religions). Needless to say, bride prices or blood money no longer play a major role in modern currency choice.

Further, anthropological and historical evidence (and speculation thereon) cannot be taken as ultimate authority, given that such evidence consists of only what has survived. Typically, hard structures, like bones or fossils of species, or the buildings and stone tablets of social institutions like church and state, leave the most easily readable record. Just like the outward appearance dinosaurs is based on 'informed speculation' (expert guesses), the evolution of money might be as much a chartalist myth as a metalist myth. Both myths can be equally useful in explaining to a child or to an economics student why a convention exists. Being a 'soft' chartalist does not preclude being a 'soft' metalist. Polemical discussions between 'hard' advocates of either position are likely to create losers on both sides if a socially valuable convention is undermined thereby.

Finally, from a public choice perspective, monetary sovereignty depends on more than simply 'democratic acceptability and legitimacy'. It depends on appropriate limits to the potential for democratic destruction of money as a public good (such limits as perhaps entailed by current Euro institutions). For those whose case is based on historical 'facts', some contributors to this book seem strangely unaware that 20th century experiments with Keynesian 'fine-tuning' and 'functional finance' often had unanticipated consequences for monetary stability, especially in democratic countries subject to pressure-group influence. Thus, despite claiming concern with institutions and social relations, some authors' contributions appear uninformed by modern public choice – being quite in the 'nirvana' or 'benevolent dictator' mode of previous generations of orthodox policy analysts. Consequently, this book offers an interesting contrast to other modern literature on currency competition and other monetary regimes.

Oh, yes, in answer to the title question: money's origin does matter in theory (and to theorists), but money's survival is what matters in practice.

Zane A. Spindler
Simon Fraser University

Happiness. Has Social Science a Clue? by Richard Layard (Centre for Economic Performance, London, 2003), pp. 60.

Richard Layard believes that social science can provide a clue to happiness, provided that social scientists (including economists) search for Bentham's 'bluebird of happiness'.

One might enthusiastically welcome Layard's opinion regarding the desirable future path of economic thought, while questioning his conclusion regarding peoples' attitude to happiness. He examined some 10 surveys of peoples general states of satisfaction with life. He concluded that they indicated that people usually feel approximately:

Very happy	30 per cent of the time
Pretty happy	60 per cent of the time
Not too happy	10 per cent of the time

People have also reported in surveys becoming less happy over recent years, and that their income relative to that of others was more important to happiness than its absolute size. He concludes from his survey of surveys that 'the happiness of the population has stagnated'.

But these surveys are examples of the weaknesses of quantitative analysis. The views expressed reflect the conditions at the time of the survey. When material conditions improve, expectations regarding future progress improve, and present conditions appear to be relatively less roseate. Hence, people might report that they are becoming less happy when their position is improved by less than they had hoped for.

The conditions under which people respond to a survey will also determine their responses.[3] With the conditions and expectations prevailing in pre-Black Death England, a survey of happiness at that 'tithe might well have had a 30/60/10 pattern. It is even possible to argue that his conclusion regarding people's opinion, would negate the basis of his argument. People will be more willing to substitute 'happiness' for increases in income if their incomes are approaching a satiation point.

Layard accepts that, as measured by GDP, we have progressed dramatically in the last half century. He even concludes that it is possible that we 'already halve a society that is probably as happy as any there has ever been' As income increases, other aspects of welfare become more important. Consequently, he believes that the time has come for economists to expand their interest to a broader objective. Bentham's target of 'greatest happiness of the greatest number' should replace the Samuelsonian 'mathematics is a language' cliché as the centrepiece of economics.

The adoption of utility/welfare as the major objectives of economic theory would raise the importance of a large number of issues.

Given his position with the Employment Policy Institute, it is not surprising that Layard regards unemployment as 'a major disaster' (even for the employed). It is not, as many economists maintain, primarily a loss of income to society, but, as Beveridge has said, as 'rows of men and women, not of figures only' Yet, unemployment continues at high levels in most high-income countries (admitted by ABS to be higher in Australia than indicated by the official unemployment rate).

Layard observes that the fundamental economic principle of decreasing marginal utility indicates that a transfer of income from rich to poor will

[3] For example, Layard quotes a survey that indicates that the attitude to life will be markedly better if the responses are given after sexual relations than if given after an evening commute.

increase ‘happiness’. This raises several questions. What should be done about the deplorable state of present income distribution statistics before criteria for income transfers can be developed? What concepts of relative income should provide the criteria for redistribution. Should those who have temporarily low incomes be eligible recipients, or should transfers be limited to those in long-term poverty? Taxes to finance transfers have a depressing effect on GDP.

Layard’s view on migration raises the question of the scope of welfare analysis. It might be true, as he suggests, that an increase in foreign or domestic migrants reduces local cohesion and, hence local security, but migrants would not migrate unless they thought that they would become happier by migrating. Analysts must balance the Benthamite gains to migrants against the possible loss of local security.

These are only a few of the problems that a revival of Benthamite economics will bring to the fore. But the ‘amusement’ of economists is not an excuse for investigation. Economists should aim for the advance of human ‘happiness’. Layard has indicated an urgent need for a new course for economic analysis in the search for this end.

For that, we should be or grateful.

Graeme S. Dorrance

Economic Growth, Inequality and Migration, edited by Amnon Levy and João Ricardo Faria (Edward Elgar, Cheltenham, UK, 2002), pp. xv + 365.

This book seeks to collect theoretical and empirical papers concerned with the growing debate on the relationship between three major issues in development and international economics – inequality, migration and growth. Without doubt migration, economic growth and inequality are significantly interlinked and this book provides a thorough investigation of their relationships. A rather general overview of the argument suggests that during the growth process inequality can change (either rise or decline), affecting not only economic growth but also earning prospects in various regions. In turn, such prospect of growth and earnings can then trigger migration to these regions. By examining three major themes: (i) inequality and its effects on growth, labour market integration and government policies; (ii) migration and its effects on the host’s countries output, employment and standard of living; and (iii) economic growth and its relationship with trade, capital accumulation and debts, this book has produced a well integrated discussion of these three very important issues that have been strongly debated in economics. The review that follows below illuminates some of the interesting results found in this book.

The book introduces the issues of inequality, migration and growth at both a national and international level. At the international level, the book provides the reader with surveys on theoretical aspects and empirical findings, while at the national level it reviews the literature and offers interesting case studies from developed and developing countries as well as from countries in transition to market economies. Consequently this unique collection of works not only delivers on its objective, but also serves as a good reference for PhD economists and students of economics interested or researching in this area.

The book is comprised of three parts. Part I introduces a series of papers discussing inequality and its affects on economic growth, labour migration and government policy. This part begins with a survey of the literature on the interrelationships between inequality and growth that includes a test of two-way causality between these two variables. The second half of Part I examines the relationship between income distribution and government policy. In an empirical study of a sample 37 industrialised and developing countries, Carneiro, Mello and Tiongson suggest that for those countries where redistributive policies are needed, redistribution is rather limited. In another paper Miranda, using Brazil as an example, develops a model whereby economic groups wielding power attempt to increase their income share at a cost of increasing inflation, and potentially harming economic growth.

Part II deals primarily with migration, its determinants and its possible effects on the host country’s output, in particular unemployment, assimilation and expected returns and risks. Faria (ed.) develops a model with two labour market scenarios: full employment – in which industries pay market clearing wages – and unemployment, in which efficiency wages are paid. He demonstrates that governments can design a tax system that corrects distortions caused by non-clearing market wages which leads to a decline in both urban unemployment and migration from the rural sector to the urban sector. Levy (ed.) on the other hand develops a model of supply and demand for migrants and proposes an immigration feedback rule which is aimed at stabilising the host country’s unemployment rate. A series of other papers in this section of the book discuss: (i) human capital theory of internal migration (Rodgers and

Rodgers); (ii) post-WWII immigration inflow into Australia using an open-economy economic growth model with economies of scale (Landon-Lane and Robertson); and (iii) the relationship between the levels of foreign borrowings and the demographic structure of the economy (Guest and McDonald).

Part III deals primarily with economic growth and its relationship with trade, capital accumulation and internal and external debts. This section begins with a review of studies dealing with trade and its affect on economic growth. The first paper by Arbache presents a review of the literature, both theoretical and empirical, on the debate of the effects of trade liberalisation on wage inequality. In an interesting study using Mexico as an example, Mollick finds no support for the assertion that increased openness to international trade plays a significant role in increasing the wage gap between skilled and unskilled workers. Leon-Ledesman investigates whether R&D spillovers increases competition in international markets. The results suggest that the argument that foreign innovation tends to harm skilled workers at home potentially ignores the possibility that spillovers can enhance export performance of domestic sectors enjoying a comparative advantage. In a series of other papers in this section, the relationship between capital accumulation debts and growth are examined. Martins and Araujo use an overlapping generations model with AK technology and bequest decisions to illustrate that Ricardian equivalence is not generally valid, while Levy (ed.) expands the neoclassical intertemporal maximisation model of growth to include international trade and external debts.

This book is certainly a great reference for anyone interested in development economics not only because of the specific issues that it covers, but also in terms of the general approaches that it presents. Economists and researchers studying development economics and migration studies will find this collection of works of great usefulness and interest.

CHRISTOPHER BAJADA
University of Technology Sydney

The Colour of Credit, Mortgage Discrimination, Research Methodology, and Fair-Lending Enforcement, By Stephen L. Ross and John Yinger (MIT Press, Cambridge, Mass., 2002), pp. 459 + viii.

The catchy title of this book owes its origins to a series of Pulitzer Prize winning newspaper articles, 'The Color of Money', that appeared in the *Atlanta Journal-Constitution* in 1988, that described lending practices that appeared to be discriminatory. This book, then, is concerned with mortgage discrimination, with a particular focus on minority-white loan-approval disparities in Boston in 1990.

A key motivation for the authors of this study was a desire to address the lack of consensus arising from what has been a huge literature on mortgage lending discrimination in the US In the authors' own words, 'the main purposes of this book are to explain what has been learned about mortgage lending discrimination in recent years and then, building on that understanding, both to reanalyse existing data and to devise new tests for discrimination in contemporary mortgage markets . . . We blend the strengths of several existing methods to develop new, straightforward procedures that provide clear estimates of discrimination and avoid the statistical and interpretative problems that have plagued other approaches'. In doing this, the authors provide a comprehensive discussion of the methodological issues that might arise when attempting to determine the presence or absence of discrimination in mortgage lending. Although the authors remain totally focused on their particular theme, the issues they raise and the solutions they offer are likely to be of interest to researchers in other fields of study where problems of endogeneity, omitted variables, sample selection bias, data errors in explanatory variables, misclassification in the dependent variable and incorrect specification are endemic.

The springboard for the empirical work undertaken in this book is a study, originally published as a Federal Reserve Bank of Boston working paper in 1992 (Munnell *et al.*, 1992), and later revised and published in the *American Economic Review* (Munnell *et al.*, 1996). The 'Boston Fed Study', as it is widely known, has been a focal point of much of the literature on mortgage lending in the past decade. It was lauded as a path-breaking, but also has received a considerable amount of criticism.

Ross and Yinger evaluate criticisms of the Boston Fed Study results by re-examining the results, and re-analysing the data with more complex specifications that take into account the concerns raised by various critics. One by one their results are used to highlight estimation issues and the lessons that can be learned are clearly indicated. Their critical evaluation in Chapter 5, preceded in chapters 3 and 4 by background information and a review of the early literature on mortgage lending discrimination, provides a comprehensive overview of what is known and not known about mortgage lending discrimination and synthesises the theoretical and empirical analyses that have been employed in generating this knowledge. Chapter 5 of their book is as close as a serious academic book can become to a detective novel. It is essential reading for anyone tempted to undertake applied economic analysis without a full understanding of the institutional framework within which the problem being addressed is located.

The authors use the results of Chapter 5 to conclude, *inter alia*, that no study has demonstrated the presence or absence of disparate treatment discrimination in loan approval, nor the presence or absence of disparate-impact discrimination in loan approval[4]. Ross and Yinger also conclude that a prime reason for the lack of definitive evidence is that existing studies do not adequately explore the possibility that underwriting standards vary across lenders, some of which may be legitimate (on the grounds of business necessity) but others of which could be illegitimate and might contribute to disparate impact discrimination.

These conclusions provide the basis for the second half of the book, which provides an overview of dimensions of discrimination that are related to but different from discrimination in loan approval, and aims to provide the definitive study on discrimination with data that are publicly available. Chapter 6 begins with a detailed review of the literature that has considered variation in underwriting standards. It builds on this and on analysis undertaken and the lessons learned from the literature reviewed in the first half of the book to establish clearly that differences do exist and provide an overview of the reasons for them. The authors develop a number of ways of taking these differences into account and build a case for concluding that idiosyncratic variation in underwriting standards across lenders is likely to be reflected in the discrimination coefficient in most studies. However, they conclude that, in the absence of loan performance data, it is not possible to determine whether this variation is justified on the basis of business necessity or whether it does, in fact, constitute disparate impact discrimination. Chapters 7 and 8 complete their review of recent literature by concentrating first on redlining (that is, discrimination based on a property's location). This is followed by a review of the literature on differences in loan defaults that brings loan performance information into the analysis (but not without introducing counteracting econometric issues).

The final empirical chapter brings together the literatures on loan approval, loan performance and types of lending discrimination to show how certain types of discrimination, which are likely to be missed by most current research and enforcement procedures, can be incorporated into credit scoring and other automated underwriting systems. The authors devise new tests to capture all types of discrimination and avoid the methodological problems focused on in the earlier chapters.

The book concludes with an analysis of the US federal fair-lending enforcement system. The authors conclude that the current system is seriously inadequate and draw on the results of the earlier chapters to develop two new enforcement tools to address the weaknesses identified in current practices.

The seemingly narrow focus of the book might suggest to the casual reader of this review that the study has little to offer to researchers outside of the US. This casual assessment is likely to be reinforced by a reference list of several hundred books and papers. Whilst this comprehensively covers the US literature on discrimination in the last two decades, it contains no reference to non-US material. Passing the book over on these grounds, however, would be a mistake. Such a decision would rely too much on the weaknesses of much of the housing and mortgage market literature that comes out of the US, and would pay too little attention to the strengths of the best of this material. For the international reader, this book displays both the weaknesses of its focus on US institutions but it also displays the strengths that can come from a clear understanding of the framework within which applied economic research must be undertaken.

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JUDY YATES
University of Sydney

[4] 'Disparate treatment discrimination' is defined as the presence of factors that explicitly discriminate (and so provide overt evidence); 'disparate impact discrimination' is defined as differences not fully explained by legitimate non-discriminatory factors. These terms are explained in more detail in the FFEIC (Federal Financial Institutions Examination Council) guide to fair lending.

Keeper of the Faith. A Biography of Jim Cairnes, by Paul Strangio (Melbourne University Press, Melbourne, 2002), pp. 340.

The dust jacket describes this book as ‘a masterly portrait of one of those rare people who never waver in their search for truth’. To any detached observer who labours through its 395 pages (plus notes), this might suggest two comments. First, throughout his political career Dr Cairns seems to have had a quite remarkable difficulty in deciding where the truth lay. Second, and more serious, there is a persistent pattern not of searching for the truth, but of rejecting any evidence not conforming to his vision (to use a kindly word) of that ‘truth’.

These generalisations noted, let me dispose of some preliminaries.

This biography covers, naturally, Cairns’ whole life. His role in Australia’s economic affairs (the aspect of principal interest to *Economic Record* readers) constitutes only about 30 per cent of the book (roughly, pp. 228–334). While as a matter of duty I have read the work in its often mind-numbing entirety, I shall focus almost entirely on that segment.

I should also mention my own involvement with the events recorded there. I was Deputy Secretary (Economic) to the Commonwealth Treasury when the Whitlam Government came to office in December 1972 (supported by my own, subsequently regretted, vote). I remained so throughout that government’s life, including during the period (November, 1974 to June, 1975) when Cairns was my Minister. Prior to his appointment as Treasurer, I had little personal contact with him although the ‘Kirribilli House’ meetings following the 1974 election allowed me to observe him at first hand.

My own name appears only once in Strangio’s pages (a glancing reference on p. 291). I note this not from any sense of personal pique – quite the contrary – but rather because in most accounts of the Whitlam Government’s economic record I have almost invariably been cast as a (or even the) chief demon. Perhaps Strangio’s more considered researches have led him to a different conclusion?

Still, it is remarkable that in discussing the Khemlani Loans Affair and the 1974–75 Budget, Strangio makes no mention of two key documents with which I was associated.

The first is my minute to Cairns of 10 December 1974 about the US$4 billion borrowing proposal via the agency of Khemlani. Now this omission is odd. Strangio spends many pages seeking to rationalise Cairns’ extraordinary behaviour throughout the Loans Affair. Yet he fails even to mention a Treasury minute which, three days before the fateful Executive Council meeting of 13 December 1974, laid out a host of compelling reasons why the Government should reject proposals which were to do more than anything else to bring it down.

The second omission is the Cabinet Submission by Cairns’ predecessor, Frank Crean that was written by myself and Bill Cole, of Treasury’s General Financial and Economic Policy Division. This summed up, prophetically, the likely consequences of the 1974–75 Budget Cabinet meetings over which Cairns and his colleague Tom Uren, and their then misguided adviser, Dr Michael Keating, had in practice presided.[5]

This second omission is equally significant. In effect, the views briefly encapsulated in Crean’s Cabinet Submission signalled the end of Keynesianism as we knew it. That recognition was publicly summarised a year later in a passage (which I admit to having drafted) in the 1975–76 Budget Speech of Cairns’ successor, Bill Hayden, namely:

> ‘We are no longer operating in that simple Keynesian world in which some reduction in unemployment could, apparently, always be purchased at the cost of some more inflation. Today, it is inflation itself which is the central policy problem. More inflation simply leads to more unemployment’.

Yet although Strangio records Hayden’s words (p. 338), he clearly entirely fails to understand (or accept?) their significance. Thus, in his epilogue, he says (pp. 390–91):

> ‘The Whitlam Government came under pressure to abandon the ideal of full employment and suspend its social reform program. Labor’s conservative critics demanded that Keynesian economic management give way to a neo-liberal deflationary policy’.

It is wearisome to have still to correct such stuff. The Whitlam Government came under no such ‘pressure’. What it did receive (although prior to its last 6 months in office, it largely disregarded it) was honest advice from loyal public servants – and I speak, of course, particularly of the Treasury – who sought to inform it of what they had come to see would inevitably be the disastrous consequences, for the Government itself and for Australia, of pursuing

[5] This submission appears in Michael Sexton’s *Illusions of Power*. The minute appears in Alan Reid’s *The Whitlam Venture*. Both books appear in Strangio’s bibliography.

the grossly inflationary policies on which, from the outset, it was hell-bent.

True, that apprehension – that any return to full employment could now only be achieved by 'fighting inflation first' – was a novel one when first formulated. Certainly, it found no comprehension among most Australian professional economists at the time, and notably not among the so-called 'Adelaide School' (Geoff Harcourt, Barry Hughes, etc.) prominent among Cairns' unofficial advisers. Post-Whitlam, the Treasury preached that lesson wherever it could – chiefly in its annual Budget Statement no. 2 disquisitions, but also in the OECD's Economic Policy Committee (where it came to be called 'the Canberra Model'), the IMF's Executive Board, and so on. Today it is generally accepted throughout the world.

All of which raises the question: why, after all this time, and all that experience, do the Strangios of this world simply not get it?

In truth, Strangio's whole assessment of his subject's economic record is flawed throughout by persistent denial of reality. The Whitlam Government's most crucial period (following the 1974 election and culminating in the 1974–75 Budget), when Whitlam himself strove mightily to save his already foundering ship, is portrayed as one of Cairns successes, with seemingly no comprehension of what those 'successes' brought in their train. The 22 July mini-budget, when Whitlam's endorsement of Treasury's proposals was thrown in his teeth by a Cairns-led Cabinet, is viewed as a triumph: it 'marked the virtual eclipse of Treasury as a source of economic advice. . . .' (p. 291). True. And the consequences?

The fact is, I believe, that Cairns was never up to the job from his first day in office as a responsible Minister of the Crown. Indeed, it is one of the not otherwise notable merits of Strangio's book that, read carefully with both eyes open, that fact will be apparent – albeit not, it seems, to its author. As someone said (in a theorem reminiscent of Occam's Razor), if it looks like a duck, walks like a duck, and quacks like a duck, it is almost certainly a duck. Strangio's appears to have been unable to apply this simple theorem to the life and career of his subject, Jim Cairnes.

Strangio says (p. 4) that his book does not offer 'a fully fledged psycho-biography of Cairns', noting that the absence of private papers militates against that anyway. Nevertheless, the book is heavily laden with the encumbrances of such an approach. Perhaps the author, who clearly holds his subject in almost idolatrous esteem, had in mind the old French tag, *Tout comprendre, c'est tout pardonner*. Certainly Dr Cairns had much for which to be forgiven.

JOHN STONE
Samuel Griffith Society