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BOOK REVIEWS

Marshall's Tendencies: What Can Economists Know? By SUTTON (JOHN). (Cambridge, Mass. and London: MIT Press, 2000. Pp. xvi+122. £15.50 hardback. ISBN 0 262 19442 2.)

Economics accounts for the allocation of resources and the associated interactive decisions of individuals. It explains the past and predicts the future.

A theory imposes restrictions on observations: it claims magnitudes and patterns of dependence over time and across variables. Typically, restrictions follow from models that relate observable and unobservable structures and parameters.

Observations may fail to conform to a theory and thus falsify it. Following Popper (1959), this is hallmark of science; as Feynman (1965) described it, first we guess; then we compute and compare the result of the computation with nature; 'if (theory) disagrees, with experiment, it is wrong; that is all there is to it'. Others, practising scientists and Kuhn (1962), among philosophers, have argued that falsification is not how science progresses; according to Chomsky (1999), '… nothing works like [Popper's method of falsification.] … The question is … is there some other way of looking at the apparently refuting phenomenon, so as to preserve or preferably enhance explanatory power, where part of the phenomenon falls into place and the other half turns out to be irrelevant'? The argument does not claim that falsification would not put an end to a theory; it describes how scientists defend their claims against falsification. Falsifiability and the power to explain and predict, nonetheless, do not exhaust the interest and contributions of a theory: it provides intuition and understanding or, as Weinberg (2001) put it, 'if I had to give an a priori definition of explanation in physics, I would say ''explanation in physics is what physicists have done when the say Aha!'''. But *a priori* definitions (including this one) are not much use.

Neoclassical economics, the currently dominant approach, builds on the premise of equilibrium: not the rest point of a dynamical process; rather, a situation that satisfies the internal consistency necessary for intelligible discourse. The inability to consider out of equilibrium behaviour is, nevertheless, a drawback that game theory, for instance, attempts to overcome.

Whether economics performs adequately its role as a scientific discipline is a question that does concern academic economists.

On the optimistic side, macroeconomic management is credited with the dampening of aggregate fluctuations in the second half of the twentieth century, though Romer (1986) contests the claim.

On the pessimistic side, criticism of the performance of economics comes in many forms. Experimental evidence suggests that individuals, faced with uncertainty or conflict, do not display rational behaviour as economic theory posits; this work was deemed sufficiently important to be recognised by the 2002 Nobel memorial prize in Economics.

Doubt about the adequacy of economics for the explanation and prediction of phenomena in its scope was also expressed from the very core of economic theory. A conjecture and early work by Hugo Sonnenschein (1973, 1974) and definitive demonstrations by Gerard Debreu (1974) and Rolf Mantel (1974) established that any function of prices that satisfies the minimal requirements of homogeneity or absence of money illusion and Walras' law or the budget constraint can be generated as the excess demand function of individuals that maximise concave monotone utility functions; aggregation dissipates the restrictions that rationality imposes no restrictions on aggregate consumer behaviour.

The result was interpreted as proof of what many had suspected: general equilibrium theory, with fundamentals unrestricted beyond transitivity and decreasing marginal rates of substitution, has no observable implications.

Rumour has it that James Tobin, who held strongly that economics can and should be used to alleviate need and improve general welfare, considered the result of Sonnenschein – Debreu – Mantel as a result that should not have been proved; Kenneth Arrow (1991) asserted that 'in the aggregate, the hypothesis of rational behaviour has in general no implications.'

Recent work has reversed the intuition initially derived from Sonnenschein – Debreu – Mantel. Brown and Matzkin (1996) pointed out that what matters is not the excess demand function for fixed fundamentals; rather, the comparative statics of prices and allocations as fundamentals vary. They proceeded to show that arbitrary comparative statics are not compatible with rational choice and market clearing. Subsequently, Chiappori, Ekeland, Kübler and Polemarchakis (2002) refined the argument by showing that knowledge of the correspondence that associates endowments to equilibrium prices suffices to recover the profile of preference of individuals; thus, rationality and equilibrium not only impose observable restrictions, but they allow for the prediction of the consequences of economic policy interventions. These results, demonstrated in minimal, abstract settings, require elaboration to be taken to the data; but they make convincingly the point that equilibrium economics is falsifiable.

John Sutton has been a leading player in the development of empirical industrial organisation with solid theoretical foundations. In Marshall's Tendencies: What can Economists Know ? he addresses 'the practical difficulties we face in carrying out model selection exercises with a view to testing theories within the framework of the standard paradigm'. He develops the argument as follows:

1. Explanation and prediction in the physical sciences are possible thanks to the presence of strong influences, relative to which, multiple other influences that are operative are secondary: gravitation is a determining influence in the movement of the tides, while meteorological factors, though relevant, are of a lower order of magnitude.
2. There are instances, where the same approach works for economics: non-arbitrage is a determining influence in the pricing of derivative assets, which

accounts for the practical success of asset pricing following Black and Scholes (1973).

3. More often than not, economics does not have recourse to determining influences: in Debreu-Mantel-Sonnenschein, there is no structure to aggregate excess demand as prices vary or similarly, the reaction of an oligopolistic market to an increase in market size is indeterminate.
4. The remedy is (a) not to insist on fully specified models: in Hildenbrand (1983) simple restrictions on the distribution of characteristics suffice to generate the law of demand or (b) in Sutton (1991), a general class of multistage games, that allows for a wide menu of specifications of the entry process, generates a lower on the maximal market share or one-firm concentration ratio.

The focus on the frequent absence of determining influences in Economics is insightful; and it can explain convincingly why economic policy cannot claim triumphs comparable to those of engineers or applied scientists.

Concerning the remedy of not insisting on a full specification or, equivalently, considering classes of models, indeed this is a way to go – possibly the only way. It remains to understand how and to see whether success stories can offer guidelines, asset pricing is a case in point. In a way, it is an instance of a less than complete specification that does the job: fully specified, equilibrium models allow for pricing of all assets – not only of spanned derivatives – but lead to results of indeterminacy, as in Constantinides and Duffie (1996) or Kübler (2003). The insight of Black and Scholes was that the pricing of derivatives decomposes from the general equilibrium pricing of fundamental assets and is solvable. More generally, the method that works is the identification of decomposable, manageable problems.

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The Role of Social Capital in Development: An Empirical Assessment. Edited by GROOTAERT (CHRISTIAAN), VAN BASTELAER (THIERRY) and with a foreword by ROBERT D. PUTNAM. (Cambridge and New York: Cambridge University Press, 2002. Pp. xxii+360. £45.00 hardback, US $60.00 hardback. ISBN 0 521 81291 7.)

Group Behaviour and Development: Is the Market Destroying Cooperation?. Edited by HEYER (JUDITH), STEWART (FRANCES) and THORP (ROSEMARY). (Oxford: Oxford University Press, 2002. Pp. xvii+364 £19.99 hardback. ISBN 0 19 925692 6.)

In recent years there has been renewed interest among economists in understanding the influence of society and culture on individual decisions. A new theoretical and empirical literature examines the nature of social influence, and the way in which it leads to the emergence of norms. On the theory side two main strands can be identified. A game theory literature, with origins in Thomas Schelling's famous papers on neighbourhood segregation, examines how locally uniform patterns of behaviour (i.e. 'norms' or 'conventions') emerge in society. A separate literature, influenced by the work of Robert Putnam and James Coleman, studies how networks of relationships affect individual behaviour and collective choice. Central to this theory is the concept of 'social capital' – i.e. the features of social organisation (specifically, social networks and their associated norms and values) that facilitate coordination and cooperation. The concept of social capital has gained considerable prominence in public policy debates, where it is deemed to be a determinant of the success of poverty alleviation and development programmes. It is to this discussion that the books reviewed here contribute. They are the product of initiatives that generated a number of case studies, which are documented in rich detail by the contributors to the two volumes. The studies are diverse, yet each volume is tightly organised and delivers a consistent message. The books add substantially to the empirical base from which the role of social influences in development will ultimately be assessed, and are therefore both timely and welcome.

Although they have much in common, the books provide an interesting contrast in focus. The studies in *The Role of Social Capital in Development* (hereafter SCD) were undertaken as part of the World Bank's Social Capital Initiative. The threefold objective of the Initiative was 'to assess the impact of social capital on development outcomes, identify cases in which outside assistance facilitated social capital formation, and develop indicators for monitoring social capital' (SCD, page 7). The effort to draw lessons for governments and donors, and for the administration of projects is clearly discernable. The studies do not see, and in fact are not designed to look for, any broad conflict between markets and social capital. In contrast, *Group Behaviour and Development* (hereafter GBD) asks, in its very subtitle, 'Is the market destroying cooperation?' Studies in this collection,while agreeing with SCD about the significance of social norms, express serious reservations about policy shifts that advocate a stronger role for market incentives and material rewards. For instance, Stewart sees a conflict in the present stance of development institutions such as the World Bank that seek to advance market models, and at the same time argue for more social capital: 'There is an inherent inconsistency between giving an overriding role to a market model of development, based on individual maximising behaviour, and the adoption of more socially oriented norms' (GBD, page 49).

An objective of SCD is to understand geographical variations in economic development – why do apparently similar communities differ in the degree of success with which they manage common resources, and why do countries with apparently similar endowments experience different rates of growth? The claim is that social capital can effectively account for these variations. However, the measurement of social capital poses serious challenges. Social capital is not tangible in the sense that physical capital is, and unlike human capital it has no obvious proxies. Researchers often use some index constructed from membership in associations and networks and other measures of participation in community activities. What is somewhat problematic is that the best indicator could vary from one context to another, leaving the choice of proxy to the judgement of the researcher. For instance, in their piece on social capital and watershed management in Rajasthan, Krishna and Uphoff write, 'The measures that Putnam employed in his study of social capital in northern Italy tell us little about social capital in Rajasthan' (SCD, page 103; see also Pargal *et al.*, page 201). This is not to suggest that the enterprise is futile, but rather that measurement of social capital calls for good judgement and careful justification. The case studies demonstrate how the construct can be meaningfully measured, and SCD is likely to become a useful resource for researchers in this field.

The general conclusion of the authors of studies in SCD is that social capital matters significantly, and has a beneficial impact on group outcomes. However, I am not entirely convinced that we are justified in reaching such a conclusion yet. The familiar difficulty, of inferring causation from correlation gets magnified in the study of social interactions. For instance, suppose that people belonging to some voluntary association also experience favourable outcomes. This could be because of the positive effect of association (i.e. social capital), but it may also be the case that people choose to associate based on their propensity for high achievement. While some of the papers do attempt to address the problem, the difficulty is a substantial one and needs greater attention before we can infer broad lessons for the conduct of policy. It is instructive that the one study that is best able to estimate causal impacts reaches an essentially negative conclusion – Gugerty and Kremer examine the effectiveness of efforts to build social capital and find little evidence that such efforts are successful. As a result, SCD reaches an appropriately qualified conclusion on the possibility of successfully investing in social capital, but perhaps such caution is warranted for the assessment of social capital impact as well.

The focus of GBD, on how group functioning and performance are affected by a shift to market-oriented reforms, leads the authors to devote attention to somewhat different questions. The individual studies in GBD examine how the mode of operation of groups influences outcomes, but also ask how group behaviour is influenced by societal norms ('macro-norms'). The overall claim is that macro-norms have a strong influence on group behaviour and that a shift to market liberalism, by changing macro-norms, destroys cooperation. As evidence, the editors cite studies such as that of Sneath on the decollectivisation of the Mongolian pastoral economy. Sneath describes the failure of attempts to establish cooperatives that replicate some of the functions of dismantled pastoral institutions. The reasons for failure appear to be complex, and it is certainly plausible that the dismantling of long-standing collective institutions could destroy cooperation. However, I am not convinced that the collection of studies in GBD, pre-selected as they are, together provide support for a general case against markets.

An appealing feature of GBD is its use of alternative theoretical perspectives in explaining group behaviour. While some of the studies adopt a framework centered around individual maximising behaviour, others assume individual self-interest to be secondary, and choose to explain action in terms of collective notions such as group identity and loyalty. For instance, in her study of political activism among sex workers in Calcutta, Gooptu explores how 'engagement in group action transforms individual motivation and behaviour, rather than the opposite' (GBD, page 228). Group activity contributes not just to the construction of community, but also leads to a reevaluation of an individual's identity, capabilities and goals. GBD makes a persuasive case for the use of a social perspective in the study of group behaviour and development.

In conclusion, the two books contribute significantly to our understanding of social influences and group behaviour. They are likely to be a valuable asset to researchers and practitioners in the field of Development, and should be of interest to a wider readership as well.

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A Theory of Economic Growth: Dynamics and Policy in Overlapping Generations. By DE LA CROIX (DAVID) and MICHEL (PHILIPPE). (Cambridge and New York: Cambridge University Press, 2002. Pp. xvii+378. £55.00 hardback, US $75.00 hardback, £20.95 paperback, US $28.00 paperback. ISBN 0 521 80642 9, 0 521 00115 3.)

The overlapping generations model of Samuelson, Diamond and Allais has been for three decades an important instrument of policy analysis. However, the role played by the demographic structure in why and how economies grow was not recognised: 'The mechanics of growth', as Lucas would say, seemed to owe little to demography. But times are changing. The overlapping generations model has started a second youth and is now a 'growth model'. The book by David de la Croix and Philippe Michel comes at the right time as it provides the necessary foundations for this renewal. Indeed, the book gives an exhaustive analysis of the overlapping generations model with simple production. It also provides an up-to-date and complete analysis of the issues for which the model happened to be useful. Aimed at graduate and PhD students, it nicely complements its predecessors, in particular, Azariadis' Intertemporal Macroeconomics and Wallace and McCandless' Introduction to Dynamic Macroeconomics.

De la Croix and Michel have chosen the strategy of being exhaustive on one model, the Diamond model, instead of covering a variety of models. I will develop three points. In 1965, Diamond introduced production in the pure exchange overlapping generation model of Samuelson. Most of the literature on overlapping-generation with production has followed his formalisation. De la Croix and Michel also unconditionally adopt this model and its philosophy. Diamond made a strong assumption though: accumulation of capital is the only way agents can save. This is far from innocuous and rules out one of the most important features of general overlapping generation economies, the indeterminacy of equilibria. Clearly, eliminating indeterminacy makes the model 'presentable' and easy to use in addressing policy issues. However, as other assets than capital can be found out there in the real uncertain world, one should probably be careful not to simply ignore this issue.

Focusing exclusively on the offspring of the Diamond model also eliminates some of the 'best of' of the overlapping generation literature. In particular, the 'esoteric' aspects of the model are minimised. For example, indeterminacy is a major source of Sunspots when the economy is imbedded in an uncertain environment. Another example is provided by the heated debate on why the first welfare theorem fails in this model. Important references as Shell's 'Notes on the economics of infinity' and Brown and Geanakoplos' 'Understanding overlapping generation economies as lack of market clearing at infinity' concern pure exchange economies.

The reader could also be surprised by the absence of some obvious generalisations, in particular to two sector production economies. Models with long-lived consumers are mainly absent from the book. Finally, altruism and human capital are indeed treated, but to a much less extent than the basic Diamond model.

However, the authors had to make a choice. They excel in the treatment of all generalisations that keep the basic Diamond's structure. And as such the book contributes significantly to the literature. The structure of the book also reflects this choice. Chapter 1 contains the set-up of the model and the main definitions. Chapter 2 is devoted to the traditional issue of optimality. Chapter 3 is devoted to all policy considerations while Chapter 4 is dedicated to Debt. The inclusion of altruism and of human capital is confined to Chapter 5, Further issues. The book carefully follows the tradition established by its predecessors. It shows absolute rigour in defining the concepts and in proving the results. It provides extensive analysis of the impact of the various policies, as changes in the tax scheme, debt etc. Finally, some space is devoted to (another) survey on mathematical tools. The plan underscores the fact that little attention is devoted to the link between the data and the models. This could be seen as a weakness particularly in view of the role that empirical analysis occupies in the endogenous growth literature.

To conclude, this is an excellent book in the tradition of the best books available on overlapping generation models. Even though it probably doesn't provide 'A Theory of Economic Growth' it is yet the final word on the Diamond model and its extensions. As such it is an indispensable tool allowing graduate and PhD students not to reinvent the wheel.

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Rational Ritual: Culture, Coordination, and Common Knowledge. By CHWE (MICHAEL SUK-YOUNG). (Princeton, N J: Princeton University Press, 2001. Pp. xiv+130. US $16.95 paperback. ISBN 0 691 11471 4.)

This book highlights one particular aspect of culture – that of enabling common knowledge among individuals. A fact is common knowledge if everybody knows it; everybody knows that everybody knows it; and so on *ad infinitum.* For Economists and Game Theorists, it is well known that common knowledge (and not just mutual knowledge) is essential for achieving coordination (Rubinstein, 1989). In a nutshell, if participating in a public enterprise – from stag-hunt to underground activities – would turn very sour for each participant unless everybody (or at least enough other people) participates, each individual might avoid the enterprise altogether in the lack of *common knowledge* that everybody indeed opts in. Embarking from this theoretical insight, Chwe discusses a remarkable array of examples of social institutions and arrangements that bring about common knowledge. These include public ceremonies, rituals, parades and dances to announce and recognise authority and norms; inward facing architecture; focal-place-and-time advertisement of items whose value to individuals hinges on collective consumption or awareness; cohesive social networks; and many more. The appendix delivers an accessible presentation of the abstract formulation of knowledge and common knowledge.

A careful distinction is made between the content of public activities and the fact that they induce common knowledge. Though the two aspects can seldom be completely delineated in practice, Chwe makes a convincing case for his argument that if common knowledge were not necessary for coordination, some of these public activities could and would look different.

At a first glance, this book looks simply like an enthusiastic attempt to popularise an abstract, theoretical point. But in fact, the book delivers more than that. The relentless juggling of examples and ideas does induce a meditative state of mind, illuminating further new angles and connections, even if not developed explicitly.

For example, if common knowledge is so central because it enables (or almost implies) the vital coordination essential for existence and well-being, this may shed light on the human tendency for conformity. The adherence to whatever is widely and publicly accepted may thus be an adaptive trait. Trying to convince others of the advantages of one's idea might be self-detrimental if it precludes common knowledge of *whatever* idea. With this in mind, it is no wonder how determined tyrants are able to impose their rule and authority once they succeed in making it commonly known. This elaborates on Simone Weil's (1973) point that, paradoxically, the masses are vulnerable to oppression not albeit they are many but exactly *because* they are many.

The seductive power of common knowledge is not limited to totalitarian regimes. Chwe elaborates the fact that TV advertising during the super bowl in the United States makes it common knowledge that many others have seen the same commercial, which is essential for 'social goods' (like beer, pizza or computers), part of whose value is based on them being popular. But the same applies to political ideas: Only widely announced and discussed policies have the chance of becoming common knowledge (and therefore achieve coordination in polls or at the ballots), which may account for the huge effort of politicians to appear in public or in prime time on TV. This reasoning is distinct from the typical Political Economy models, which hinge on the information superiority of the politician *vis-à-vis* the public (due to complex or secret information from technical experts accessible only to the politician, or information about the true innate abilities of the politician or her opinions). Of course, asymmetric information can accrue information rents and bias the behaviour of constituencies in the direction favoured by politicians, but the tug-of-war among politicians to create the public image and common belief that they are widely supported may be no less important.

Indeed, time and again we see how the public opinion in the polls shifts following widely broadcasted announcements of leaders, even when these contain virtually no new information, and apply to vital issues like the onset of war. This highlights the non-neutral role of the media even when it simply and objectively portrays the statements of politicians, and suggests new perspectives on issues like professional ethics of the media and its role in a 'checks and balances' democracy.

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Property Rights: Cooperation, Conflict, and Law. Edited by Anderson (Terry L.) and McChesney (Fred S.). (Princeton, NJ: Princeton University Press, 2003. Pp. x+398. £45.00 hardback, £19.95 paperback. ISBN 0 691 09997 9, 0 691 09998 7.)

This book looks at the significance and evolution of property rights. It explores the theme that rational individuals who compete for scarce resources need some rules to regulate and govern the resulting competition. Such rules can possibly include, violence, 'might is right', societal norms, or formal and informal property rights defined by some political institutions such as a government. The choice among the many diverse rules is a cost-benefit decision that applies the standard 'marginal principle'.

The book has thirteen chapters which are in turn organised into six parts. In Part I, Edwin West looks at the views of the classical economists on the issue of property rights; relative to modern neoclassical economics, their emphasis is on the normative basis for property rights. Yoram Barzel argues for the importance of incorporating transaction costs (defined as the resources used to establish and maintain property rights) into the neoclassical competitive model. He also illustrates and extends Cheung's argument about the difficulty of separating transactions that occur within firms and markets, an issue that lies at the heart of the modern theory of the firm.

In Part II, Thráinn Eggertsson examines the differences between two types of property rights: 'open access' (resources on which there is no restriction on access) and 'common property' (access is limited to members of a group). The classic example of the former is 'tragedy of the commons'. A range of solutions such as private ownership, government regulations and transferable quotas are considered. Exclusion from open access can be potentially costly for the government to enforce, and in some instances, social norms are much cheaper to define and enforce. Hence, under certain circumstances, open access problems can be solved independently of government intervention by the formation of common property rights. Louis De Alessi presents empirical evidence to demonstrate the gains from well-defined and enforced property rights, as well as the choice among various property rights regimes. Evidence shows that ill-defined property rights lead to inefficient outcomes and that open access often leads to overexploitation and indeed premature exploitation of the resource. This has been the case, for instance, for open access oyster beds. He also illustrates the inefficiency that might sometimes be associated with government regulation, especially through bureaucratic corruptibility.

In Part III, Terry Anderson and Peter Hill discuss how property rights might emerge endogenously. They introduce a rational 'institutional entrepreneur' who stakes a claim, based on the net private benefits of doing so, to an asset previously operating under open access. There are potential welfare implications of such institutional transformations, particularly if the original open access situation involves a substantial 'tragedy of the commons' problem. Technological changes are also fairly important to understand the evolution of property rights. For instance, the introduction of the telephone has made one's home an 'open access' resource for telemarketing companies. Similarly, the invention of barbed wire facilitated private property where once open access was optimal on account of exclusion difficulties. Gary Libecap discusses problems of private contracting and bargaining of property rights with several interesting examples. David Haddock examines an alternative institutional rule for defining and enforcing property rights: 'might is right'. In equilibrium it is not necessarily the case that this solution will involve a degree of violence; indeed the threat of violence can eliminate violence under certain conditions.

The remaining parts of the book look at the role of the government in defining and protecting property rights. In Part IV, Dean Lueck examines the arguments for using 'first possession' as the basis for defining property rights. This is clearly important for determining investment in intellectual property rights and in the choice of public policy towards patents and copyrights. The government, by using its monopoly powers of coercion, can reap economies of scale, to keep the costs of defining and enforcing property rights, low. However, under certain conditions the government, or its agents, might misuse the monopoly power substantially, lowering the benefits of government involvement. The optimal role of the government, relative to private solutions, then, depends on the outcome of such calculus. Such issues are analysed by Fred McChesney.

Part V examines the role of public policy in dealing with the problem of externalities. Bruce Yandle compares and contrasts the well-known approaches of Pigou and Coase to the problem of externalities. In chapter 11 Harold Demsetz looks at private contracting solutions between parties to internalise Pigouvian externalities, such as vertical integration. But he also points out the possibly increased transaction costs as a result.

Part VI continues with the theme of government action and asks how society might protect itself against a self interested government that has monopoly rights over defining and enforcing property rights? One solution is obviously to place constitutional limits on the governments' powers in such matters; an idea that goes back to Madison. Within this theme William Fischel looks at the arguments for giving powers to local governments to define and enforce zoning laws. Richard Epstein looks at the practical issues involved in transferring property between distinct parties as a result of the government's attempt at defining property rights.

The book covers a wide terrain, and examines several interesting issues in public economics as well as in law and economics. At its core, the set of central messages are not much different from what might be gleaned from the leading undergraduate books in these areas. The book uses mainly the verbal/graphical method of argument, which might be one reason for the omission of several other interesting, but technically more advanced, approaches to property rights such as the incomplete contracts literature. There are nevertheless, some interesting ideas for the advanced reader and a collection of several important stylised facts that could facilitate further research.

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Deliberate Discretion?: The Institutional Foundations of Bureaucratic Autonomy. By Huber (John D.) and Shipan (Charles R.). (Cambridge and New York: Cambridge University Press, 2002. Pp. xv+284. £47.50 hardback, US $65.00 hardback, £17.95 paperback, US $24.00 paperback. ISBN 0 521 81744 7, 0 521 52070 3.)

Huber and Shipan's book tackles an important issue for the understanding of the relationship between politicians and the state bureaucracy in determining policy decisions. This issue has received a significant amount of attention in political science literature but has so far not been tackled in the economics literature. Taking into account the explosion of the literature on political economy, this is rather surprising since it is clear that any understanding of the policy making process should be a crucial question for anyone interested in the relationship between political institutions and economic outcomes. In the light of this, I believe it is useful to provide a brief overview of the literature as it stands to emphasise the book's specific contributions.

There is an increasing interest amongst economists on the question of whether collective decision making should be allocated to politicians who are accountable to citizens as a whole or to expert bureaucrats. In the past, this question has been very narrowly focused on the issue of central bank independence, the argument being that monetary policy be allocated to an agency which is immune from political accountability so that the time-inconsistency problem does not arise. (Kydland and Prescott, 1977; Barro and Gordon, 1983). Very recently, however, the literature has begun to look at the broader issue. Maskin and Tirole (2001) and Alesina and Tabellini (2003) consider the trade-off between bureaucrats and politicians in much greater generality and show how the choice depends on different factors: for example, politicians should be in charge whenever policies do not require too much expertise while bureaucrats are preferable whenever there is a risk that large majorities or powerful interest groups may exercise excessive influence on politicians.

The literature's approach to the issue has been driven by a fundamental assumption: in modern democracies, citizens don't have the time, ability, knowledge necessary to undertake collective decision making directly and so they have to appoint agents (politicians and bureaucrats) who act on their behalf. This is a very plausible argument, but it can clearly be applied to politicians and bureaucrats as well because they also need agents in the form of lower ranking bureaucrats. A question that arises then, is whether the analysis of agency relationships at a level where principals are politicians and agents lower ranking bureaucrats can add to our understanding of collective decision making in democracies or not.

At the theoretical level, the problem is interesting for at least three reasons. In the first place, the principal-agent relationship between citizens and politicians/bureaucrats will itself be altered by the existence of a further agency relationship. For example, as Alesina and Tabellini (2003) suggest, politicians may be able to blame their own agents for failed policies. More generally, the nested agency relationship makes it more difficult for citizens to determine where responsibility lies.

Secondly, if the agency relationship between uninformed citizens and politicians is conceived as a contract where politicians are rewarded in elections for providing certain policies (Ferejohn, 1986), it is clear that contracts available between a politician and a bureaucrat can be much more sophisticated than that, making the politician's own level of expertise a crucial factor. Finally, there is the crucial issue of who is the politician/principal. In a citizens-politician agency relationship, the assumption that the politician's constituency is the appropriate choice of a principal seems relatively uncontroversial but when the politician/bureaucracy relationship is concerned, institutional details of the political process dictate who the principal really is. For example, in a presidential system, whether the executive and the legislative power are controlled by the same side or not will be crucial in determining the nature of the relationship with the bureaucracy.

While economists have just begun to investigate the politician/bureaucracy issue in detail, there is a significant literature in political science. For many years this has been dominated by the notion of 'administrative state'. According to this perspective, the pendulum of relative power in the politician/bureaucrat relationship swings inevitably towards the bureaucracy. The complexity of modern policy-making makes it impossible for politicians to exercise enough control on the process to say that they enjoy significant policy-making power. There might be disagreement as to whether on balance this is a good thing (neutral experts get to make decisions) or a bad thing (accountability for decision making is lost) but there is a consensus on administrative dominance in policy-making. The main weakness of this approach is that it is difficult to determine whether an administrative state exists if there are no clear criteria for identifying such a state. From an empirical perspective, this takes the particular form of the problem of 'observational equivalence' as described by Weingast and Moran (1983): if we observe that bureaucrats have ample discretion in decision making, how can we know whether this is because the bureaucrats have effective control or because *politicians have deliberately delegated decision making powers to them?* Obviously, the only way to solve the problem is to determine empirically whether changes in politicians' preferences determine changes in policy outcomes. A more recent empirical literature has indeed shown that there is little evidence for the notion of an administrative state where control by politicians is non-existent. Indeed, the perception is that the new task is to determine to what extent politicians have been able to exercise control, not whether they have control at all (e.g. Wood and Waterman, 1991).

In addition to this empirical attack on the validity of the administrative state perspective, a theoretical literature has developed to provide an understanding of the mechanisms through which politicians may be able to control bureaucrats. This literature has a link with the economics literature as it follows from an understanding of the principal-agent nature of the relationship between politicians and bureaucrats. The first consequence of this new understanding has been the acceptance of the fact that specific institutional rules for specific political systems have an impact on the politician/bureaucrat relationship itself. Thus, the study of the relationship in parliamentary systems has diverged from the literature concerning the United States. The main difference between the two approaches has been that in parliamentary systems, scholars have often looked at cabinets not parliaments, as the main target of their analysis. This is because the strict party discipline characteristic of these systems, together with the fact that the parliamentary majority controls the executive, gives parliaments a marginal role in policy making. The focus on cabinets has led to an emphasis on instruments of direct control for cabinet members as opposed to the use of legislation. Further, as Huber and Shipan themselves point out, most scholars seem to have understood the principal-agent nature of the relationship between politicians and bureaucrats but '... most of the research is highly interpretative and descriptive, with little actual development of testable hypotheses and even less systematic empirical investigation'.[1]

The literature concerning the United States, on the other hand, has focused on Congress as the crucial 'principal' while the president's ability to control agencies has led to consider the executive as part of the agency problem that Congress attempts to reduce. The emphasis on Congress has thus led to much more attention to legislation as an instrument of control. It is important to note however, that the focus has been on those legislative provisions that control the policy-making

[1] Page 30.

process rather than the indirect control that specifying *policy details* may provide (McCubbins *et al.*, 1987; 1989).

'Deliberate Discretion?' follows on the political science literature and has the objective to advance at least on three levels. In the first instance, the authors take to heart the importance of the distinction between different institutional systems and aim to provide a theory that is applicable to both parliamentary and presidential systems while allowing for differences in their institutional details. Secondly, the theory focuses on policy prescriptions in legislatures as the main instrument of control. As I've mentioned above, the literature on parliamentary systems does not look at legislation as an instrument of control at all while the literature on the United States has looked at it but focusing on provision that institute control processes, not on policy prescriptions. Finally, the book provides an empirical analysis of the theory from a comparative perspective.

The first chapter of the book is devoted to introducing both the issue and the author's argument while the second chapter provides an informative review of the previous literature. Chapter 3 can be conceived as a methodological defence for the empirical work done in the rest of the book. Here, the author's aim is to show that the argument that policy instructions in legislation really do matter for limiting agency discretion. In particular, the author's argument relies on the premise that there is enough variation in statute structure across political systems even when they address the same policy issue, that policy instructions, which specify outcomes to be achieved, are at least as important as procedural instructions, and that discretion can simply be measured by the length of the legislation itself. The crucial argument here is that the longer the statute is, the more likely it is that discretion to bureaucrats is reduced. The point is, I find, convincing, also because great care is taken in describing the coding process itself. However, I found it confusing that the data-sets used to establish these points are not the data-sets that will be used in the later analysis.

In chapter 4, the model is introduced informally whereas a formal version is in an appendix. In the model, politicians vote a statute, which allows for a certain degree of discretion for the bureaucrat. Given the statute's limitations, bureaucrats decide the policy to implement: if this policy is within the discretion allowed by the statute there is no cost while if it isn't then there is a cost for the bureaucrat. This very basic framework is extended to cases where there is only one politician determining policy (this is the parliamentary case where the preferences of the executive and the parliamentary majority coincide), where one politician has the power to propose a statute and there is a politician who can only veto it (the US case with no conflict between the two chambers) and, finally, where two politicians can both propose a statute (the US case with conflict between the two chambers).[2] In addition, bureaucrats are assumed to be better informed than politicians about the outcomes corresponding to a certain policy choice and that it is also assumed there are exogenously given, non-statutory mechanisms, for the control of bureaucrats. Finally, bureaucrats are given policy preferences which will not

[2] The actual bargaining model used is a simple two-period version of the Baron and Ferejohn (1989) model.

generally coincide with those of the politicians. The results of the model determine equilibrium discretion in a statute as a function of the conflict in preferences between politicians and bureaucrats, of the informational gap between politicians and bureaucrats, of the non-statutory mechanisms for bureaucratic control and of the bargaining environment in which politicians operate (e.g. whether the system is parliamentary or US-style with or without divided control of the legislature). Not surprisingly, high conflict between politician and bureaucrats, the presence of significant non-statutory controls and well-informed politicians tend to choose less discretion while the presence of conflict between different political actors who have some power over status leads to more discretion. In addition, the model also shows that for very ill informed politicians, policy conflict plays a much smaller role than for well informed politicians. In other words, discretion will be high for non-professional legislatures (such as some of those in the US states) regardless of the level of policy conflict but policy conflict becomes a significant issue for more professional legislators (such as cabinet members in a parliamentary system).

The model is simple and generates quite a few testable implications. I must say that I find much of what is going on persuasive and the results obtained highly intuitive. There are however, a couple of issues that I would offer as worthy of mention. The first is that in the empirical analysis the authors take great care to separate policy prescriptions from the procedural rules embedded in legislation and focus on the former but the model's analysis is clearly compatible with either. In other words, the model is too stylised to allow us to distinguish from a theoretical perspective between discretion through clear limits given by policy prescriptions and limits given by any other means of control available to legislators. The consequence is that any empirical evidence that supported the notion that only these other methods of control responded to factors such as policy conflict between politicians and bureaucrats, would have still been compatible with the model. As we shall see, the evidence does support the notion that discretion is also controlled though policy prescriptions in statutes, but then the question is whether the model presented here was necessary at all since the conclusions are straightforward anyway and the model can be meant to suggest many more things than the narrow interpretation the authors provide.

The second point is that this model does not address the first theoretical problem I mentioned at the beginning of this review. In the model, there is no consideration for the politician's constituents role: to what extent is the relationship between politicians and their constituents capable of influencing the politician's willingness to allow bureaucrats discretion? Clearly, this seems an important issue that future research will have to tackle.

In chapter 5 the authors compare their theoretical conclusion with a case study of the evolution of Managed Medical Care legislation in Michigan while in the following two chapters a comprehensive empirical analysis of the model's results for US states (chapter 6) and parliamentary systems (chapter 7). In both cases, the conclusion of the model are broadly confirmed although in at least one case, the relevance of the findings is striking. The analysis shows that the level of professionalism for the politicians involved is crucial for US, whenever professionalism (measured by the salary legislators get) is low, delegation is high regardless of the level of conflict between legislators and bureaucrats. In parliamentary systems, on the other hand, where the cabinet represents the relevant politicians, professionalism is invariably high so that it is the level of conflict in the cabinet that explains much of the variation in statutes. Given the extreme importance given to the issue of divided government in the United States, these findings make the best possible case for the importance of the analysis of the nature of the relationship between politicians and bureaucrats involved in policymaking. I think it is worth pointing out that in order to describe bureaucrats' preferences at the empirical level, the authors assume that these are broadly consistent with those of the politician who has the greatest opportunity to influence their behaviour: the relevant cabinet minister or the state governor. This is a significant departure from the Alesina and Tabellini (2003) model where bureaucrats are assumed to be motivated by career concerns. It would certainly be interesting to see to what extent the career concerns assumption applied to the politician/bureaucrats would fare, both theoretically and empirically. In chapter 8, Huber and Shipan review the results of the previous chapters and draw some possible normative policy implications of these findings.

To conclude, I believe the book makes a significant step forward in our understanding of the relationship between politicians and bureaucrats although I must admit I found the empirical work more interesting for reasons discussed above. Perhaps one can hope in the future for a more systematic attack to the theoretical issues involved coupled with additional empirical analysis at the level of the analysis presented here. Highly recommended.

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The Political Economy of Rule Evasion and Policy Reform. By LEITZEL (JIM). (London and New York: Routledge, 2003. Pp. xvii+201. £50.00 hardback. ISBN 0 415 28272 1.)

The book starts with an interesting anecdote. 'In Bogota, Colombia, cars stopping at red lights late at night became favoured targets for robbery and carjacking. As a result, nocturnal drivers refused to brake for lights… The Bogota city traffic department responded to these development by legalising the behaviour of drivers: between 11 PM and 2 AM, red lights were eliminated, and replaced with flashing yellow signals'. It goes on to argue that the response of the authorities 'can make virtually every participant (except, perhaps, for the thieves at the crossroads) better off than they were under the old, evaded rule'. And the reason is: 'not all drivers are accustomed to the informal ways of driving in Bogota at night. Inexperienced motorists are apt to take green lights at face value… For these less savvy motorists, a flashing yellow light induces more caution than a green signal… Further, the policy reform identifies the precise time period during which the ''no-stopping'' norm is in effect'.

The observation is an insightful one and, together with the rest of the introduction, raises the expectation that the book is about a careful examination of the conditions and circumstances under which similar rule evasion leads to similarly beneficial policy reforms, and of further implications of the basic insight. That expectation unfortunately is something the book does not live up to. At a conceptual level, it never goes beyond saying that 'wide-spread' evasion *often* leads to beneficial policy reforms. As to 'small-scale' evasion, there seems even less concrete to be said: it is argued that such evasion can either 'help solidify the rule, to pose a barrier to reform' (page 5) or 'provoke major policy change' if there is 'sufficient positive feedback in evasion' (page 47). The precise nature and characteristics of positive feedback, however, are not identified and analysed, which renders the discussion largely void of substance.

The rest of the book consists mainly of the author's opinions on issues broadly related to rule evasion and policy reform, which reads more like a collection of disparate newspaper columns than chapters of a book supposedly with a central theme. Apart from noting that it is a form of rule evasion, the discussion on corruption (Chapter 6), for example, adds nothing to clarifying the relationship between rule evasion and policy reform. Instead, a list of positive and negative features of corruption is offered, which leaves one to wonder about the point of discussing the subject at all. The same basically goes for the treatment of zero tolerance (Chapter 3), where another common-sensical list of the pros and cons of zero tolerance is produced, and that of preventive and punitive controls (Chapter 5), where the sensible (but not new) point is made that if ex-post (or punitive) control is feasible and inexpensive, it is in general superior to ex-ante (or preventive) control, but its relevance to whether or how rule evasion leads to policy reform is left unexplained. The final two chapters are supposed to be case studies to which the discussions of the preceeding chapters are applied. They however not only fail to shed any new light but are littered with less than convincing arguments. It has never been made clear, for example, why given the author's arguments elsewhere generally in favour of less regulation, gun control is *conceptually* such an exception. It begs the question of whether gun control is the only exception to a general principle or similar arguments made for gun control can be made for other types of regulation as well.

Thus, it is not clear what the book is meant to be. If it is just meant to be a survey of the literature on all issues related to rule evasion and policy reform with no (new) over-arching thesis, the treatments of these subjects lack breadth and depth and provide no critical evaluation of the vast relevant literature.

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Trust: Forms, Foundations, Functions, Failures and Figures. By Nooteboom (Bart). (Aldershot and Lyme, NH: Edward Elgar, 2002*. Pp. xii+231. £55.00 hardback. ISBN 1 84064 545 8.)

This book provides an interesting and informative account of the nature, causes and consequences of trust. It is perhaps best thought of as an exercise in conceptual ground-clearing, offering as it does a systematic delineation of the various aspects of that multidimensional phenomenon.

Nooteboom begins by arguing that a prerequisite for understanding the nature and significance of trust is a recognition of the fact that we live in a world of radical uncertainty, that is, a world in which people are unable even to imagine all the possible consequences of their actions, let alone assign sharp numerical probabilities of the sort postulated by subjective expected utility theory. For Nooteboom, viewed under its epistemic aspect, trust is the belief that people will respond to unforeseen contingencies by acting in good faith, eschewing the instrumentally rational and opportunistic modes of conduct emphasised by rational choice theory and transactions cost economics in favour of actions which express their commitment to norms of fairness and reciprocity.

The prominent part played in Nooteboom's account of norms by reciprocity and fairness suggests that, viewed (under its ontological aspect) as an object of belief, trust is best understood in *relational* terms, either as a set of micro-level personal relations between specific individuals or as a structure of macro-level impersonal relations between positions in organisations and institutions. The norms of fairness and reciprocity that underpin these relations give rise to obligations and standards of (trustworthy) behaviour the motivational force of which can outweigh the dictates of pure self-interest. And it is an awareness of the impact of such relations on the behaviour of their fellow men which underwrites people's belief in the trustworthiness of others and ensures that their faith is not completely 'blind' (Lawson, 1985).

Nooteboom's emphasis on radical uncertainty, on behaviour which deviates from the pursuit of self-interest (however broadly defined), and on the causal influence exerted on people's actions by the social relations into which they enter, signal his departure from orthodox, game-theoretic models of trust in favour of a sociological approach which explains trust in terms of the causal interplay between social structure and human agency. For Nooteboom, (social-structural) relations of trust are recursively related to people's (trustworthy) actions in the sense that each is both a cause and a consequence of the other (Archer, 1995). People who are embedded in a well-developed network of relations of trust are encouraged to behave in a trustworthy fashion, while their trustworthy behaviour in turn contributes to the (continued) existence of those relations. According to this transformational model of trust, relations of trust are socially constructed by people who are embedded in networks of social structures (Lewis, 2004).

While this reviewer finds the general thrust of Nooteboom's approach to be broadly correct, there remains scope for improvement. Perhaps most notably, Nooteboom does not give an explicit account of the nature of social institutions. This decision appears curious given that so many aspects of his approach – his distinction between the various sources and objects of trust, his attribution of different causal powers to those objects, and his argument that the genesis and breakdown of trust is best explained in terms of the causal interplay between relations of trust and people's actions – presuppose an ontological distinction between social structure (social institutions and rules) and human agency (people, their capacities and their actions).

There are at least two respects in which Nooteboom's reluctance to reflect explicitly on such ontological issues is significant. In the first place, it is not immediately obvious, to this reviewer at least, how Nooteboom reconciles the use of numerical probabilities to express the beliefs of the agents who populate the computational model of trust outlined in chapter 6 with his earlier claim that an acknowledgement of the significance of radical uncertainty is a *sine qua non* for understanding trust. More sustained, explicit reflection about the ontology of the socio-economic world may have alerted Nooteboom to the tension between the view of the socio-economic world as an open system characterised by radical uncertainty which underpins his earlier sociological account of trust and his later use of formal, closed system models which presuppose that agents can assign sharp numerical probabilities to the consequences of their actions (Lawson, 1997). Second, while Nooteboom is clearly aware of the importance of relations of power and domination, his account of the interaction between power and trust is underdeveloped. Explicit theorising about the nature of social structure can help to overcome this problem (Lewis, 2004).

These criticisms notwithstanding, Nooteboom has written an interesting book which has prompted this reviewer to think fruitfully about various aspects of trust. I am confident that the book will provide other readers with similar intellectual stimulation and sustenance.

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Incentive-Based Budgeting Systems in Public Universities. Edited by Priest (Douglas M.), Becker (William E.) and Hossler (Don). (Aldershot and Lyme, NH: Edward Elgar, 2002. Pp. xii+245. £55.00 hardback. ISBN 1 84376 170 X.)

Incentive-based budgeting in higher education comes in many guises; and this edited collection, *Incentive-based Budgeting Systems in Public Universities* (Edward Elgar Press, 2002), does a good job in reviewing the theory, describing case studies, and offering analysis of this multifaceted practice. The book is well-written, sufficiently rigorous, and insightful of how such budgeting systems work in higher education; it describes a sensible alternative to the more common practices of formula funding, activity-based costing, and incremental budgeting.

After a short Introduction by the Editors, the book divides into three Sections with contributions from Economists, administrators, and those who are both. Each Section has interesting Chapters.

The 'Overview' addresses the need for Incentive-Based Budgeting (IBB). IBB is a somewhat vague term (all systems have incentives to do 'something'), but it rests largely on turning university departments, colleges, and faculties into autonomous 'cost centres' and 'decision-making units' with only a light-touch central administration. To Economists, who favour atomistic competition, are wary of inefficiencies in command structures, think information costs rise sharply, and are sceptical of the existence of public goods, the wonder is why IBB is not the *status quo*. John Wilson's excellent Chapter ('The efficiency of responsibility centre management', Chapter 2) robustly reviews this presumption, and rejects it. Because these autonomous centres/units do not control tuition prices but instead receive formula funding from the central administration, they are unlikely to make efficient resource allocation decisions. Because centres/units are not profit-maximising, the logic of the competitive market cannot be applied. Because students are not homogeneous inputs (or outputs), centres/units will have difficulty in responding to students' needs. And because there are many university-wide externalities from a student in a given department, centres/units will not 'price' student behaviour optimally. Wilson's arguments are strong, expressed fully, and would serve as engaging reading for students wishing to apply the theory of the market to real, complex organisations. (It is less formal – and more general – than Winston's (1999) intelligent treatment of financing in US universities). But, it is unclear how alternative budgeting systems would improve on IBB, so as to resolve the difficulties that Wilson catalogues. Moreover, introducing IBB into public universities is clearly practicable, as several case studies attest.

Part II, 'Case Studies', reports on the introduction and use of IBB at three large public universities: the University of Indiana, the University of Toronto, and the University of Michigan. Each case study is interesting, with enough detail but also enough generality about the implementation of IBB (a more loosely defined strategy in practice). These Chapters give useful information about the 'black box' of higher education organisations. Moreover, each study concludes with a reasonably positive evaluation of IBB, notwithstanding the predictable objections and reluctance from administrators and faculty comfortable with historical budgeting. A common conclusion is that IBB can be introduced partially across campuses; although this pragmatic approach may too cause tension (as described in David Kirp and Patrick Roberts' (2002) energetic account of the 'secession' of the Business School from the University of Virginia).

Part III, 'Effects and Lessons', includes a mix of contributions. One Chapter on the relationship between IBB and teaching makes clear what is well-known from similar organisational reforms in schools: instruction and pedagogy are largely unaffected (Bullock and Thomas, 1997). In their contribution ('Reward structures and faculty behaviour under responsibility centre management', Chapter 9), William Becker and Neil Theobald set out a formal model of how academics would behave under a competitive salary-setting scheme. Notwithstanding the null effects on teaching, the impact of IBB on faculty's behaviour as researchers and administrators should be analysed further, and the authors' model provides a useful framework for subsequent research. Another Chapter describes the travails of adopting performance indicators, arguing that such indicators cannot accurately measure the quality of higher education institutions. This argument – that contrived statistical measures fail to identity quality – might predispose some readers toward IBB, where money is the unit of account and the budget is the constraint. In general, though, the contributors to this volume seem very cautious in their support of IBB. However, the final Chapter of the book adopts a more positive tone, advocating at least for the consideration of IBB.

In summary, *Incentive-based Budgeting Systems in Public Universities* serves as an up-to-date, interesting, and useful treatment of funding systems within US universities. The case studies in particular are useful evidence that, even for complex enterprises such as colleges, incentives can be made to count.

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Monopsony in Motion: Imperfect Competition in Labor Markets. By MANNING (ALAN). (Princeton, NJ: Princeton University Press, 2003. Pp. x+401. £29.95 hardback, US $45.00 hardback. ISBN 0 691 11312 2.)

In the 1990s, empirical studies into the effects of policy-induced wage increases such as minimum wages and equal pay showed that such measures had a noticeable effect on the earnings structure but apparently no adverse effect on the employment of the workers concerned. These conclusions, which are clearly at odds with the view that firms are price-takers in the labour market, have led to renewed interest in the possibility of monopsonistic behaviour. Alan Manning's timely book is a self-contained, wide-ranging treatment of some of the key issues in labour economics from a monopsonistic point of view.

Monopsony is often taught as a limiting case but it is seen as having little relevance to modern labour markets since the case of only one buyer of labour is extremely rare. However, the author emphasises that what is important is the idea that employers have power in the labour market, so that monopsonistic competition or oligopsony would be more appropriate terms.

The book is organised in four parts. In the first, the author presents a number of formal models in which employers set wages ranging from the standard static partial equilibrium textbook case of monopsony to dynamic general equilibrium models based on labour market frictions and the search behaviour of workers. He goes on to explore efficiency issues and shows that in a free market the level of employment is not optimal, and this inefficiency can be corrected in the majority (but not all) of cases presented by a judiciously set minimum wage. The first part is concluded by a chapter on what the author refers to as 'the single most important idea' in the book – that the elasticity of labour supply to the firm with respect to wages is not infinite. He concludes that a value of between 2 and 5 would be 'reasonable'.

In the remaining three quarters of the book, he uses the theoretical framework to address the main questions in labour economics from a monopsonistic point of view. The section on the structure of wages contains chapters on employers' wage policies, earnings over the life cycle, gender discrimination and the effect of employer characteristics on earnings. The following section on labour demand and supply, contains three chapters which cover unemployment and labour supply, vacancies and the demand for labour, and general and specific training respectively. The final section consists of a chapter on minimum wages and trade union wage bargaining and a concluding chapter which provides an overall perspective. The content of these chapters is mainly of an empirical nature and the author uses data from the United States and the United Kingdom to test hypotheses and to substantiate the relevance of a monopsonistic approach to the issues.

It is important to stress that this is not a textbook written from a monopsonistic point of view. It is more an attempt to convince labour economists of the relevance and advantages of introducing imperfect competition into labour market analysis – instead of using 'the competitive model with bits bolted onto it when necessary to explain away anomalies' (page 11). On many themes he is keen to emphasise how straightforward it is to provide a rigorous and intuitively appealing explanation of these 'anomalies' if the starting point of the analysis is that employers set wages rather than take them as given by the market. The presentation is formal using a proposition-proof style (with proofs presented in appendices to the chapters) for the theoretical sections and using econometric analysis for the empirical parts.

It is often said that it is difficult to relinquish an internally consistent though faulty theoretical approach until a well-formulated, more relevant alternative is found. Manning claims to have provided this, but will labour economists find the book convincing? On several occasions the author points out that it is unrealistic to imagine that if an employer reduced wages by a small amount, all workers will leave, and thus the supply curve to the firm is not horizontal. However, it is also difficult to believe that if the firm wished to recruit one more worker then it would have to raise wages including those of all existing employees. What does this imply about the firm's labour supply curve? Those unhappy with the orthodox supply and demand framework may not find replacing one formal assumption (the firm is a wage-taker) by another (the firm sets the wage) within the same basic paradigm convincing.

These remarks aside, the book will be useful for graduate labour economics courses and will also serve as an excellent reference for researchers, since Manning has provided a coherent theoretical basis for imperfect competition in the labour market. He has shown how the approach can be usefully applied, and in doing so leaves many empirical questions in the balance thereby raising a number of interesting issues for further research.

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Recent Advances in Environmental Economics. Edited by LIST (JOHN A.) and DE ZEEUW (AART). (Aldershot and Lyme, NH: Edward Elgar, 2002. Pp. xvi+391. £79.95 hardback. ISBN 1 84376 002 9.)

This edited volume is a follow-up publication to papers presented at a joint conference organised by the University of Central Florida's Center for Environmental Policy Analysis (the inauguration of which it celebrates) and Tilburg University's CentER for Economic Research in the winter of 2000. Invitations to the conference were issued with a view to presenting innovative research, with no particular requirements being placed on the topic. Hence what one sees in this volume is 'state of the art', though this has come at the expense of a tight thematic structure.

In fact, the editors have been able to glean two broad topics from the sixteen chapters contributed. The first is environmental federalism. That is, the debate over which level of government should perform what role in environmental regulation. Gates leads us into this topic with a fine review of the economic literature, focusing in particular on the so-called 'race to the bottom', whereby competing local jurisdictions attempt to attract inward business investment by, *inter alia,* setting excessively lax environmental standards. This is one of the few chapters in the volume that can be recommended for undergraduate level students. Chapters 2–5 offer selected further perspectives on the environmental federalism debate, based on theoretical modelling studies. Johal and Ulph introduce transboundary pollution to their established model of supranational environmental governance captured by special interests, while Rauscher models transboundary pollution as an economic decision. Hoel considers the problem of allocating greenhouse gas emissions under a multilateral agreement if populations are assumed to be perfectly homogenous and mobile (which they are not). And Elbers and Withagen address the issue of ecological dumping using a general equilibrium analysis. These chapters will be of interest to those engaged in the theory of environmental federalism, but disappoint insofar as they suggest a future research agenda, rather than defining it.

In chapter 6, Levinson gives a very readable critical 'send-up' of the environmental Kuznets curve (EKC) literature. It is his position that the vast number of EKC articles now published have lost sight of the only proposition it can test: that pollution does not necessarily increase with economic growth. This chapter should also be on undergraduate reading lists. Chapters 7–10 continue the regulatory theme, though in ways less fundamental to environmental federalism. Dawson and Segerson compare the long-run scope for free riding in voluntary regulation to the short-run. Xepapadeas considers the irreversible development of an exhaustible resource, under uncertainty both on the profits from extraction and on the value of conservation. This seminal paper applies advanced concepts of stochastic modelling and is a highlight. Agee and Crocker model the linkage between neighbourhood conditions and a child's home environment. This is in many ways only of tangential interest, but it yields one important insight for research into the effects of local environmental quality on individual well-being: that one must consider the family. Kunce *et al.* extend the empirical analysis of regulatory voracity and industrial activity to the oil and gas industry.

The volume's second substantive theme is environmental valuation, with a particular emphasis on contingent valuation (CV). Various cutting-edge issues are discussed in chapters 11–16. Kask *et al.* develop flexible (i.e. respondent-controlled) scenarios to circumnavigate the various problems posed for CV when respondent perceptions diverge from the information presented. Mansfield and Smith thoughtfully explore the nexus between general and partial equilibrium analyses of the costs and benefits of large policy/programme decisions. Though much attention in the valuation literature, including in this collection of papers, is trained on the accuracy of contingent valuation, this is no less important, and hence is a valuable contribution. Milon and Scrogin evaluate the problems posed for CV by heterogenous preferences and complex environmental goods. Finally, Goeree *et al.*, Bjornstad *et al.* and Schulze *et al.* outline various insights from recent 'laboratory' valuation experiments. Of particular note is the Schulze *et al.* chapter, which provides an excellent review of what is generally termed 'other-regarding behaviour'.

Environmental economics is now a broad church, and this volume reflects the wide range of problems to which it is directed. This was never intended to be a comprehensive, nor I dare say, coherent, thematic collection, and unashamedly offers different interests to different researchers. For those researching at the forefront of environmental federalism, chapters 1–6 offer some original insights. For me, however, it is those chapters on environmental valuation that constitute its greatest contribution, as they are both novel and, more importantly, *fundamental* to the discipline. Indeed, one can perhaps lament the fact that most of the essays, despite pioneering the discipline, will not permeate discussions far outside of specialist research groups.

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Multinational Firms and the Theory of International Trade. By Markusen (James R.) – (Cambridge, Mass, and London: MIT Press, 2002. Pp. xxii+440. £29.95 hardback. ISBN 0 262 13416 0.)

This volume is to a large extent a summary of Markusen's contribution to the study of the multinational firm over the past 20 years, tracing the introduction of game theory and modern trade theory to the analysis of FDI and the multinational enterprise. Work of this type by Markusen, Venables and others has not only added significantly to our understanding of the multinational enterprise, but has also informed much of the applied work and policy analysis concerned with the location of economic activity.

The volume provides interesting insight into how models of the multinational enterprise and of strategic trade policy developed simultaneously, largely independent of firm-based transaction cost models of FDI in industrial economics. The latter approach is discussed later in the book, and therefore this is an ideal volume for graduate students in both industrial and international economics studying the analysis of the multinational enterprise.

The early chapters are devoted to a discussion of the models of firm location based on new trade theory. The partial and general equilibrium models are clearly presented, while the author is very honest in outlining the limitations of the analysis. A counterfactual analysis is introduced towards the end of the initial chapters. This seems to be something of an afterthought, which is rather disappointing, as a fuller discussion of the counterfactual to FDI would have been of interest from a policy perspective as well as making a significant contribution to much of the current applied work in this area.

The volume then moves on to the numerical GE model, and the knowledge-capital model, based on Markusen and Venables (2000) which will be familiar to many readers. The model presented here is slightly simplified from the published paper, but generates similar results. This simplified model does however generate one counter-intuitive result, failing to predict that growth generates increased FDI. This would be a surprise to people doing applied work, who often find that market size is the most important determinant of FDI flows. The subsequent chapters however go some way to resolving this issue and present a detailed analysis of the knowledge-capital model. This demonstrates the importance of firm specific assets in the motivation for FDI, a phenomenon that is at the core of industrial economics analysis of FDI, but often ignored by trade theorists. The model also generates an intuitively appealing result which mirrors recent applied work. Inward FDI increases the return to skilled labour in the host country, even in the absence of spillovers, thus increasing inequality. The mechanism by which this occurs is well defined, and should inform much of the current applied work in this area.

The volume then moves on to testing the knowledge capital model against alternatives, and presents some estimation. The results that are presented are relatively standard and limited by a common problem in this area, the large degree of aggregation in sectoral level FDI data. The econometric results are a clear exposition of the model, but in terms of current econometric work on the determinants of FDI flows, this section looks perhaps rather dated.

The final section presents analysis of the problems of contract enforcement in the multinational enterprise, leading to internalisation of assets and thus encouraging FDI rather than licensing for example. This section is a clear synopsis of the theoretical treatments of agency problems within multinational-local firm relationships, but the scope could possibly have been widened. The models presented touch on the possibility of foreign technology being transferred to the local sector, largely as a result of contract imperfections. However, recent applied work shows that technology spillovers from FDI can occur for a wide range of reasons, and in many cases are encouraged by the MNE in the form of technology transfer or quality support for local producers. The idea that technology transfer is encouraged by MNEs is largely ignored by Markusen, as it is by virtually all theoretical work in this area.

There is perhaps one surprising omission in this text. There is a well developed theoretical literature demonstrating the importance of 'technology sourcing' as a motivation for FDI. This occurs where multinationals enter a particular location in order to access local technology. A discussion of technology sourcing would have been an appealing addition to the theoretical analysis, and would have linked Markusen's interesting journey through the development of the analysis of FDI to an important current literature.

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Markusen, J. R. and Venables, A. J. (2000). 'The theory of endowment, intra-industry and multi-national trade', *Journal of International Economics*, vol. 52, pp. 209–34.

Dollarization: Debates and Policy Alternatives. Edited by YEYATI (EDUARDO LEVY) and STURZENEGGER (FEDERICO). (Cambridge, Mass. and London: MIT Press, 2003. Pp. ix+341. £29.95 hardback. ISBN 0 262 12250 2.)

The choice of exchange rate regimes has been a subject of debate for a long time. Across the globe countries have experimented with fixed, floating, and a wide variety of pegged exchange rate regimes. The vast empirical evidence on this matter appears to show that there is no single recipe to suit everyone. Hence it is no surprise that the rise of dollarisation has generated so much controversy. The book *Dollarization: Debates and Policy Alternatives* edited by Eduardo Levy Yeyati and Federico Sturzennegger, is a welcome attempt to organise our understanding of the topic. It consists of a collection of papers that blends analytical, empirical and institutional analyses in tackling this important policy issue.

Levy Yeyati and Sturzennegger divide the book into eight Chapters, starting with a useful roadmap in Chapter 1. Chapter 2, by Roberto Chang and Andrés Velasco, provides an analytical framework to elucidate the basic mechanics of dollarisation. Pablo Andrés Neumeyer and Juan Pablo Nicolini engage one of the most debated issues in the context of dollarisation, i.e. the link between sovereign default and devaluation risk, in Chapter 3. Christian Broda and Eduardo Levy Yeyati examine a similarly critical topic, the implications of dollarisation for the central bank's lender of last resort function, in Chapter 4.

More empirical and institutional problems are dealt with in the rest of the book. In Chapter 5 Ugo Panizza, Ernesto Stein, and Ernesto Talvi gather some illuminating evidence to help assess the likely costs and benefits of dollarisation. The authors place particular emphasis on Central American and Caribbean countries, namely Belize, Costa Rica, the Dominican Republic, El Salvador, Guatemala, Honduras, and Nicaragua. A key conclusion arising from this chapter is that countries with a high liability dollarisation pay a lower price for the loss of their monetary independence. Another pressing matter in the context of dollarisation, namely the link between devaluation and default risk, comes to the limelight in Andrew Powell and Federico Sturzenegger's contribution in Chapter 6. The authors employ an event study methodology to explore this relationship, providing empirical evidence for European and Latin America economies.

The last two Chapters address operational and institutional factors arising from the dollarisation debate. Once a government decides to dollarise, how does it implement the policy? To aid in the successful achievement of this complex task Chapter 7, written by William C. Gruben, Mark A. Wynne, and Carlos E. J. M. Zarazaga, provides useful guidelines to take into account in the process of adopting dollarisation and forming currency unions. Finally, Chapter 8, by Jeffry A. Frieden, tackles political economy aspects of dollarisation.

Overall, *Dollarization* is an engaging book. Unfortunately, there is no space here for a detailed review of all the material contained in the book, however interesting it might be. The reader should bear in mind that the nature of dollarisation and our knowledge of how it works is such that what can be learned from a book like the one under review will be constantly challenged.

Consider, for example, the impact of dollarisation on financial stability. Many commentators argue in favour of dollarisation as a way to improving this aspect of the economy. However, De Nicoló *et al.* (2003) generate empirical evidence using data from over a hundred countries, showing that financial instability is actually *higher* in economies that have a substantially dollarised banking system. Consequently, even if, for instance, the conclusion in Chapter 5 that countries with a high liability dollarization face a lower opportunity cost as a result of giving away their monetary independence has some validity, it might be the case that countries would be worst-off, *ex-post*, by dollarising.

So the evidence on the benefits that could potentially be derived from dollarising is at best mixed, and therefore far from being unambiguously useful to inform such a momentous policy decision. What can policy makers derive from this conundrum? Dollarisation is an extreme policy choice, and one that the profession still does not understand as much as it would like. Countries that plan to dollarise should carefully study the potential costs and benefits to their economy: dollarisation is not an over-the-counter medication. This book answers several important questions on dollarisation, but many more have to be addressed.

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Reference

De Nicoló, G., Honohan, P. and Ize, A. (2003). 'Dollarization of the banking system: good or bad?' IMF Working Paper 146, Washington, DC, July.

Saving, Investment, and Growth in India. By ATHUKORALA (PREMA-CHANDRA) and SEN (KENAL). (Oxford: Oxford University Press, 2002. Pp. xiii+181. ISBN 0 19565874.)

This short book is a well-documented and interesting account of the trends and determinants of savings, investment and growth in India between 1955 and 1996. Given the critical role that savings and investment play in the process of economic growth, the authors closely examine the saving-investment nexus and the interaction between investment and growth in the context of the new endogenous growth theory [EGT]. In the light of the emergence of the 'new' view of growth, it is now acknowledged that economic policies [e.g. public, financial, trade] matter in influencing long run economic growth. And thanks to the EGT, economic growth is no longer regarded as the 'manna' from heaven which could happen on the crest of an accident like technical progress. In this book, the authors have selected the Scott model as the most appropriate vehicle to test the validity of the EGT in economic development of India for the past four decades. Since the authors have used the long-run time-series data, they have rightly chosen the 'Hendry method', i.e. the 'general to specific' methodology [GSM] as the cross-country regression analysis is based on the restrictive and dubious assumption of 'homogeneity' in the observed relationships across countries. The GSM allow the researchers to avoid the problems of spurious regressions which are frequently encountered in time-series analysis.

The book comprises eight chapters and four useful appendices. In chapter two, the authors discuss the debate on the role of savings and investment in economic growth and indicate the renewed emphasis on investment in the context of the EGT. Chapter three describes the shifts in Indian economic policies in the past forty plus years – from capital accumulation, import-substitution industrialisation and financial 'repression' to policy reforms aimed at achieving 'efficiency' in resource use through market oriented reforms of trade and financial policies. Chapter four depicts the rather impressive trends of savings and investment in India – particularly in the context of other developing countries of the world. In chapter five, the authors use the 'elegant' life-cycle model of Modigliani and conclude that savings in India are positively affected by:

(a) growth and the level of *per capita* income;
(b) real interest rate and the spread of banking facilities; and
(c) rather counter-intuitively, mild inflation.

Fiscal policy also plays a role in increasing total savings with the public sector saving being an imperfect substitute for private saving, thus invalidating the Ricardian equivalence hypothesis. Remittances and terms of trade changes exert a negative effect on savings.

In chapter six, the authors explore the determinants of business investment in India with a neo-classical model of business fixed investment that includes investment in equipment and structures both by the private corporate sector and by unincorporated agents in the household sector. Given the data constraints, the difference between total private fixed investment and household investment in structures is used as a proxy for non-residential business fixed investment [NRBI]. Although the rental-cost of capital is found to be an important determinant of private corporate investment, it is not the case for NRBI. However, bank credit and public investment are observed to be significant determinants of both corporate and NRBI.

Clearly economic policies played their role in helping or hindering long-run Indian economic growth. The authors also observe a remarkable rise in NRBI following the market-oriented structural adjustment reforms initiated in 1991 in India. They conclude that the net effects of economic reforms on investment must have been positive as the negative effects of the fiscal-squeeze following the structural reforms are out-weighed by the positive effects of the fall in real rental cost of capital.

In chapter seven, the authors examine the impact of investment on growth within the new EGT developed by Maurice Scott since the empirical specification of the Scott model allows the role of economic policy in determining the inter-relationships between economic growth and investment. The writers also try to explain the differential effect of investment in machinery compared to investment in construction.

The results support the hypotheses that: a) corporate investment is negatively and significantly influenced by the size of the initial capital stock and the cost of borrowing; b) the accelerator model is important in explaining corporate investment behaviour. Also, a prudent and stable macroeconomic policy [i.e. low and stable inflation rate and restoration of the stability in the fiscal management after 1991] coupled with the policies of financial and trade liberalisation contributed substantially to the acceleration of the Indian economic growth in the post reform period. Perhaps the authors could have added more recent available data to reach more definitive conclusions regarding the success or failure of the reform programmes.

There are a few limitations. For example, a joint positive effect of both inflation and the end of financial 'repression' (which implies a positive real interest rate and a decline in the rate of inflation when the nominal rates are mostly administered in India during the period of investigation) on total saving is hard to understand when the authors observe a positive and significant real interest rate coefficient. I am a bit surprised at the inclusion of both inflation and the real interest rate [i.e. nominal interest rate - the rate of inflation] as the explanatory variables.

Further, besides the ratio of broad money to income, the authors could have used other proxies to test the degree of financial deepening. The authors make a passing reference to the Feldstein- Horioka puzzle about a close correlation between domestic savings and investment in India. But they could have tested the 'puzzle' more rigorously in the context of a more liberalised open economy. The slow flow of foreign direct investment to India during the last fifty years in plugging the gap between investment and saving is an important area of research. A more detailed analysis of the 'crowding-out' hypothesis in India is also missing. Despite these criticisms, 1 should be inclined to feel that the authors of this book have investigated an important area of research which merit attention from the teachers and students of economic growth and development. It should also be useful to policy makers in India and other developing countries.

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Transatlantic Sport: The Comparative Economics of North American and European Sports. Edited by PESTANA BARROS (CARLOS), IBRAHIMO (MURADALI) and SZYMANSKI (STEFAN). (Aldershot and Lyme, NH: Edward Elgar, 2002. Pp. xi+222. £55.00 hardback. ISBN 1 84064 947 X.)

This book collects nine contributions on the economics of sport which attempt to highlight both the similarities and differences in North American and European models of sport as well as their implications for a number of economic, policy and social issues. The papers come from a conference held in Lisbon in November 2000 that brought together a group of North American and European researchers (mostly economists) who share an interest in the comparative economics of sports.

As the editors indicate in their opening sentences, for most people economics and sports may appear to be uneasy companions, at least at first glance. Yet, it should take a good social scientist not too long to realise that sports settings offer many unique opportunities for theoretical and empirical study of a number of important matters in economics and other social sciences. Matters that would be difficult, if not impossible, to study in other settings. Also, sports occupy an important place in the lives of most people and represent a very large share of the time that is devoted to leisure in modern societies.

Thus, the theme of the book is of genuine interest to a broad audience. Moreover, there is a very stark contrast between the United States and European models that contributes to increasing the basic interest. In the United States the organisation of sports is such that: (i) teams are organised into hermetically sealed leagues so that entry at the level of an individual team in not possible without consent, (ii) restraints are extensive in the player labour market (e.g., draft rules, roster limits, salary caps, etc) and player contracts are negotiated on the basis of collective bargaining, (iii) on the product market, teams have entered into collective agreements that restrict the scope of economic competition (e.g., revenue sharing, joint merchandising, collective sale of broadcasting rights, etc). These and other restrictions clearly would not be admissible if competition between teams in the sports leagues was treated by the US government in the same way as competition among firms in other industries.

From an economics perspective, this antitrust leniency in the United States is difficult to explain. For one, there is a puzzling lack of evidence supporting the ideas behind these practices. Namely, there has been little analysis of the idea that sporting contests are not interesting unless their outcome is entirely uncertain, that uncertainty is strictly increasing in the equality of the distribution of economic resources, and that this equality can be best achieved by limiting the role of economic power in hiring players and by equalising this power through income redistribution. Although these concepts appear intuitive at first, they are far from being supported theoretically and/or empirically.

By and large, the European model of sport is entirely different. Leagues engage in far fewer and much softer restrictive practices both on the labour and product markets. Also, the distinct system of promotion and relegation means that leagues are not hermetically closed and that the set of incentives is very different.

A thorough introduction by the editors offers an excellent survey of the existing literature in the large range of potential topics, issues, and questions that deal with these and other differences. It also includes an extensive list of valuable references for anyone interested in the rich set of opportunities that sports settings have already offered in the last few decades to many researchers.

The nine chapters in the book are then divided into three areas: public policy, economic theory, and cost-benefit analysis. In the public policy area, the three contributions are concerned with the present and future of regulation of team sports in Europe and with the dichotomy between the US profit-maximising model and the European 'utility' maximising model largely based on an amateur tradition. In the economic theory area, a theoretical chapter considers mechanisms to maintain competitive balance while an empirical chapter is concerned with the interesting issue of how to measure competitive balance in a dynamic sense. Lastly, four cost-benefit analyses are, somewhat surprisingly, concerned with themes that have little to do with similarities and differences in the United States and European models of sport: the (negligible) economic impact of the Los Angeles and Atlanta Olympic games, the economic and non-economic benefits of major sporting events to society, the allocation of World Cup matches to cities, and the value of contributions made by voluntary sports managers.

The chapters differ not only in style and methodology, but also in quality. However, they are similar in that they are perfectly accessible to a broad audience of those interested in economics, sports, and the economics of sports. Yet, this broad accessibility is not without a cost. Many scholars will find most of the chapters excessively descriptive.

While the book succeeds at being interesting and accessible, it fails in a more important direction. *Transatlantic Sport* seems to be exclusively directed to sports economists and little effort is made to attract scholars and researchers in economics, law, public policy, and other social sciences. In this sense, I doubt that economists and social scientists with little interest in sports will be attracted to sports settings because of this book. However, the unusual wealth and access to data and the great variety in market and non-market settings that may be suitable to study important aspects of the economic approach to human behaviour should be attractive to virtually all social scientists.

In recent years, an important strand of economic literature has used sport data to test economic theories that involve, for instance, equilibrium concepts in game theory (Chiappori *et al.*, 2002; Palacios-Huerta, 2003; Walker and Wooders, 2001), corruption (Duggan and Levitt, 2002), discrimination (Szymanski, 2000), endogenous preferences (Garicano *et al.*, 2001), decision-making anomalies (Romer, 2002), and others. What is entirely distinctive from these and other recent papers is that they show how sports settings do offer unique opportunities to advance our state of knowledge in economics and other social sciences in important directions. I felt that the lack of motivation in this direction was the biggest shortcoming of the book. In this sense, one should perhaps take this book as an invitation to continue to realise the promise that the analysis of sports settings and data offer for the social sciences in general and for economics in particular.

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Advanced Macroeconomics. By MINFORD (PATRICK) and PEEL (DAVID). (Aldershot and Lyme, NH: Edward Elgar, 2002. Pp. xii+548. £75.00 hardback. ISBN 1 84064 090 1.)

Minford and Peel are experienced practitioners in macroeconomics and this text book draws on their familiarity with the subject. Sub-titled ''a primer'', the book aims at the post-graduate or third year undergraduate student to give them the grounding needed to go on to read the technical papers found in journals.

The book starts with a 'succinct introduction' to macroeconomics for those who have not studied the subject before, although the going would be quite tough for anyone who was really unfamiliar with the subject. The analysis here and throughout the book is helped by some excellent 'boxes' which carefully go through some of the more tedious algebraic steps and will be a boon to lecturers who do not wish to waste valuable office hours showing how to rearrange equations to the mathematically under-confident. Chapter 2 provides a quite dense mathematical derivation of models and compares different techniques for their solution and shows the 'primer' ideal at its best. The final chapter in the first part of the book looks at signal extraction, the Lucas islands model and variations thereof.

The following ten chapters then consider various applications of the rational expectations approach presented in the first three chapters. The analysis in these chapters varies between quite careful derivation of mathematical results and more verbal exposition with some gaps in the formal presentation. The choice of which models to discuss in depth and which to sketch is idiosyncratic, but in my view the balance here is about right in terms of meeting students' needs. The important topics of policy ineffectiveness, stabilisation effectiveness, Ricardian equivalence, political economy, and the open economy are covered in some detail. The discussion is often insightful: for example the continual theme that institutional arrangements in New Keynesian models such as contract length are themselves a product of the underlying behaviour of the economy and need to be considered as such.

The final part of the book is entitled 'Confronting models with Facts'. This is perhaps the weakest part of the book in terms of meeting its own objectives and empirical results are reported very briefly throughout these chapters. There is certainly little attempt to engage students with tables of econometric results, although I realise that for some lecturers this will be a strength, since students are usually wary of looking at statistical analyses. Also slightly disappointing is the very quick dismissal of survey evidence on expectations, especially as modern economics and finance are moving towards more empirical approaches to microeconomic behaviour.

The result is a very solid introduction at a relatively advanced level to the macroeconomics of business cycles, which is clearly set at the appropriate level for the target audience. As such, the title 'Advanced macroeconomics' is not entirely correct, since many topics which would normally be considered to be macroeconomics are clearly missing: there is virtually no discussion of the components of aggregate demand, the peculiar problems of labour markets and unemployment are ignored and long run considerations of growth and productivity are poorly covered in the chapter devoted to the topic. Certainly David Romer's (2001) book of the same title has much broader content.

Within the area of business cycles, the book is much stronger and would clearly merit use on graduate courses, chapter 2 being a good example of showing mathematical methods clearly. The political slant of the book is unsurprisingly New Classical – important papers such as Ball, Mankiw and Romer (1988) are not mentioned and a perusal of the references shows a general dearth of econometric and theoretical work by New Keynesian authors.

In summary, this is a good book, written at the appropriate level, which clearly represents the authors' own interests in a certain area of macroeconomics. It will be a useful addition to the reading list of any Masters course in macroeconomics.

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Exchange Rate Regimes: Choices & Consequences. By Ghosh (Atish R.), Gulde (Anne-Marie) and Wolf (Holger C.). (Cambridge, Mass. and London: MIT Press, 2002. Pp. 232. £21.95 hardback. ISBN 0 262 07240 8.)

Does the choice of the exchange rate regime matter? This book aims to answer this pressing question by assessing the impact of the nominal exchange rate regime on real exchange rate volatility, inflation, output growth and volatility, the success of disinflation programmes, and the incidence and costs of currency and banking crises.

The book begins with an overview of the evolution of exchange rate regimes and the international monetary system and continues with the presentation of a simple model of the main trade-offs in the choice of regime. The empirical analysis – and the core of the book – begins with Chapter 4, which discusses the classification of exchange rate regimes. In this chapter, the authors adopt the standard and widely-used IMF official classification as the default taxonomy of regimes; they also create an alternative 'consensus' classification, to be employed for robustness checks. Chapter 5 presents some general stylised facts about macroeconomic performance under different regimes, concluding that real exchange rate variability is not greater under floating regimes than under fixed regimes for developing countries at longer horizons, that countries with floating regimes seem to exhibit higher inflation, and that there appears to be no major cross-regime differences in output performance.

Chapter 6 goes beyond stylised facts by undertaking extensive regression analysis on the impact of exchange rate regimes on inflation and growth. The authors find compelling evidence that inflation is lower under pegged regimes. In contrast, the link between regimes and output growth is significantly more tenuous; the only safe conclusion is that growth under pegged regimes is not lower than under floating regimes. On the other hand, pegged regimes appear to result in greater output volatility.

Chapter 7 deals with the role of the exchange rate regime in stabilisation programmes. It finds that stabilisation attempts based on pegged exchange rates are no more prone to failure than those undertaken under floating regimes. These results are complemented with the study of stabilisations in Bulgaria, Turkey, and Argentina. Finally, Chapter 8 focuses on the impact of the exchange rate regime on the probability and costs of crises. It concludes that, while currency crises are more likely under floating regimes, their impact is more severe under fixed and intermediate regimes. In terms of banking crises, pegged regimes actually fare better than floats.

In general, the authors do an excellent job presenting simple but rigorous empirical analysis of the aforementioned issues. In my view, however, the book has an important shortcoming. For all practical purposes, the authors base most of their results on the IMF regime classification – and thus the validity of their results generally hinges on the accuracy of this classification. While this is the standard taxonomy widely used in the empirical literature on exchange rates, its accuracy has recently received considerable criticism. This criticism has resulted in the creation of alternative classifications, such as those developed by Levy-Yeyati and Sturzenegger (2002) and Reinhart and Rogoff (2003).

Given that Reinhart and Rogoff (2003) even argue that the IMF classification is only a little better than random, one may fear that the results in the book might be sensitive to alternative regime classifications. It is unfortunate that Ghosh, Gulde, and Wolf did not pursue further robustness checks with the two newer classifications mentioned above. Note that the book's 'consensus' classification does not suffice for this purpose. It is based on the intersection between the IMF classification and a *de facto* classification. The latter is developed by computing a 'z score' from actual changes of the exchange rate, ranking countries according to this z score, and mapping them into pegged, intermediate, and floating regimes with the same proportional distribution as the IMF classification for a given year. However, if the accuracy of the IMF classification is questionable, then its proportional distribution for any given year may also be less than fully precise, which would make the authors' *de facto* classification and thus their consensus classification problematic. The authors would have reassured skeptics if they had also used alternative regime data developed by other researchers.

Albeit minor, another limitation of the book is the choice of topics. We all care about the impact of regimes on inflation, growth, stabilisation, and crises. But the current debate centres more prominently on other issues – particularly, the impact of regimes on the currency denomination of banks' and firms' assets and liabilities, and on currency risk hedging. The data requirements for these additional endeavours are formidable, though.

These limitations notwithstanding, Ghosh, Gulde, and Wolf make an important and rigorous contribution towards a better understanding of the effects of exchange rate regimes. This fine book, as well as its accompanying database on CD-ROM, should serve as a valuable reference for researchers and policymakers alike.

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International Trade, Growth, and Development. By BARDHAN (PRANAB). (Oxford and Cambridge, MA: Blackwell, 2003. Pp. x+285. £55.00 hardback, £19.99 paperback. ISBN 1 40510140 7, 1 40510141 5.)

When reviewing a book that is a retrospective collection of works, two questions spring to mind. First, does the collected work of the author warrant such a selection to be published and second, do the papers chosen have something to say for current researchers in the field? In both cases this reader can answer very

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strongly in the affirmative. Pranab Bardhan has provided major contributions in a number of areas within economic development such as international economics, growth theory, poverty and income distribution, and agriculture. Within that he has straddled both micro and macro economics, in theory and in application. The volume here represents a high quality cross-section of his contributions, as evidenced by the endorsements of Robert Solow and Tim Besley on the back cover.

The book has a three-part structure. Part I is on Trade and Development, Part II on Vintage Capital Growth Models and Part III focuses on Factor Markets and Rural Development. For each, the author has selected six papers that provide an indication of the nature, scope and quality of his work in the stated areas. In addition, a pithy yet insightful introduction not only sets the context for the choice but also demonstrates their value to current researchers and ongoing debates.

Part I highlights the difficulty poor countries face in making trading and production choices when faced with technology transfer from richer countries. Imperfect information and moral hazard can lead to equilibria that differ from the predictions of standard micro-trade theory and are exacerbated when local capital-labour ratios differ greatly from world market ratios. Further, the ideal mode of technology transfer is not always obvious, especially when there is imperfect competition in the supply of technology. In all these areas, Bardhan provides neat, simple yet pertinent models to show his thinking. Many of the themes have yet to be explored empirically, and this collection could stimulate young researchers.

The second Part develops from the first in turning attention to growth and uses vintage capital growth models to show how the endogenously determined life of capital is an important factor in a country's productivity and helps to explain international differences in productivity. In a series of six theoretical papers, Bardhan argues that while abstraction and simplification can be taken too far, modelling with clear assumptions provides a significant understanding of capital choice decisions in developing countries and also of the inter-relationships with wage rates. The latter point is germane to the current debate over re-location of richer country manufacturing in developing countries that are perceived to be low wage economies. How this affects capital replacement and accumulation and hence economic growth is a crucial question and links directly with the choices outlined in Part I of the book.

Part III broadly focuses on labour market structures in India based on detailed and extensive survey work. While the specific detail is in itself highly interesting, the ability of Bardhan to offer a clear insight into the economic rationalisation of what appear to be socially driven relationships is excellent. As economists grapple with the effects of globalisation on labour markets, particularly in countries that become more open under trade policy reform, such micro-detail is essential to predicting future market outcomes. Indeed, this is indicative of a running theme within the book of exploring a number of related yet distinct development-based issues using rigorous economic analysis.

The appeal of the papers in the book has two facets. The first is in the written style, which is clear, jargon-free and succinct. The second is the manner in which the content, be it theoretical or applied, is given a clear context within the academic and policy-oriented debate. It is this latter point that makes what is a high quality collection of works of value to those studying the field today and is a tribute to the years of work the author has given to the area.

The book would be of interest to researchers in the field of both theoretical and applied development economics and also to graduate students wishing to understand the theoretical underpinnings of many of the issues that still face developing countries today.

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50 Years a Keynesian and Other Essays. By Harcourt (G. C.) (Basingstoke and London: Palgrave, 2001. Pp. xii+364. £47.50 hardback. ISBN 0 333 94633 2.)
Selected Essays on Economic Policy. By Harcourt (G. C.). (Basingstoke and London: Palgrave, 2001. Pp. xvi+354 £50.00 hardback. ISBN 0 333 94632 4.)

These two volumes are selections from the publications of a unique figure in modern economics: Geoff Harcourt. The first selection are essays written in the 1990s, reflecting on the discipline of economics and analysing the ideas of key figures (with many of whom he has been personally associated), as a follow-up to a previous volume, Harcourt (1995). The second is a collection of essays from the 1970s onwards on policy-related issues. The two are intimately connected in that policy implications are drawn from theoretical discussion in the first volume, while specific policy proposals are related to a wide range of theoretical literature in the second.

This connectivity follows directly from Geoff Harcourt's own methodological approach, which he discusses explicitly in the first volume. He sees theorising as being motivated by the economist's own goals and conditioned by the economist's own view of the world, such that it is important to understand an economist's values in order to understand his economics. He explains his own motivation therefore in terms of the promotion of social justice. By the same token, and in line with modern thinking about how to construct the history of economics, Harcourt explores the context and motivation of the many important figures whose work he analyses in this volume, including Marx, Keynes and his interpreters, Joan Robinson, Austin Robinson, Shackle, Tarshis, Kaldor and Steindl. Like Keynes (1972), Harcourt uses the vehicle of intellectual biographies as a mechanism to explore ideas more widely. He demonstrates the modern relevance of their ideas, and following his writing over the years we see how he adapts his discussion of these ideas to the changing circumstances. This was made explicit in the major two-volume project Harcourt undertook with Peter Riach to adapt Keynes's ideas to the modern intellectual and economic environment (Harcourt and Riach, 1997).

Several themes recur through both volumes. They stem from Harcourt's early interest in accounting issues, pricing and capital theory, as well as his involvement in theoretical and policy debates in Cambridge and Australia in particular. Two related themes are the preference for analysing aggregates as the outcome of divergent trends (rather than a scaling-up of the actions of a single representative agent), and the understanding of the concept of equilibrium among aggregates in terms of a steady state (rather than the outcome of optimising behaviour where all expectations are met). Harcourt applies these themes to his explanation of why the Phillips curve should not be thought of as Keynesian. He argues forcefully that there is no basis for a stable long-period relationship between unemployment and the rate of change in the wage (or price) level. The cure for inflation therefore lies, not in demand management but in incomes policy. Contrary to the current prevailing view that flexible labour markets are to be encouraged so that wages vary with productivity at a disaggregated level, Harcourt argues that wages should be set nationally in line with average productivity changes and inflation, so that there would be even more of an impetus for activity to shift from declining industries to expanding ones. Incomes policy, Harcourt argues, is efficient because capital and labour are complements. Similarly, Harcourt had, independently of Tobin, argued for a generalised system of taxation designed to moderate the damaging effects of flexible international capital markets. These policies are to be pursued along with demand management (although the emphasis on government spending should be more on the long-term needs of the economy, including income redistribution, than on pump-priming) and tax incentives to encourage investment. He sees changes in tax structure as just one aspect of a focus on designing institutional arrangements to encourage a full-employment-inducing level of investment. Further, in the Keynesian tradition, it is investment which generates saving, rather than the other way round. Harcourt is clearly aware of the problems preventing any 'perfect' implementation of such policies, but argues nevertheless in favour of at least trying to nudge economies in the direction of better serving social justice. This non-purist attitude typifies Harcourt's use of the Keynesian approach to theory, which recognises a conflict between precision and relevance.

The volumes make for reading which is both entertaining and thought-provoking. The volumes exemplify the prolific writing of an economist who is committed to pursuing the goal of social justice, who has had privileged access to many of the key economic figures of twentieth century economics, who has had a distinguished career with work ranging from history to methodology to theory to policy (making clear the connections between all of them), and who has continued to engage with current issues in all these areas. Geoff Harcourt explicitly and controversially seeks common ground not only between heterodox and orthodox economists, but also among different subsets of heterodoxy. The tone of the volumes reflects a personal and intellectual generosity of spirit of someone who looks for connections between different individuals and different strands of thought rather than divisions. All economists, whether already familiar with the work of Geoff Harcourt and the key figures he discusses or not, would profit from reading these engaging volumes.

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