

Book Reviews

Economics and Language. Five Essays. By ARIEL RUBINSTEIN. Cambridge University Press, Cambridge. 2000. 128 pp. £26.95; paperback £9.95.

Natural language—its structure, its evolution and the way it affects human preferences and human interaction—is the topic of this book, which stems from the Churchill Lectures delivered by the author at Cambridge in 1996. The first five chapters elaborate on the topics covered by the lectures, while the remaining three chapters includes comments on the material presented by a logician, Johan van Benthem, and two economists, Tilman Borgers and Barton Lipman.

The lack of economic analysis of the natural language that characterizes human economic behaviour is certainly a large and visible hole. The most primary of economic activities, trade, cannot be carried out without language. This observation goes back to Adam Smith, as Rubinstein mentions. It is also implicit in the photograph of the author's father buying bread in a Jerusalem street that appears on the volume's cover. The book is an important first step in remedying this omission. As with every first step, it is the author's privilege not to provide a comprehensive analysis of the economic role of language but rather to analyse a number of questions that are of interest to the author and, of course, the reader.

The first three chapters bring economic criteria and game-theoretic tools to the analysis of the structure and evolution of natural language. The fourth chapter analyses the constraints on preferences imposed by the structure of the language used by an individual to verbalize his own decisions. The fifth and final chapter discusses the author's critical view of the language of game theory and the misperceptions that such language may have generated.

The linguistics literature that analyses the structure and origins of natural language is vast. The author's contribution in the first three chapters of the book—grouped under the rubric 'economics of language'—consists in weaving efficiency criteria and game-theoretic tools that economists are well accustomed to into the general endeavour to understand more and more aspects of the structure and origins of natural language.

The first chapter asks why linear orderings are so common in natural language. The answer suggested is that linear orderings perform best according to three inherently economic criteria. Linear orderings are the most efficient tool to indicate unambiguously every element of a general set. This criterion is labelled by the author *indication friendliness*. Linear orderings are also the tools that allow a speaker to describe a (binary) relation among the elements of a general set in the most accurate way. This second criterion is labelled *informativeness*. Finally, linear orderings are the binary relations that can be described by means of the least number of examples. This third and final criterion is labelled *describability*. In other words, taking these criteria as the benchmark for efficiency, if an imaginary planner were asked to design the most efficient natural language, she would choose linear orderings as its key ingredients.

The second chapter asks how a statement in the natural language comes to have a given meaning. The explanation put forth is evolutionary. However, the mere pressure of evolution to select a language that is stable, in the sense that it cannot be altered by a possibly small mutation in the interpretation of a given word, is not enough to render a given statement meaningful. For this to be the case, the standard forces of evolution need to be paired with an additional evolutionary force—one that favours simplicity. In particular, it is key that evolution has a, possibly lexicographic, tendency to select strategies for the sender and the receiver of a given message that do not specify complex reactions to signals that are never sent in equilibrium. The chapter concludes by arguing that, while the evolutionary approach provides an answer to the original question, it fails to explain why evolutionary forces operate on human language but not on the communication that takes place among animals.

This question was first raised, not in terms of the evolution of language, but in terms of the very existence of a language, by Adam Smith. The answer that he proposed is that language is hardwired in humans but not in animals. Rubinstein concludes by asking whether considering the evolution of language rather than its existence simply begs the most important question: the difference between humans and animals. The idea that language is hardwired in humans is, of course, pervasive in linguistics.¹ Of course, the hardwired hypothesis could present some challenges for arguments that explain certain features of natural language in evolutionary terms. The point is that the evidence points to the fact that the key rules governing the syntax of natural language are hardwired in humans. They tend to be strikingly similar in situations where learning from other humans can be excluded as the means by which the common structure has evolved. So, the efficient language structure would have to be selected by evolutionary forces at the hardwiring stage, not while the language is used to, say, facilitate trade among humans.

The last aspect of natural language that is discussed in the book is the structure of debates. In particular, the author starts from the observation that in a debate the interpretation of a statement used as an argument differs considerably from the interpretation of the same statement used as a counter-argument. Once again, the explanation can be found in the attempt to elicit efficiently the information communicated to outsiders by the debate. A planner that is constrained by the number of arguments that can be made will, first of all, impose a sequential structure on the game form representing the optimal debate. The planner will also select a 'persuasion rule' of outside observers that treats asymmetrically an argument and a counter-argument. In other words, once again, efficiency considerations provide a rationale for why the strength of the same statement differs when this statement is used as an argument as opposed to a counter-argument in a debate.

The last two chapters of the book are grouped under the rubric 'the language of economics'. The first of these, Chapter four, raises an interesting puzzle. When modelling the preferences of individual agents, economists tend to favour certain utility functions. Rubinstein's working hypothesis is that the constraint on preferences might arise from the language that the decision-maker uses to verbalize the decision taken. The author goes on to formalize this working hypothesis and derive a set of preferences that are 'definable' by means of binary relations. The surprising feature of Rubinstein's analysis is that the most natural (definable) preferences that can be derived using this construct are lexicographic preferences. These are also the least popular among the preferences that economists use in describing the behaviour of individuals.

The fifth and final chapter has a rather different tone and emphasis from the preceding ones. The author presents his critical view of the language of game theory. In particular, he argues that the popular success of game theory and its ability to permeate the jargon of businessmen and politicians can be explained, at least in part, by the language used. He argues that, however, this language is misleading: it tends to depict game theory as an applied topic that provides users with ready-to-use predictions, quantitative answers and uncontroversial solutions, whereas nothing could be farther from the truth. As with 'classical' economic theory, game theory is a 'search for *connections* between concepts, assumptions and assertions which we use in understanding human interaction'. The applicability of the subject is not its strength, and according to the author it is not a virtue either. Game theory, and more generally economics, is a *language* that helps us understand better certain spheres of human interaction.

We find it extremely hard to disagree with Ariel Rubinstein on this view. It is impossible not to admire his intellectual honesty. By now, the term 'methodology of economics' suggests to most a rather outdated debate, one that has not received new blood for a very long time. Perhaps a fresh look would suggest the study of economics as a *language*, one that goes beyond the mere observation that it is in fact a language concerning human interactions. Ariel Rubinstein does not tell readers explicitly what his views are on this point. If one had to level a criticism of the volume, it is probably the lack of a sixth chapter, discussing the structure of modern economics as a language of the social sciences.

NOTE

1. An accessible and extensive text that portrays the status of the hardwired language hypothesis, including the accumulated evidence that supports it, is Steven Pinker's *The Language Instinct* (New York: Harper Collins, 1994).

Economics and Language. Five Essays. By ARIEL RUBINSTEIN. Cambridge University Press, Cambridge. 2000. viii + 128 pp. £26.95. Paperback £9.95.

One picks up this book with a modicum of trepidation, rather as if one had stumbled across a pamphlet by Eminem on *How Opera Works*. While the experience of reading it does not altogether dispel those initial fears, nevertheless it does prompt one to reflect upon the fact that seems to have eluded the author: that there exists a long string of attempts by analysts inspired by neoclassical economics to try to say something cogent about the mysteries of language, ranging from Saussure's appropriation of Walras and Fraser's gloss on Marshall to McCloskey's late forays. While some of these essays appear to take for granted that the reader will cheerfully acquiesce in starting from scratch (or, worse, game theory) in searching for that elusive Economic Theory of Everything, it is perhaps more troubling to observe just how impertinent his ambitions would appear to someone situated outside the charmed circle of economic theorists. Indeed, the history of analytic philosophy in the last century, from Schlick to Brandom, has constituted little more than the protracted vain search for an ideal language, expressed as an austere formalism which would optimize meaning or facilitate communication. The dream of an optimal language that conveys only what has been consciously put into it has enjoyed a revival lately, thanks to the computer and the hype of artificial intelligence, but that does not belie the rational expectation that no small set of formal conditions will ever encompass the gnarly ambiguities of language.

This volume consists of a disjointed set of five essays delivered as the Churchill Lectures in 1996. The first proposes that constrained optimization can 'explain' a putative prevalence for linear orderings in (essentially written) languages; the second imagines that meanings are assigned to words as the outcome of evolutionary stable strategies in repeated games. The third lecture purports to deal with pragmatics, but awkwardly shifts gears in the middle to model something perhaps misleadingly called a 'debate' in order to suggest that arguments need not conform to strict transitive orderings in 'beating out' rival arguments. The fourth lecture seeks to assert that the manner in which an agent represents his preferences to himself (and here one can not shrug off Wittgenstein's derision about 'private languages') serves to restrict the shape of those preferences. The fifth essay is a series of complaints about the ways in which game theory is justified in the economics literature, especially with regard to the concepts of strategy, equilibrium and solution concept. The final essay closes with the opinion that has been gaining ground of late that, 'I have no expectations of Game Theory becoming "practical" as the term is understood by most people' (p. 88).

As might be suspected in confronting the proposals of an autodidact, a number of infelicities are committed in the name of economic imperialism. In the first two essays, for instance, 'language' is treated as if it consisted solely of nouns and verbs of a single tense. In the second, organic evolution is baldly conflated with maximization. The third confidently posits that the overriding purpose of human debate is to 'extract information'. The fourth imagines a property of preferences called 'definability', which is presumed to hold even when the preferences can be formally demonstrated to be non-computable. The fifth, however, attains new vistas in paradoxes of self-reference, with repeated assertions that game theory is not empirically verifiable or conceptually useful in structuring problems encountered in real life—all contained within a volume by all accounts devoted to the application of game theory to do just that. But then, the author blithely admits in passing that most game theorists do not have a firm grasp on what they are doing when they impose a particular solution concept upon a given game (p. 86). Language has the unnerving capacity to contain a universe never dreamt of in your philosophy, or in that of your interlocutor: a fact unpropitious for the widespread

deployment of the Nash equilibrium. Before the reader becomes snagged on the horns of a vicious dilemma, it may be necessary at this juncture to ask what it is that Rubinstein thinks game theory is good for? I personally find it difficult to extract any clear answer from this book other than that game theory is being done for its own sake—the last refuge of the autodidact.

It would be imprudent for me to recommend this book as anything other than a tonic for those still infected with the belief that a reputation for mathematical facility is *prima facie* evidence of rigour and consistency in thought and expression. Rather, the relevant question in this instance should be why it is that some people are encouraged to engage in public exhibitions of free semiotic play resembling the present set of lectures, whereas others are kept safely confined to the audience. An hour spent with Ludwig Wittgenstein's *Philosophical Investigations* and its meditations on language games will go much further to provide some insights into the vagaries of human discourse than months spent with the Nash bargaining solution.

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Economics and Language: Five Essays. By ARIEL RUBINSTEIN. Cambridge University Press, Cambridge. 2000. viii + 128 pp. £26.95. Paperback £9.95.

The five essays in this book cover a wide range of topics about the factors that mould the general shape of language, the structure of arguments and the interpretation of game theory. In the introduction Rubinstein gives a very bland description of what holds the book together, and at the end he says that the three other scholars who have added comments have convinced him that there is no deep unity to the ideas he is presenting. But there are threads, interesting threads, passing through all the essays.

The first essay asks why we are so comfortable with binary relations, particularly total orderings. Rubinstein presents a number of little theorems stating that, if you have a finite set and you want a small vocabulary to describe it, then you are best off with two-place total orderings. These are neat satisfying results. They can easily be interpreted, as Rubinstein does, as suggesting that in the evolution of language there will be a pressure to have words for binary relations. For example (he does not point this out), it is a universal of human languages that their syntax takes a subject/object/verb form, with languages varying in the standard order of these elements. But a verb taking a subject and an object is a two-place relation. The evidence that these relations are total orderings is much weaker. Many of our relations are comparatives ('bigger', 'better') and these usually admit of many incomparabilities (Mozart and Bach). And even the example of 'to the left of' that Rubinstein cites does not really work. Leftness is only an ordering locally, since the earth is round. This should not really matter: what Rubinstein's considerations suggest is that languages will have the syntactic resources for naming strict orderings when they need them, and this they certainly do.

Rubinstein assumes throughout that the logical machinery of a language will contain the apparatus of first-order predicate calculus with identity. Thus, we have the quantifier 'all' but not the quantifier 'most'; we have 'is identical to' but not 'is similar to'. Some of his results would fail without these assumptions. In the third essay he makes some preliminary observations towards giving a game-theoretic analysis of argument or debate, taking it to be a process in which two people try to persuade an audience, subject to rules that are designed to give the audience a chance to discover which persuader is right. The results of this analysis are less interesting than the idea behind it, and again there are some very restrictive assumptions—in particular, that when one persuader makes a claim the set of facts assumed by all changes to remove facts inconsistent with it. Conceivably, though, some such model could justify taking some skeleton of logical devices as basic to language, as it could turn out that the most efficient assignment of meanings to logical symbols to make a debate have the intended information-extracting function that would privilege the standard operators.

In discussing the evolutionary pressure on a language to take a certain form, Rubinstein considers variations on the idea of an evolutionarily stable strategy, as part

of an attempt to argue that there are some concepts, such as ‘danger’, that most languages will have simple means of denoting. Some of the argument here is reminiscent of attempts in epistemology a generation ago to find the best descriptive basis from which to make inductive generalizations. The consensus now is that this was a mistake, and that instead we should assume that agents come equipped with general beliefs and observational capacities, and should try to articulate the ways in which they will form new beliefs. So too here, one suspects, form may be more tractable than content.

In the remaining essays Rubinstein discusses how the terms we use to describe game theory, in particular the loaded word ‘solution’, warp our understanding of the theory. These essays are full of material that will be useful for resisting facile game-theoretical explanations of real-life situations. At first sight these seem quite disjoint from the earlier parts of the book. But there is one fascinating link. We are biased in choice situations to considering options we can easily describe; so too when considering what options other people may be considering, or may be expecting that we are considering. In fitting an abstract game-theoretic analysis to a situation involving real human beings, therefore, we ought to factor in the linguistic resources available. This ought to constrain the range of solutions that are serious candidates: they must figure in the subgames all of whose moves are describable by the people in question. So the application of game theory is constrained to respecting limits on articulateness which, if Rubinstein is right, are themselves products of a larger evolutionary process in which restricted agents pressure one another into possession of efficient expressive devices. Seen this way, the book is more of a unity after all.

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ADAM MORTON

Markets, Games, and Organizations: Essays in Honor of Roy Radner. Edited by TATSURO ICHIISHI and THOMAS MARSCHAK. Springer, Heidelberg. 2003. vi + 314 pp. £56.

This book is a collection of 17 essays written in honour of Professor Roy Radner’s 75th birthday, first published in two special issues of the *Review of Economic Design* in 2001. Radner’s impact on economics has been phenomenal, both through his own work and through the influence on his students, colleagues and co-authors. The essays are by his former students at Berkeley, his former post-doctoral fellows at Bell Labs and his published co-authors. The list of contributors is very impressive. The papers are of high standard, as would be expected from the contributors writing in honour of one of the leading economists of the last fifty years. They cover a wide area of economic theory, many in which Radner himself has been a seminal influence. In my review I will concentrate on a few papers.

The first paper is by Rabi Bhattacharya and Mukul Majumdar who examine the problem of survival in a competitive economy. The issue is to study the likelihood that the value of endowments is greater than some threshold level of income (which depends on the current prices) necessary for survival. After a simple example, which would be excellent for classroom exposition, the authors obtain general characterizations. The asymptotic probability of survival can be calculated, and one of the key insights is that indirect term-of-trade effects can lead to ruin. This problem echoes Amartya Sen’s concept of entitlement failure. In small economies one can use these ideas to look at problems of terms-of-trade and current account sustainability in an international trade context. Rose-Anne Dana examines the uniqueness of competitive equilibria in finite and infinite dimensional economies (with and without complete markets) when consumers have an additively separable preference. The paper derives an important set of results, as this class of preferences is typically used in finance where uniqueness of equilibria is often implicitly assumed.

The paper by Kenneth Arrow looks at the question of entry of new firms in a situation where there is limited knowledge of productive opportunities. The key question is how the entry of new firms should be financed. He stresses that an optimal

entry policy is a more efficient route to the development of a free market economy than privatization, as the latter does not generate any new capital. Entry can have a direct effect on savings, which can ultimately be used for modernization.

Itzhak Gilboa and David Schmeidler look at the theme of 'satisficing' which has been highlighted by Radner. They develop a model of aspiration levels: preferences determine actions, which in turn determine preferences. In the Gilboa-Schmeidler model, the preferences depend not only on current consumption but also on the entire history of consumption, and thus can be seen as a generalization of some ideas of Kahneman and Tversky. The model generates non-monotonic changes to demand in response to price changes.

Stanley Reiter addresses the design of economic institutions. He considers whether coordination of economic activity should be done through the market or through other mechanisms such as bureaucracies. He shows that in some circumstances markets cannot efficiently organize economic activity, and even if they can it may impose higher informational costs. This has important implications for the discussions on market reform and privatizations.

James Jordan looks at the game of allocating wealth, where, given the allocation, a coalition with half or more of the wealth is a majority which can redistribute society's wealth to itself. He characterizes the core and the stable set of the game. This is an interesting problem which highlights the role of majorities in appropriation of economic wealth in societies. Charles Wilson studies a bargaining game with a mediator. The mediator announces a solution according to some density function, and the two players have to accept or reject the offer. If the offer is rejected, the mediator makes a new proposal according to the same density function. This provides insights into the effect of mediators in real-life situations. Jess Benhabib, Aldo Rustichini and Andres Velasco develop a dynamic game of distortionary tax policy where the benevolent government provides public capital which is necessary for production. The optimal policy is time-inconsistent, and they characterize the policy where it is not desirable to revise the original tax plan at any point of time. This policy can lead to lower growth rates.

In their paper, Prajit Dutta and Raghu Sundaram study a dynamic model populated by expected profit maximizers and survival maximizers. The proportions of each type will be bounded away from zero, but survival maximizers will dominate in number while the profit maximizers will dominate in terms of wealth. This is an interesting line of research following ideas of Alchian and Friedman on the market selection mechanism.

There are also papers by Richard Ericson and Barry Ickes on the co-existence of barter and monetary transactions in the Russian economy, and by Peter Linhart on sealed-bid bargaining games with incomplete information. Harold Cole, George Mailath and Andrew Postlewaite look at investment decisions of individuals when they are motivated by their relative position; Leonid Hurwicz and Stanley Reiter study existence of system of distinct representatives (SDR) for a collection of sets such that an element from each set (the representative) uniquely identifies the set it belongs to; and Eric Maskin surveys results in incentive theory, highlighting Radner's important contributions to the area. Steven Williams's paper is his earlier important, unpublished, 1984 paper that clarifies Maskin's conditions for Nash implementation; Roger Lagunoff and Akihiko Matsui study the robustness of Folk Theorem results for a class of repeated games; and Robert Rosenthal examines an evolutionary game based on a modified prisoner's dilemma and discusses cost-effective ways of maintaining an efficient equilibrium.

The papers in the volume raise important issues that bear further study. While in general they are technical and abstract, they impinge on our understanding of economic institutions and market outcomes, and a familiarity with them would be beneficial to anyone thinking deeply on these issues.

The papers in this collection are journal articles, and would have benefited from a more general introduction to make them accessible to a larger audience.

Nothing is Sacred: Economic Ideas for the New Millennium. By R. J. BARRO. MIT Press, Cambridge, Mass. 2002. xxi + 179 pp. £16.50.

This is pleasant bedtime reading for professional economists, but not so pleasant if (like me) you do not share the author's well-honed free-market arguments recycled from the pages of the *Wall Street Journal* and *Business Week*. Students of macroeconomics are familiar with Barro's writings through such well-known textbooks as *Macroeconomics* (1993) and *Economic Growth* (with Sala-i-Martin) (1995), but here they meet him as a latter-day Milton Friedman addressing businessmen and the educated public on outstanding (largely American) issues of economic policy, such as abortion laws, the legalization of drugs, internet piracy, anti-trust policies, tax cuts, personalized social security accounts, the 'dollarization' of failing economies, the efficient market hypothesis and, therefore, the case for intervention in stock markets, etcetera, all treated at break-neck pace in a few pages.

To those who believe that economics is potentially value-free and independent of the political inclinations of economists, this book will provide food for thought, not all of which is unambiguous. When one compares it with that other stellar example of the same genre, Milton Friedman's *Capitalism and Freedom*, one is struck by the fact that Barro takes much more for granted than did Friedman. When Friedman argued for education vouchers, a negative income tax for all, a volunteer army, the reform of the welfare state and the elimination of affirmative action, he argued with gusto, combining economic, political and even philosophical considerations. Barro, on the other hand, too frequently does no more than assert his support for tax cuts, reductions in public expenditures, objections to antitrust policies, a flat-rate income tax and privatized social security without any arguments at all. Even when he leans on his own cross-country research to cast doubt on the declared aim of US foreign policy to promote democracy at all times and in all places, he does little more than point to the rule of law in sustaining property rights as more important than free speech and civil liberties when it comes to promoting economic growth. I am sure that he is right in his conclusion, but my point is that he writes as if the audience he is addressing requires little persuasion because it is already converted to free-market fundamentalism.

Throughout this book, Barro takes comfort from the fact that most of the empirical evidence around the world supports a belief in free markets, minimal government intervention and a hard monetary policy – and so he should. Since we are all inclined to look favourably on evidence that endorses our preconceptions, and to pour cold water on evidence that contradicts them, his willingness to give a hearing to recent evidence that the US legalization of abortion in the 1970s may well have been a causal factor in the drop of US crime rates in the 1990s is commendable. While not an extreme pro-choice advocate himself, he agrees that he would indeed alter his views in favour of choice, if abortion rights turn out to be a strong enemy of crime (pp. 76–7). Considering how central abortion laws are to American republicans around Bush, this is a dramatic political concession. However, what is of interest to me is what it reveals about the attitudes of economists to adverse evidence.

When Barro first stated what was later called the Ricardo Equivalence Theorem between taxation and public borrowing, he was not aware that the germ of this theory was in Ricardo. It was Buchanan who recognized it as a Ricardian argument, a genuine early example of rational expectations. However, Ricardo appears to have entertained it as a conjecture which he immediately dismissed because taxpayers did not hold rational expectations. Barro claims that Ricardo's doubts about the theorem were 'pretty much copied from Smith's *Wealth of Nations*' (p. 11), a claim that strikes me as unfounded. Equally unfounded is another claim, echoed over and over again by many other commentators: 'Adam Smith is justly lauded for his advocacy of free markets and limited government. Particularly famous is his idea that each person's pursuit of self-interest leads, as if by an invisible hand, to socially efficient outcomes' (pp. 8–9). The first sentence is unimpeachable, but the second one is utterly misleading. Smith believed that each person's pursuit leads, as if from an invisible hand, to a social consequence, whether for good or for ill, that was never intended by that person; this is the 'doctrine of unintended social consequences' that Hayek discovered in a number of

Scottish philosophers of the eighteenth-century enlightenment. The outcomes of free markets, Smith insisted, fostered economic growth, but it would never have occurred to him to call them efficient, a term that does not appear in *The Wealth of Nations*, a book Barro professes to admire.

I close this review with a truly pedantic objection: after paying a charming tribute to George Stigler in the opening pages of the book, Barro later refers to Stigler's Law of Eponymy – but attributes it to George instead of his son, Stephen Stigler, a distinguished historian of statistics (p. 155n.), thus unwittingly confirming the law that nothing is named after the person who actually invented it.

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The Economics of Exchange Rates. By LUCIO SARNO and MARK P. TAYLOR with a foreword by JEFFREY A. FRANKEL. Cambridge University Press. 2003. xii + 318 pp. £65; £24.95 paperback.

The cover of this book is festooned with praise from leading exchange rate academics, and the foreword by Frankel concludes: 'Sarno and Taylor's book is a *tour de force*. The exposition is comprehensive, covering contributions from all corners of the field, and covering the range from the seminal models of the 1970s to the latest discoveries on the theoretical and econometric frontiers of the 2000s. There is no excess verbiage or mathematics. Everything is there to serve a purpose. This is the current state of knowledge.'

This is one of the rare cases where the praise publishers solicit seems accurate; it is a very impressive book. It provides a comprehensive treatment of the foreign exchange rates that shows great familiarity with the institutional realities of the market, technical sophistication in both economic theory and econometrics, and a lot of care in ensuring clarity of exposition.

After the Introduction there are two empirical chapters. Chapter 2 reviews the large variety of tests of market efficiency (rational expectations plus risk neutrality) and Chapter 3, tests of purchasing power parity. Chapter 4 surveys the traditional exchange rate models: Mundel–Fleming, the sticky-price monetary model (Dornbusch), the flexible price monetary model, liquidity models and portfolio balance models. The models are set out within a consistent framework and the evidence for and against them is reviewed. The traditional models had *ad hoc* elements, and in Chapter 5 the authors review the optimizing new-open-economy intertemporal models of Obstfeld and Rogoff. The rest of the book examines specific topics: currency unions, pegged exchange rates and target zones in Chapter 6; official intervention in Chapter 7; exchange rate crises and speculative attacks in Chapter 8; and market microstructure models in Chapter 9.

As the authors explain, this is part monograph (they have done much of the research they describe) and part advanced textbook. In the preface, Mark Taylor says that one origin of the book goes back to when he was working as a foreign exchange dealer while pursuing graduate studies part-time. I remember in the early 1980s lecturing about how econometric tests had rejected covered interest parity; Taylor interrupted to say that the econometrics must be wrong, since he used CIP to calculate forward rates every day. Subsequently he showed that the econometrics *was* wrong – the data did not constitute feasible trades – and in a paper published in this journal in 1987 he showed that, with high-quality data, there are few profitable violations of CIP.

As befits a good textbook, the authors present the results in a clear and intuitive way, and hope that the book will be of use to financial market practitioners. However, the technical level behind their analysis is fairly high. There are some helpful descriptions of methods, e.g. on simulated moments and Itoh processes, but some parts may be heavy going for those not familiar with the techniques. Their knowledge of the literature and the history of exchange rates is encyclopaedic; they reference over 500 authors. Research students will benefit both from their suggestions for fruitful research areas and their plausible two- to three-sentence summaries of difficult papers.

The chapters are relatively self-contained and can be read on their own as surveys of particular fields. This says something about the subject. For instance, their general

conclusion in the opening empirical chapters is that there is strong evidence against both market efficiency (the forward premium has the wrong sign in predicting future changes in exchange rates) and short- to medium-run purchasing power parity (PPP). They follow this with a theoretical chapter that assumes both. This is a feature of open-economy macroeconomics: it is full of empirical puzzles that are difficult to reconcile with standard theories – the forward premium puzzle, slow adjustment of real exchange rates, the Feldstein–Horioka puzzle, the home bias in investments, etc. However, this is an area where the facts are rarely pure and never simple, and most of the conclusions are sensitive to arcane niceties of econometric specification. Their preferred explanation for the glacial pace of PPP adjustment is non-linearities: the real exchange rate is a random walk within a zone of inaction set by transaction costs, but it adjusts back to that zone quite rapidly on leaving it. Long-span data on the real exchange rate over centuries and panel tests provide some support for that interpretation, though there are technical problems with each. But even if this is true, it leaves the question as to why the zone of inaction should be as wide as it is and why it should be substantially wider during floating-rate rather than fixed-rate regimes. There will be no shortage of new results for a second edition.

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RON SMITH

Marshall's Tendencies: What Can Economists Know? By JOHN SUTTON. Leuven University Press and MIT Press, Cambridge, Mass. 2000. xvi + 122 pp. £15.50.

Today, economists learn about the world by building empirical models. This practice of empirical modelling emerged halfway through the twentieth century. Starting as 'Professor Tinbergen's method', it found its nursery in the Cowles Commission, where it grew to a 'standard paradigm' as designed by Haavelmo.

Right from the beginning, practitioners of this approach worried about the gap between their simple models and the complex and messy world they were attempting to model. And they – rightly – do today, considering the intriguing 'Professor Dr Gaston Eyskens Lectures' given by John Sutton and published as *Marshall's Tendencies*. To everyone who is captivated by this problem, I recommend that they read not only this elegantly written book, but also the symposium on the book, published in *Economics and Philosophy* (2002), with contributions by Marc Fleurbaey, Mary S. Morgan, Franklin M. Fisher, Carl F. Christ, Eric Renault and Kevin D. Hoover and including a reply by John Sutton. They give a much broader context to this book than I ever can give here.

At the heart of Sutton's lectures lies Marshall's analogy between the 'laws of economics' and the 'laws of the tides', which provides Sutton a nice framework within which to discuss the conditions for which the standard paradigm works. The tides are affected by two different influences. The primary influence lies in the gravitational pull of the moon and the sun, and this contribution can be modelled with great accuracy. But tides are also affected by meteorological factors, and these are notoriously difficult to predict.

Fortunately, they are a secondary influence, and by modelling the astronomical factors we can still arrive at a theory that affords us an adequate prediction, though always one that is subject to some error. In the same way, the standard paradigm is legitimized by the belief that a law of economics can be represented by a function of a small number of observable characteristics, subject to a small 'noise' component.

Trygve Haavelmo's classic article on 'The Probability Approach in Econometrics' developed a method for empirical research that was based on Marshall's analogy. To illustrate this method, let us assume that we can induce from a dataset the following relationship linking a variable y to a set of observable variables, x_1, \dots, x_n , in the sense that

$$(1) \quad y_i = a_1 x_{i,1} + a_2 x_{i,2} + \dots + a_n x_{i,n} + \eta_i.$$

Here the index i labels data points, and η_i is a stochastic disturbance term. When this relationship appears to be inaccurate, that is when $E[\eta_i] \neq 0$, or imprecise, that is when

$\text{var}(\eta_t)$ is large, these properties of the residual indicate that a potentially relevant factor is omitted. As long as the resulting relationship is inaccurate or imprecise, potentially relevant factors should be added. The tides analogy justifies the expectation that this method will result in the fulfilment of two requirements: (i) the resulting model captures a 'complete' set of factors that exert large and systematic influences; (ii) all remaining influences can be treated as a noise component with 'nice properties'.

So long as all potential influences are measurable, there is no problem, in principle. As datasets accumulate, we might reasonably expect to converge bit by bit to a closer approximation of the complete model, as all the most important x 's reveal their influence. But what if the tides analogy does not hold? What if there are variables that we cannot measure, proxy or control for, but which exert a large and systematic influence on outcomes? Then their presence will induce a bias in the estimated coefficients of the model that we fit: 'were we to impose the "correct" model, with the unobservable variables suppressed, then there is no reason to believe that our fitted residuals would have a nice structure. To use the quest for such nice properties as a basis for model selection may now lead us badly astray' (p. 23).

For those situations where the problems posed by these "unobservables" become so serious that we cannot hope to list all those important and relevant factors, Sutton introduces another analogy: Carnot's ideal steam engine. Instead of developing a realistic mathematical representation that covers all relevant measured features of a specific engine, to produce precise predictions, Carnot began with a different question. He asked: 'is there anything that can be said, independently of the details of design, about the factors that must ultimately limit the efficiency achievable by *any* engine?' (p. 60) With this question in mind, Carnot reduced the description of the engine to a simple abstract representation, in an attempt to isolate some basic features common to a large class of engines. Armed with this simple representation, he was able to establish a bound on the operating efficiency of any engine of a particular kind.

By introducing this analogy, Sutton actually makes a similar move to that of Robert Lucas ('Methods and problems in business cycle theory', *Journal of Money, Credit, and Banking*, 1980) in response to the standard paradigm strive to realismness:

Insistence on the 'realism' of an economic model subverts its potential usefulness in thinking about reality. Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently 'unreal'. (p. 696)

This analogy implies that model selection or testing should be based not on accurate and precise predictions, but on their ability to reproduce the same qualitative patterns as observed in the data.

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A Theory of Economic Growth: Dynamics and Policy in Overlapping Generations. By DAVID DE LA CROIX and PHILIPPE MICHEL. Cambridge University Press, Cambridge. 2002. xvii + 378 pp. £20.95 paperback.

This is an excellent book which I am sure will become a well-thumbed friend on the bookshelves of many students and researchers in the field of dynamic macroeconomic policy analysis. The book is based for the most part on the Diamond (one-sector, two-factor, two-period lives) overlapping generations model, and it concentrates on the policy areas for which this model is the most appropriate: the analysis of the optimality of equilibrium under perfect markets, public debt and public spending, optimal second-best taxation rules for financing public spending, and pensions policy. It also covers more briefly other important policy areas, such as human capital accumulation and the relative benefits of public and private education, intergenerational bequests and taste externalities. The strength and distinguishing feature of this book lies in the depth and detail of the analysis. Results are derived from first principles, yet the analysis is still

highly accessible. The level of detail is such that there are no jumps between lines, so that the detail has the effect of demystifying the analysis.

The analysis of ponzi schemes and public debt gives a good illustration of the nature of the book. Not only is the standard analysis presented, in which ponzi debt schemes (where the level of debt in each year is at least as high as the level of last year's debt plus interest payments) are not feasible when the rate of interest is greater than the rate of population growth, if debt repayments are paid for by taxes on only one generation; but also, it is shown that, if the government can tax both generations, then there is no restriction on the level of government borrowing, since the government can simply take away with one hand what it is repaying with the other. Thus, the old generation may be repaid an amount greater than the level of GDP, since this repayment is then taxed back and given to the young generation, which allows them to purchase the large level of debt. The implication of this for policy analysis is clear. Government is much less restricted in its actions than one would suppose from analysing models where the government must rely on only one tax instrument.

The great detail and depth of the book comes at the cost of breadth. Although the book is called *A Theory of Economic Growth* rather than *Theories of Economic Growth*, I am sure that many readers might expect some coverage of issues such as endogenous technological progress, endogenous fertility and the break-out from the Malthusian trap, and the impact of the international environment on growth (technological diffusion, international capital flows and international trade). Some might also be expecting more than a couple of pages on the empirics of long-run income levels and economic growth, and the effects of income inequality on growth.

Clearly, given the choice between covering all the bases to some degree and concentrating on a subset of growth theory in depth and detail, Professors de la Croix and Michel have chosen the latter. In so doing, they have provided us with an excellent teaching tool and reference book on the important issues of public debt, optimal taxation, public pensions and the optimality of equilibrium under perfect markets and complete information.

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