Economica (2004) **71**, 689–698

Book Reviews

Rational Ritual: Culture, Coordination, and Common Knowledge. By MICHAEL SUK-YOUNG CHWE. Princeton University Press, Princeton and Oxford. 2001. xiv + 130 pp. £26.95. Paperback £11.95.

The main argument of this book can be summarized in two sentences: 'Successful coordination requires common knowledge. Many existing institutions serve the purpose of creating the common knowledge needed for coordination.'

Chwe's leading example of a coordination problem is whether we should accept some authority, such as a king. This is in Chwe's terminology a coordination problem, because there is an incentive to coordinate actions: if everybody else accepts the authority, then I have a strong incentive to accept it, too. If everybody else rejects the authority, then my incentive to accept it is very small, or even negative. Chwe argues that those who want to reinforce an authority will create public acts in which the authority is recognized. It is not sufficient for the authority to verify in private that each individual is inclined to respect the authority. A public event is needed because it is important that everyone knows that everyone else is inclined to accept the authority, and that everyone knows that everyone knows that everyone else is inclined to accept the authority, etc. Knowledge chains of this sort are referred to as *common knowledge*.

It is clear that knowledge of others' plans is useful for resolving coordination problems, but why is higher-level knowledge useful? For example, why is it useful for co-ordination that I know that the others know what everybody's inclinations are? The answer is that such knowledge is useful because it will make me more likely to believe that the others will respect the authority, and then I too will become more likely to respect the authority. How do authorities create common knowledge that everybody accepts them? Royalty, for example, can do so by arranging 'royal progresses' in which everyone can observe everyone else expressing devotion to the royal authority. Royal progresses are thus among the rituals of which Chwe believes that their main function is to create common knowledge.

Chwe uses the expressions 'coordination problem' and 'common knowledge' in the technical sense of game theory, where these concepts have been formally studied at least since the work of Thomas Schelling and Robert Aumann. But Chwe considers a much larger variety of contexts than game theorists have previously had in their sight.

Chwe's book has a simple structure. The basic idea is informally developed in Chapter 1, and is expanded in a slightly more technical way in an Appendix. In Chapter 2 Chwe reviews a variety of real-world institutions and argues that these institutions can be understood as being aimed at generating, or disrupting, common knowledge in the context of coordination problems. Chapter 3 discusses possible objections to, and extensions of, the main argument. Chapter 4 is a very brief concluding discussion.

Chapter 2 is the heart of the book. The most prominent type of public event that is considered in this chapter, and of which it is argued that it reinforces existing authorities, is the *ritual*. Examples of public rituals that Chwe considers are royal progresses in sixteenth-century England, ceremonies among contemporary African tribes and festivals in France in the period immediately following the French Revolution. Chwe suggests that repetition in rituals is not redundant, but helps to create common knowledge, because repetition makes it safer for each individual to assume that the other individuals have heard the message.

Advertising is another activity that Chwe views as a means for creating common knowledge. This is relevant when the consumption of a good involves an element of coordination. An example would be computer operating systems. Chwe provides a small empirical analysis which shows that sellers of goods involving a coordination problem are more inclined to place their advertisements on television shows that are known to be seen by many viewers, and that these sellers are willing to pay a premium for such slots. If viewers know that the show they are watching is seen by many other viewers, then a commercial shown during such a programme is more likely to create common knowledge among a large group of individuals than if it is shown in other slots.

Michael Chwe's book stimulates the reader to look at familiar real-life institutions with new eyes. He presents an original and interesting idea. A person with an analytical mind who has an interest in life in general will learn much from this book, and will find much to think about.

The book is not without weaknesses, however. Concerning the theoretical structure, two crucial and related questions are not addressed in an entirely satisfactory way: (i) What is it really that needs to be common knowledge? (ii) In which precise way does common knowledge help successful coordination? Possible answers to the first question include people's preferences, people's knowledge and beliefs, and the set of actions available to people. Chwe does not commit to any single answer. In the Appendix, in which, for a stylized example, he provides a relatively formal version of his argument, the common knowledge concerns the availability of an action. But the common knowledge that is achieved by the real-world institutions that Chwe examines in the main body of the book often concerns other things. What is communicated in a tribal ritual? Is it information about people's preferences? Is it information about people's beliefs? Or should one view such rituals as 'cheap talk' in which no substantive information is transmitted?

Chwe sometimes suggests that the 'content' of the information transmitted is not as relevant to his argument as the fact that common knowledge is generated. But if one wishes to understand precisely how common knowledge facilitates coordination, then it surely does matter what is commonly known. If it were irrelevant to the coordination game to be played, then the fact that it is commonly known would not necessarily be important. If rituals are cheap talk, then it isn't obvious why the fact that this cheap talk becomes common knowledge matters.

Even if available actions, players' preferences and players' knowledge and beliefs are common knowledge, coordination games have multiple equilibria. This is clear in the example that Chwe considers in the Appendix. In that example, without common knowledge there is sometimes only one equilibrium, but with common knowledge there are two equilibria. It is then by no means automatic that the generation of common knowledge allows co-ordination on desirable equilibria. Even if it becomes common knowledge that Apple computers have been advertised on television, and therefore perhaps that we all know about their features—and even if it were preferable for all of us to switch to Apple computers—the subsequent game still has an equilibrium in which none of us uses Apple computers. So why does Apple find it worthwhile to pay for expensive advertising slots? Although Chwe acknowledges the problem of multiplicity of equilibria in the Appendix, he does not give it prominence in the main body of the book.

As he acknowledges, common knowledge is not always perfect. Repetition of the same message, for example, in a public ritual may take us closer to common knowledge; but if the possibility that some of us do not pay attention is to be reckoned with, then even repetition will not generate perfect common knowledge. But then, the question arises as to whether an event that takes us 'closer' to common knowledge facilitates coordination. This is a comparative statics prediction, and it is not clear on theoretical grounds that it is true.

Chwe's analysis of real-world examples that illustrate his theory sometimes rests on secondary sources. Parts of the book read like a review of what other people have said about royal progresses, tribal rituals, etc. A larger proportion of original empirical work would have been more satisfying.

Chwe's empirical work is understandably aimed at supporting the theoretical argument on which the book is based. But what does the fact that he can find a list of such examples tell us about the validity of the theory? Even if the real world were largely random, one would still find examples to support a given theory. The argument would be stronger if one could find a set of situations that is not pre-selected, and if some situations involved coordination problems and some other did not. One could then test the hypothesis that institutions generating common knowledge are more likely to exist when a coordination problem is involved. Chwe's work on advertising is based on this methodology, but his other examples are not.

The critical remarks of this review should not detract from the qualities of this book. Chwe's work contains a gem of an idea, and the above comments are meant to indicate how this idea could be further developed. The originality of Chwe's thinking, and his courage in stepping over the boundaries of academic disciplines, deserve admiration.

University College London TILMAN BÖRGERS

The Scientific Study of Society. By MAX STEUER. Kluwer Academic Publishers, Boston, Mass. 2003. xix + 464 pp. £38.50.

The Scientific Study of Society is an exciting and ambitious undertaking. Its heart lies in a survey of what the five social science disciplines (anthropology, economics, political science, social psychology and sociology) had to say about six representative topics (crime, immigration, the family, money, housing and religion) during the 1990s. The data come from looking at everything published on these topics in the main academic journals in each field. In some cases, all articles on a topic can be discussed, though in others a representative sample has had to be taken. The six topics were chosen because they were ones on which all five social sciences had things say. Steuer convincingly argues that other valid selections would produce substantially the same picture.

The author's main aim in this book was to provide a picture of what social science is, using examples of the best social science research. There are several features of social science research that he wished to illustrate, and to do this he varied the way he organized his material from chapter to chapter. The first point he wished to make was that different social sciences approach the same topic in different ways, and he deals with this in the chapter on crime. Anthropology offers detailed studies of specific, identifiable cases. Social psychology, in contrast, considers populations through statistical analyses of questionnaires about people's attitudes. Economists use much more formal, deductive modelling. Political science involves a mixture of case studies and statistical analysis. Sociology also uses statistical analysis, but emerges as a residual discipline, covering issues that are not covered by the other social sciences. After this, the other chapters are arranged to bring out different features of social science. Work on housing is organized in terms of policy relevance; that on the family reveals types of fact and explanation found in social science; money is used to show the way in which social science research is connected to other research.

Steuer uses this complex picture to make the point that social science is possible—that it is not an oxymoron. Here, his target is lay people who dismiss it (or politicians—one remembers Margaret Thatcher's renaming the Social Science Research Council the Economic and Social Research Council). He is also targeting practitioners of various social science disciplines, who need to know about work in other disciplines that is complementary to their own. His message is that social scientists need each other (which is not the same as saying that interdisciplinarity is inherently good). Anthropology provides 'intimate, subtle and detailed enquiry' (p. 375). Social psychology gives insights into how people think, often providing facts that can be used to test other social scientists' theories. Economists, on the other hand, have a powerful theoretical apparatus that no other social science can match. A further message is that not enough social science work is conducted: he finds it surprising how little research has been done on many very important topics.

Much of the time, Steuer simply reports what social scientists have done, leaving it to the reader to decide whether to believe it. However, he passes judgments quite frequently, pointing out where social scientists have failed to take account of problems that seem obvious, such as that causation might run in directions different from those assumed. More important, judgment is involved in deciding what constitutes social science and what does not. He explicitly excludes six topics that do 'not belong in an investigation of the scientific study of society': social theory, post-modernism, post-structuralism, risk, networks and globalization (p. 369). The first three are methods, not social science, which he defines in applied terms as the study of social phenomena; the last three are about social phenomena, but he is sceptical about them all. He regards social theory and its similar approaches as based on 'foolish suggestions' on how to think about society (p. 371). In the literature on risk, 'we get serious sounding statements of the obvious, side by side with the obviously wrong' (p. 372).

I have picked out these points, though they are very minor in the context of the book as a whole, because they help to explain what the book does and does not provide. Steuer has a clear view of what science is, and the book aims to discuss research that falls within this category. He allows economic theory in, but the main thing he is looking for is empirical, applied work. This means that the book does not provide an account of what one finds when one looks at what is going on in the five social science disciplines. For example, economists do much work on theory, but Steuer's selection criteria mean that he cannot provide a systematic overview of it. This matters, because it is possible to argue that without an understanding of economic theory it is not possible to make sense of economics as a whole. Similar remarks could, no doubt, be made about other disciplines. In Steuer's terms, the five disciplines include much that counts as social science—the scientific study of society—but also much that does not. It is possible to agree with his judgments on what science is about and on the value of much of the work that he excludes, but also possible to believe that it would have been useful to have the disciplines themselves analysed a bit more comprehensively. As it is, the case for certain types of social science research rather than others is not made in detail.

A helpful way to look at this book is to view it as a piece of social science research, for social science is a part of society and analysing it scientifically is therefore social science. (This is the problem of reflexivity, which has been extensively discussed in the sociology of science.) Viewed in this way, what method is Steuer following? Though an economist, he is clearly not undertaking an economic analysis of social science—there is no theoretical model and no statistical inference. Such an approach could have been followed but was not. Nor is he engaging in social psychology or political analysis—and again, such approaches would have been possible. If forced to classify his book, I would suggest that it comes closest to an anthropological study—case studies of how social scientists view the topics they are tackling. As such, the book arguably suffers from some (though not all) of the weaknesses he finds in anthropology. The theoretical structure is weak and it is hard to test generalizations. Steuer does not engage with others who have studied social science or even science in general, whether philosophers, sociologists or historians. He does not even identify those of whose work he is critical. This means that his judgments, though founded on an impressive knowledge of his subject matter, might appear to be 'amateur' rather than 'social scientific'. (This impression is reinforced by frequent typographical errors and some mis-spelling of names.)

The extensive bibliography covers his subject-matter—his database, as it were, of articles on the six topics—but not references to other literature on social science. It is perhaps a pity that he did not include 'science' as one of his six topics. This is something on which economists, political scientists and sociologists have had much to say, and reflecting on social scientists' analyses of science might have suggested ideas that could have been used in his own inquiry.

The book's approach and conclusions are defined by four chapters, two at the beginning and two at the end: 'What is social science?', 'Valid and invalid alternatives', 'What social science is' and 'Social science as public policy'. These appear to contain no references at all to the literature. Because he does not engage with the literature, the case for his approach, rather than others followed in the literature on science, is not made. The result is that many of his judgments are somewhat speculative. For example, he considers the idea that a problem for social science is that it has never attracted the first-rate minds that have entered the natural sciences. As it stands, is this not simply an amateur judgment that needs to be investigated by the methods of social science research? What grounds are there for arguing that Keynes was inferior to Darwin or Newton? What does he make of the incursions of undoubtedly first-rate minds such as that of John von Neumann into economics? Perhaps the problem is that our judgments of the quality of scientists' minds reflects their achievements, and in social science the dramatic achievements that would lead to that judgment are not possible. Perhaps, as Max Planck is reputed to have said, social science is simply more difficult than physics.

The Scientific Study of Society reports the result of a fascinating, highly original research project. In my view it has significant limitations, but to a certain extent these reflect the extremely ambitious nature of the undertaking. The book is well informed and hits many nails squarely on the head, and everyone with a serious interest in the social sciences ought to read it. Hopefully it will be the prelude to further work on what is a very important topic.

University of Birmingham ROGER E. BACKHOUSE

Assessing Rational Expectations: Sunspots Multiplicity and Economic Fluctuations. By R. GUESNERIE. MIT Press, Cambridge, Mass. 2001. xxiv + 319 pp. £23.95.

The theory of rational expectations has so deeply marked the evolution of economic modelling that, as explained in the preface of the book, 'the main participants of modern macroeconomic theory, who may be antagonistic, seem nowadays to accept the idea that macroeconomic phenomena have to be explained within the rational expectations paradigm' (p. viii). This egemonic position justifies the careful evaluation of the theory that is offered in this volume.

Assessing Rational Expectations is a collection of ten papers by Roger Guesnerie. From a methodological point of view, the chapters of this volume are all concerned with an internal criticism of the rational expectations hypothesis. By 'internal criticism', Guesnerie means a critical assessment of the consequences of the rational expectations hypothesis. A companion book shall be concerned with an external criticism, that is a critical assessment of the hypothesis itself through the discussion of its foundations.

As the title suggests, the connecting thread of the assessment is the multiplicity question that arises in rational expectations models and the attempt to derive the implications for a theory of endogenous fluctuations. Two are the key concepts. The first is that of indeterminacy, that is the continuum of rational expectations equilibria often arising in infinite horizon economies. The second, which is a new source of multiplicity, is that of Stationary Sunspots Equilibria (SSE), that is rational expectations equilibria in which agents' representations of future events are based on extrinsic signals deemed relevant.

Assessing Rational Expectations is divided into four parts. A nice preface is very useful in putting the analysis into a broad intellectual perspective.

Part I is devoted to a clarification of the theoretical construct of SSE in the framework of one-dimensional overlapping-generations economies. A distinctive feature of this part is the attention devoted to the characterization of the stochastic processes triggering the self-fulfilling prophecies. Chapter 1 (previously published in French) introduces the method for analysing SSE based on the Poincaré–Hopf index theorem, and discusses the role of conflicting theories. In Chapter 2 the method is refined and exploited to investigate the connections between deterministic cycles and SSE. These two chapters (both co-authored with C. Azariadis) are concerned with sunspots of order 2. Sunspots of order k are the subject of Chapter 3 (jointly with P. A. Chiappori), which develops from a more axiomatic basis the Poincaré–Hopf procedure.

The two chapters of Part II (both written with P. A. Chiappori), by focusing on the nature of the stochastic processes triggering sunspot equilibria, address a key point. Here the authors show that, instead of being triggered by an extrinsic signal whose origin is not always clear, sunspot equilibria may be viewed as self-fulfilling overreactions to small variations of intrinsic variables. In this perspective, some sunspot phenomena are more focal than others. One leading candidate is the stochastic process governing money supply, discussed by Lucas in his 'Expectations and the neutrality of money' (*Journal of Economic Theory*, 4 (1972)). It is well known that, if attention is restricted to the class of price functions linear in the stock of money (a strong 'neutrality assumption', since it does not stem from Lucas definition of rational expectations), then the Lucas model predicts a unique solution (stationary in probabilistic terms). Chapter 4 shows that, in addition to the Lucas solution, a continuum of (non-stationary) solutions exists, all invalidating the strong 'neutrality assumption'. This questions the referential status of the Lucas solution. The issue is further developed in Chapter 5, where it is shown that the non-Lucas solutions have the theoretical status of sunspot solutions where the sunspot variable is represented by the realization of the exogenous money supply. Both chapters provide very important examples of a world in which two conflicting theories, one Keynesian-like and the other monetarist-like, can be alternatively self-fulfilling.

Part III presents an extension of the themes of Part I to an *n*-dimensional, one-step forward-looking, economy. Among other things, Chapter 6 shows that, because of the multiplicity of equilibria that can arise in an *n*-dimensional world, a particular category of sunspots emerges in which sunspots may work as selection criteria. Chapter 7 (written with P. A. Chiappori and P. Y. Geoffard) focuses on a special class of equilibria generating small fluctuations around a deterministic steady state and provides a complete characterization and classification of the equilibria.

Part IV is devoted to extensions and variations. Chapter 8 (written with M. Woodford) studies the connection between determinacy and stability under adaptive learning of a deterministic cycle. Chapter 9 (jointly with J. Davila) is an exploration of the existence of SSE in one-step forward-looking models with memory. A broad perspective on the analysis of previous chapters is given in Chapter 10, where the connections between indeterminacy, sunspot multiplicity and learnability of rational expectations equilibria are scrutinized.

The papers collected in this volume testify to Guesnerie's highly authoritative and influential role in developing a research programme challenging the egemonic status reached by the rational expectations theory. The book is a very valuable reference for students of dynamic macroeconomic models.

Università Cattolica, Milan GIORGIO NEGRONI

Machine Dreams: Economics Becomes a Cyborg Science. By PHILIP MIROWSKI. Cambridge University Press, Cambridge. 2002. xiv + 655 pp. £70. Paperback £24.95.

Philip Mirowski is known for his provocative interpretation of the history of neoclassical economics as an importation of concepts and metaphors from other sciences. In earlier works he has argued for the decisive influence of thermodynamics on nineteenth-century marginalist economics; in his latest book he argues that neoclassical economics from the Second World War to the present has lost its protoenergetic character by becoming more or less directly influenced by the theoretical developments surrounding the advent of the digital computer.

The core of the story is built around the intellectual career of John von Neumann, best known for his co-authorship of the *Theory of Games and Economic Behaviour*. For Mirowski, however, this work is not von Neumann's most significant contribution to economics; rather, he paints it as a transient stadium between the genius's quest for a formalized mathematics and the development of the theory of automata. Game theory is to be understood as merely a 'tentative exploration of various paradoxes of certain definitions of rationality' (p. 134), while the theory of automata, discussing all forms of self-regulating information processing, provides the general approach to rationality. For Mirowski, cyborg sciences started here: with the discussion of natural or social phenomena as information processes of a finite computational machine. This, and not his theory of games, is von Neumann's 'most profound contribution to economics' (p. 139).

It is also here that Mirowski spots the crucial theoretical fork. Von Neumann, with all of his interest in questions of rationality, showed no inclination to apply the theory of automata to questions of human cognitive architecture. His interests focused on questions of organization—in the realm of biology, in military operations and in social questions in general. And so it was not von Neumann, but the economics profession, that developed the idea of human cognition as algorithmic processes and turned it into a reinforced concept of economic rationality.

Machine Dreams recounts how the military organization of science in and after the Second World War facilitated that transformation. Created by a small group of 'science managers' under von Neumann tutelage, Operations Research provided a new field of interaction between scientists and the military. Novel hierarchies emerged; science was subordinated to a new division of labour; and most importantly, as Mirowski reminds us, large parts of science became financially dependent on the military. This holds in particular for the Air Force think-tank RAND, a harbourer of cyborgs, which in 1948 bought into one of the holy grails of neoclassical theory, the Cowles Commission. In this institutional dependence, Mirowski sees the decisive impulse to reconceptualize the neoclassical agent: 'Cowles preserved its neoclassical price theory by recasting its a priori commitment to utilitarian psychology as though it were best described as the operation of a virtual computer' (p. 222). Cyborg science went cognitive, a transformation that Mirowski finds as dislikeable as it is pivotal for contemporary economics.

Finally, algorithmic rationality infected game theory itself, changing von Neumann's original project considerably. The Nash Equilibrium, for Mirowski the paradigm case of the 'rationality of the paranoid' (p. 343), stands for him in marked contrast to von Neumann and Morgenstern's earlier work. Mirowski points to various sources that report von Neumann's rejection of the equilibrium concept; further, he finds in Nash's work all of the ingredients that had beset the Cowles Commission: 'hyper-individualism, non-accessible utility functions, constrained maximization, and cognition as a species of statistical inference' (p. 348). The Nash equilibrium, Mirowski concludes, is to be seen as a 'logical extension of the Walrasian general equilibrium tradition into the Cold War context' (p. 339), but not as a continuation of von Neumann's project.

Mirowski leaves no uncertainty about his opinion that this tine of the fork is a cul-de-sac; and he shows that the 'true cyborgs'—von Neumann's acolytes—are with him on this. In the enthusiastically titled chapter 'The Empire Strikes Back', he shows how the cyborgs took issue with the concept of algorithmic rationality itself: first by questioning the computability of Arrow's choice function, and thus the rational preference assumption of his Impossibility Theorem; and later by targeting the assumptions of common knowledge implicit in the Nash Equilibrium. Computability became a criterion of rationality—a torpedo big enough in Mirowski's eyes to sink the Walrasian vessel.

In reaction to these complications, Mirowski claims, Herbert Simon developed his program of simulacra. Instead of trying to grasp the whole cognitive structure of human thinking in a grandiose theory of rationality, and therefore falling into the same trap that Cowles and the game theory revival allegedly did, Simon simulated piecemeal human behaviour, specific to task and environment. In doing so, he began the quest for overall consistency, or even for coherence towards a unified self. Thus, although he shared the conviction of the centrality of the computer with von Neumann, Simon ended up diametrically opposed to him, as a '"humanist" and anti-foundationalist' (p. 471) cyborg.

The computer and the theoretical development surrounding it have shaped neoclassical economics in a variety of ways. Mirowski's impressive archival research, which he presents in his inimitably tapered style, reports this with clarity and excitement. However, the major thesis of this book—that there is a *uniform* influence of the cyborg sciences on economics—suffers somewhat from the ambiguity of the cyborg concept itself. That might be due partly to the fact that the computer itself did not stand still during that period, as Mirowski puts it, and that therefore the concept was deliberately kept flexible. But beyond that, it is so overcharged with connotations that never really get clarified, and the reader so repeatedly loses track of who is and who is not in the cyborg camp, that the conceptual framework of this history must be taken with a grain of salt.

The purported uniformity of this history might instead serve another purpose: to create an antagonism between two schools of economic thought on yet another level. While Mirowski steadfastly holds on to his champion von Neumann, and particularly to his cyborg project, he makes clear in no uncertain terms his contempt for the Walrasian school, their supposed 'rationality of the paranoid' and their conceptualization of the cognitive realm as 'acidly corrosive to the constitution of the human being' (p. 656). At the end of the book, Mirowski sketches von Neumann's project of automata theory as a platform for institutional economics, free from neoclassical orthodoxy, just as his hero–genius supposedly did. Here the reader is finally presented with the antagonism that underlies the whole book—and it makes one ask oneself whether doctrine and history really always fit so neatly together. Writing a history that culminates in a doctrinal conclusion is a somewhat odd project, but makes for a provocative and often inspiring read—as does this book, without question.

London School of Economics TILL GRUENE

Corporate Financing and Governance in Japan: The Road to the Future. By TAKEO HOSHI and ANIL K. KASHYAP. The MIT Press, Cambridge, Massachusetts. 2001. xx + 358 pp. £23.95.

For more than 150 years the Japanese economy has fascinated Western observers. However, over the last decade the basis of this fascination has changed. Before the 1990s, sustained episodes of extraordinary growth exerted the dominant attraction. Since then, a persistent, perplexing stagnation has taken centre stage. Yet whatever the period, foreigners have sought to understand and learn from Japan's economic experience. Takeo Hoshi and Anil Kashyap's new book now makes the learning process much easier. Their main concern is how (and how well) economic growth has (or has not) been financed in modern Japan. They break this large issue into four systematically treated components, examined over four epochs since 1868. The components are: (1) What financial assets do households accumulate when they save (e.g. currency, bank deposits, securities, etc.)? (2) To what extent and how are businesses externally financed (in particular, what is the extent of their reliance on bank loans rather than security sales through public financial markets, relative to internal or private sources of funding)? (3) What is the range of business services provided by banks (e.g. loans, asset management, security issuance and underwriting, advice on mergers and acquisitions, etc.)? (4) What is the nature and extent of bank involvement in corporate governance (in particular, how closely and effectively are managers monitored and what assistance is given to distressed borrowers)? These components are assessed over four periods: (1) the foundation of the modern economy, c.1868 (Meiji Restoration) to c.1937 (beginning of sustained war with China); (2) total war and occupation (c.1937–55); (3) the era of super growth (1955–75); (4) the era of transformation through deregulation (1975–98, still in progress).

Hoshi and Kashyap firmly believe that history and context matter. Unlike many writers, they see no mystery in the way the Japanese economy operates or is financed.

Once the regulatory environment is understood, economic behaviour becomes straightforwardly comprehensible. In their view, the financial system that emerged with the Meiji Restoration was no more bank-dominated than was the contemporary American one. Reasonably reliable data are available from the early Meiji period. From 1900 until the early 1930s, households usually held half or more of their financial wealth in marketable securities, with 30% or less in bank deposits. Businesses then correspondingly satisfied the bulk of their external financing needs through security markets rather than bank loans, although bank loans were undeniably important, amounting on average to perhaps some 40% of business external financing. The conspicuously bank-dominated system that emerged after 1945 was a new departure, initially a product of the military government's mobilization efforts, which shut down security markets and transformed banks into passive agents of armaments production, consciously deprived of any scope for loan evaluation. However, the Occupation, with very different objectives, did not alter this system. To secure the same planning ease the militarists had sought, security markets stayed carefully suppressed. Households had little savings alternative but to accumulate bank deposits. Moreover, the massive wave of corporate bankruptcies that accompanied the thorough-going repudiation of the military government's debts left the banks, which the authorities favoured in the process of financial recapitalization, playing an even more central role than they had done during the war—for over a decade after 1945, banks were the only organized source of external finance in a devastated and capital-starved economy. To be sure, after 1945 banks had to relearn at least the rudimentary skills of loan evaluation, but bureaucratic 'guidance' was still rife, reinforced by cheap funding from the Bank of Japan, at least for compliant banks.

This system began to break down after the oil shock of 1973–4. For the first time since the Pacific War, the Japanese government had to fund significant budget deficits, and for this task a functioning bond market was needed. Financial market regulation was accordingly adjusted. However, as the bond market gradually came back to life, the strongest Japanese companies began to seize the (unintended) opportunity of an alternative, and much cheaper, means of external finance. At first the consequences for the banks were muted, for Japanese growth was still by international standards unusually vigorous, even if not as vigorous as during the previous period of super-growth. But uneven deregulation had not given households the same enrichment of financial choice that corporations had enjoyed. So, while the most creditworthy corporate borrowers turned to security markets (many of them located overseas) to service their (often declining) external financing needs, the banks remained awash with domestic deposits, becoming increasingly desperate to find some use for them. The inevitable consequence, especially after the bursting of a spectacular asset bubble in 1990, was the massive pile-up of bad loans that now weigh so heavily on the Japanese economy. Hoshi and Kashyap conclude that, as deregulation is completed (the key legislation, if not its implementation, is already in place), the Japanese financial system will once again, as before 1937, be one in which markets (not bureaucrats) broadly decide asset allocations, with much-shrunken banks reduced to playing their near-universal role of mainly assessing the loan demands of borrowers too obscure or problematic to command access to public security markets.

This analytical narrative is lucidly presented and eminently plausible, backed by a rich parade of charts and tables. Moreover, it is underpinned by a properly scholarly bibliography, offering the curious (or sceptical) more than ample scope to delve deeper. And between them, the authors side-step the problem that has bedevilled Western understanding of the Japanese economy at least since the Occupation: the 'Japan hands' who know the country and its customs and institutions well have consistently counted few if any economists among their ranks, while most of the trained economists who have sought to comprehend the country have lacked the necessary background knowledge (not least of the legal system of a country famed for its sparing resort to lawyers). In addition to an appreciation of Japanese legal subtleties, the book offers (in Chapter 6) an astute application of the Modigliani–Miller theorems to aid constructive consideration of bank-centred keiretsu financing. However, not all questions are so neatly answered: in particular, the era of super-growth is covered only cursorily. No account, beyond a fleeting and vague reference to an export orientation, is offered to explain how such a bureaucratically dominated financial system could deliver such extraordinary results for more than three decades. Nevertheless, the virtues of this book dominate by a large margin—it deserves to be widely read.

The London School of Economics WILLIAM P. KENNEDY