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BOOK REVIEWS

The German Skills Machine: Sustaining Comparative Advantage in a Global Economy. Edited by CULPEPPER (PEPPER D.) and FINEGOLD (DAVID). (Providence, Oxford: Berghahn Books, 1999. Pp. xii+482. £56.00 hardback, US $85.00 hardback. ISBN 1 57181 114 3.)

The German so-called *dual system* of apprenticeship training which combines supervised in-firm training and theoretical instruction in out-of-firm centres is often considered a key element in the German success story as regards high international competitiveness in export markets and comparatively low youth unemployment. The papers in this volume (which are from two conferences at the American Institute for Contemporary German Studies in 1997) examine this apprenticeship system and the way further occupational training is taking place with a focus on the challenges facing all industrialised economies (e.g., an ageing population, and increased competition from lesser developed countries).

The volume contains nine papers, plus an introduction and a concluding chapter written by the first and the second editor, respectively. The material is divided into three parts. Part I 'Threats to the German System in Comparative Perspective' contains three papers which present the *dual system* in some detail, and which discuss some of its problems in the recent process of restructuring (flexible organisation, lean organisation), comparing the German exemplar with Japan, and with the United States. The three papers in part II 'Distributive Outcomes of the German Training System' deal with vocational training and job mobility (comparing Germany with Britain and Sweden), gender-specific aspects, and continuing occupational training in an ageing economy. Part III 'International Experiments with In-firm Training' includes a paper comparing reforms of the systems of vocational education and training in France and Eastern Germany, a study on sectoral training initiatives in the United States, and a contribution on Training and Enterprise Councils (TECs) in Britain.

Most of the papers are very informative, presenting a host of details describing the German system and comparing elements of it to the ways chosen in other countries. Unlike many collective volumes made from conference papers the contributions exhibit a high degree of coherency, and the editors did an excellent job in preparing a lengthy introduction that sets the scene, and in summarising the policy conclusions.

The authors who come from across the social sciences rely on verbal argumentation, often backed by descriptive statistics and case studies performed inside firms. To put it differently, no formal theoretical economic models are used, and no econometric methods are applied to test hypotheses derived from these models. This said, the volume is required reading for researchers in industrial relations who are interested in issues related to vocational training. Labour economists who prefer a more formal theoretical and empirical approach of analysis will nevertheless profit from the material in this book because it helps to put flesh to the bones of the models they use.

JOACHIM WAGNER

University of Lueneburg

Direct Investment in Economies in Transition. By MEYER (KLAUS). (Aldershot and Lyme, NH: Edward Elgar, 1998. Pp. xii+308. £59.95 hardback. ISBN 1 85898 736 9.)

Emerging markets offer major opportunities for business growth. The break-up of the FSU and the subsequent liberalisation of former centrally planned economies opens the way for firms from developed countries to enter these markets. This book is a contribution to a growing literature on direct investment in transition economies. It is rich in detail, and the discussion is dense. Although some sections read almost as a doctoral thesis and need a better structure and more in-depth analysis, the book contains a comprehensive survey of relevant literature and richness of statistical and qualitative information related to business environment and the conditions facing foreign investors in Eastern Europe and the Former Soviet Union (FSU).

Although the theoretical underpinnings of the author's analysis draw from a number of theories of internationalisation, the main thrust of his analytical approach is related to transaction cost economics (TCE). Within this framework, internationalisation decisions of the firm and the choice of organisational form depend on two types of transaction costs: the costs of using international market for goods and services and the costs of the internal organisation of the DFI project. The author develops a three-stage optimisation model that determines the firm's internationalisation decisions. At stage one, the firm has to decide whether it wants to be engaged in West-East business. Since this decision has been made, at stage two, the firm has to make a choice between market transactions and DFI. Finally, if the firm prefers direct investment to market transactions, it has to decide which entry mode is the most efficient one (i.e., a joint venture, a wholly-owned subsidiary, a 'greenfield' investment, etc.).

This theoretical framework generates a number of hypotheses. To test these theoretical predictions, the author assembled a database of 269 German and British manufacturing enterprises using a questionnaire survey. These data were used in the empirical tests that were mainly based on probability models. Chapters 6, 7, and 8 represent estimation results for each stage of the decision process of firms entering the transition economies. Although the author's discussion of robustness of various estimation techniques is rather scant, the empirical findings generally provide support to the model predictions, and this empirical analysis provides a very valuable contribution to the current research on economic and business aspects of transition.

A number of aspects of the book give it a slightly superannuated feel. The book's conceptual framework is developed in line with previous research that has been mainly focused on the perspective of the foreign partner. However, foreign partners considering entering emerging markets like FSU need to understand the perspective of their potential local partners. Partner selection is an important yet not well understood aspect of strategic alliances and internationalisation, and the limited available research suggests that partner selection is an important dimension of the success of alliances between foreign and local firms. The motivation behind foreign partners seeking alliances may be inconsistent with the objectives of local partners. Indeed, such conflicting objectives have been a consistent finding in empirical studies of joint ventures and other forms of alliances in transition economies. For example, local partners are usually anxious to improve technological levels in terms of new products and processes designed to secure export sales. In contrast, foreign partners are typically motivated by saturated home product markets and the need to establish a 'bridge-head' in potential foreign markets. Subsequently, local partners have often been disillusioned by an inability to export and by low levels of local procurement, as foreign partners protect employment levels at home (Buck *et al.*, 2000). In Russia, Filatotchev *et al.* (1999) report diametrically opposed objectives for local managers and outside investors. Potential foreign partners are viewed as seeking to acquire local enterprises in order to reduce capacity through closures, and this perception fuels the incumbents' entrenchment with respect to external direct investment. Although the author tries to make an account of an importance of local perspective on FDI, this analysis requires further development.

Another potential drawback of the book is a lack of a dynamic perspective on the FDI in emerging economies. According to the author's theoretical approach, each incidence and type of the FDI is an equilibrium response to a number of firm and country-specific factors within a simple optimisation model suggested by the TCE framework. However, several international business studies have indicated that internationalisation of the firms is a process in which the firms incrementally increase their involvement, gradually acquiring and using knowledge about foreign markets and operations. Within this essentially dynamic, learning model of internationalisation, an acquired knowledge may lead to successively increasing commitment to foreign markets. The basic assumption of this approach is that lack of such knowledge is an important obstacle to the development of internationalisation and that the necessary knowledge can be acquired mainly through operations abroad. Again, the author tries to make an account of these knowledge-based factors by differentiating between 'inexperienced' and 'experienced' firms in chapter 8, but, because of cross-sectional nature of the data, his analysis of possible evolutionary character of internationalistation seems incomplete.

In summary, I found this to be an interesting and informative book, whose analysis should be of interest to all economics and international business researchers concerned with the problems of direct foreign investment in post-communist economies. It represents a contribution to the economics of transition literature, both from theoretical and empirical points of view. My few reservations about it are not intended as criticisms of the author's theroretical approach, but rather as suggestions for improvement in his future research.

IGOR FILATOTCHEV

University of London

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Fabricating the Keynesian Revolution: Studies of the Inter-war Literature on Money, the Cycle, and Unemployment. By LAIDLER (DAVID). (Cambridge and New York: Cambridge University Press, 1999. Pp. xvi+380. £47.50 hardback, US $74.95 hardback, £17.95 paperback, US $27.95 paperback. ISBN 0 521 64173 X, 0 521 64596 4.)

This superbly written and cogently argued book does for interwar monetary and macroeconomic theory what Gottfried Haberler's *Prosperity and Depression* did for interwar business cycle theory. It assimilates and synthesises a huge body of literature – American, English, Austrian, Swedish – involving many theoretical frameworks, Keynesian and non-Keynesian, to discount the notion of a Keynesian revolution.

Laidler's integrating vector is the IS-LM model. He describes how in 1937 Brian Reddaway and James Meade laid out simple, small equation systems that claimed to capture the essence of Keynes's *General Theory*. J. R. Hicks then mapped these equations into his famous diagram that formed the workhorse of macro policy analysis for the next thirty years. Laidler notes that shortly after the model's formulation, Nicholas Kaldor employed it to demonstrate Keynes's truly original proposition that money wage cuts, by lowering prices and so raising real cash balances that spur spending, work exactly the same as increases in the money stock to boost employment. Likewise, Hicks and others used the model to demonstrate Keynes's central policy message, namely that fiscal policy could restore full employment when liquidity traps and interest-insensitive investment demand schedules prevent monetary policy from doing so. The model seemed stamped with the image of Keynes.

Laidler's startling thesis, however, is that IS-LM captures not just some of Keynes's insights, but also key elements of alternative theoretical frameworks that vied with the *General Theory* in the broad and diverse stream of international macroeconomic literature during the interwar period. Especially conspicuous among these alternative paradigms were (1) the heirs to Wicksell's natural rate-market rate analysis, namely the Austrian overinvestment theory of the cycle and the dynamic model sequences of the Stockholm School, (2) the Cambridge-Hawtrey cash-balance approach and its associated monetary model of the cycle, and (3) the Federal Reserve Board's real bills doctrine.

Of these, Laidler identifies the first as stemming from a Wicksellian tradition in which the rate of interest coordinates saving and investment. The second stems from a quantity theory tradition relating money to prices. And the third stems from a Banking School tradition in which output and prices drive money through business demands for bank loans, demands which commercial and central banks passively accommodate. He sees IS capturing elements of the first tradition, LM ingredients of the second, and IS combined with a flat LM curve parts of the third. The creative thrust of these frameworks together with the survival of parts of them in the IS-LM model lay to rest the myth that Keynes's work was revolutionary. Important, yes, but hardly revolutionary.

Hardly revolutionary in at least three ways. First, Keynes's policy prescriptions of cheap money (shifts in LM) and public works programmes (shifts in IS) were already standard fare in the pre-*General Theory* literature. Second, his notion of the short-run non-neutrality of money was forestalled by Ralph Hawtrey's monetary theory of the cycle in which wage and hence price level stickiness enables money stock fluctuations to affect real activity. Third, his notion of recessionary and inflationary gaps between the full-employement interest rate and the rate actually prevailing were already part and parcel of the Wicksellian tradition.

In his mission to revive overlooked work, Laidler sings the praises of many a neglected hero including Allyn Young, Lauchlin Currie, Frederick Lavington, and Dennis Robertson to name a few. His number one find, however, is Ralph Hawtrey. Hawtrey's contributions – his monetary theory of the cycle, his formulation of the Treasury View of fiscal policy, his anticipation of the concepts of effective demand and the multiplier, his argument that liquidity traps rarely exist because the availability of numerous assets substituting for money in satiating liquidity demands renders the LM curve steeply sloped, his belief that open market operations, if pushed hard enough, could overcome recessionary credit deadlocks, his application of the liquidity preference concept to banks' demands for excess reserves rather than to the public's demand for cash – all receive respectful recognition.

To Laidler's splendid work I have but three quibbles. First, his modus operandi is to make extensive use of numerous and long quotations from original sources. It is a matter of taste, of course, but to me the quotes eventually become tiresome and hinder rather than help the flow of the narrative. Given Laidler's expository gifts (which are far superior to some of the writers he quotes), he might have been better advised to paraphrase and synthesise the passages rather than quote them verbatim. Here he might follow the exemplars of Haberler's *Prosperity and Depression* or Schumpeter's mighty *History of Economic Analysis*, neither of which resorts to heavy quotation.

Second, I cannot quite agree with his argument that in the United States in the 1920s certain 'advocates of discretionary stabilization policy', notably Winfield Riefler, W. Randolph Burgess, Benjamin Strong, and Allyn Young, occupied a half-way house between the extremes of Fisher's quantity theory approach to the cycle and the Federal Reserve Board's real bills doctrine. The trouble with this classification is that these people never articulated a distinct and coherent policy model that stood as a clear alternative to the other two. Their analysis consisted of an eclectic amalgam of real bills and quantity theory strands. Depending upon which strand dominated, one could classify them as fellow travellers with either the real bills or the quantity theory camps. Riefler and Burgess clearly fall into the real bills camp. Their contribution was to reconcile the real bills doctrine with open market operations by positing a 'scissors effect' whereby such operations induce opposite and compensating changes in the volume of member bank borrowing at the discount window leaving total reserves unchanged. The result of the scissors effect was to incorporate into the real bills doctrine member bank borrowing and nominal market interest rates as key indicators of monetary ease or tightness. It was these indicators that led the Fed astray in the dark days of 1929–32.

Benjamin Strong could be ranked with either school. He rejected the real bills notion that type of paper pledged as loan collateral corresponds to particular use – productive versus speculative – of the borrowed funds. But he was willing to put the economy through the recession of 1921–2 to rid it of what he thought were the speculative excesses of the inflationary boom of 1919. Here was the mark of a real bills man.

As for Young, he had much in common with the quantity theorists, albeit eschewing their single-minded adherence to the goal of a constant price level. Like quantity theorists, he believed the Fed should use its policy weapons to induce countercyclical variations in the money stock so as to exert a stabilising influence on the economy. His vision of stabilising monetary policy thus was much like theirs.

Third, Laidler unduly slights the current resurgence of interwar ideas. He mentions but two. One is the optimising intertemporal agent approach of the Austrian School, which today finds its representation in New Classical models. The other is Robertson's theory of real business cycles complete with Kydland-Prescott ideas of gestation lags, durability and indivisibility of investment goods, real shocks in the form of cost-reducing technological progress, and the undesirability of removing cycles from growth. But other ideas have made comebacks. One is the Wicksell-Stockholm School idea that monetary policy influences aggregate demand directly through interest rates rather than through money stock adjustment. Another is the Austrian School's notion of the interest rate as an intertemporal coordinating price or marginal rate of substitution that, if maintained at its natural equilibrium level, keeps consumption on its optimal growth path. This idea justifies Fed interest rate hikes in response to permanent rises in the rate of productivity growth. Finally, there is the argument that Japan is caught in a Keynesian liquidity trap and the Chicago School-Hawtrey counterargument that she could escape the trap through monetary injections achieved either directly through fiscal deficits or indirectly through open market purchases of long term securities. Far from being passe, these ideas continue to flourish.

These quibbles, however, hardly detract from a truly outstanding work. Laidler's book is the best thing that has happened in the history of monetary thought in a long time. It is a must read for anyone interested in the development of modern macroeconomics.

THOMAS M. HUMPHREY

Federal Reserve Bank of Richmond

Computable Economics: The Arne Ryde Memorial Lectures. By VELUPILLAI (KUMARASWAMY). (Oxford: Oxford University Press, 2000. Pp. xi+222. £30.00 hardback. ISBN 0 19 829527 8.)

Economists' interest in the procedural lacunae of rationality undoubtedly follows from the protean figure of Herbert Simon. One marvels at the decades that have elapsed before we have a book of this stature to address the implications for economic theory of the epochal 20th century developments in mathematical logic and computation theory on the limits of finitely implementable procedures or algorithms. Whilst Velupillai has spared no effort to open the sluice gates on these intellectual flood waters, the self-styled praetorian guards of economic theory have succeeded far too long in barricading our science.

The book begins with the premise that in so far as an intuitive notion of calculation or of an algorithm can be formalised, the current consensus favours the Church-Turing thesis. The latter states that the models of computation considered so far, prominent among these being that of the Turing Machine (TM, for short), have all been shown to be equivalent to the class of general recursive functions. Following from the Church-Turing thesis, a number of decision problems can be proved to be algorithmically unsolvable. As these impossiblity results, typically referred to as incompleteness or undecidabilty results, are of a logical kind aimed at the avoidance of *reductio ad absurdum* in calculations, they cannot be overcome by material advances in computer technology. A well-known decision problem, Hilbert's Tenth Problem, for which no uniform decision procedure exists for its solution is the class of diophantine equations, viz. polynomials with integer coefficients.

Velupillai's main thesis is that the pragmatic science of Economics which focuses on efficient human action must in principle be founded on recursion theoretic results that delimit what can and cannot be implemented by algorithms. We are faced with the prospect that what many hold to be 'solid' micro-foundations of rational economic choice in the traditional Bourbakian formalism of economic analysis are *not* in fact algorithmically implementable. The upshot of these limits to formalist calculation has been the growth of alternative approaches which to the jaundiced eye may seem like a spin-fest of weasel words such as *adaptive, evolutionary, behavioural, inductive, boundedly rational,* etc. Velupillai offers an interesting antidote to a fallacious distinction between the perfectly rational and the boundedly rational. The latter is held to be a less than perfect approximation of the former, typically on the grounds of limited information processing ability of the brain. Velupillai considers a simple standard neuronal unit of McCullock and Pitts which has the power to compute anything that a TM can (page 56) and then assumes that a perfectly rational agent cannot be more computationally powerful than a TM. Even a simple evaluation of the truth value of X or else Y type proposition which is necessary for categorisation of objects of choice, involves calculations of the neuronal network that are non-linear, of over dimension three, along with the assignment of integer or rational valued weights as in the solution of a diophantine equation. As no uniform solution procedures exist for this, even the perfectly rational agent qua TM will arrive at a solution in an adaptive way using trial and error techniques. Thus, the often cited distinction between perfect and bounded rationality is a specious one.

The book portends that the powerful 'positive' contributions of recursive function theory will be in the areas of dynamical market equilibrium conditions and equally significantly in the modelling of randomness and information. Velupillai's vision of a unified theory of micro-economic rationality and macro economic dynamic resides in what he calls *computational universality*. By the latter is meant that rational agents have the computational capacity of a universal TM that can simulate the behaviour of other such machines. This notion also coincides with the Chomsky-Wolfram schema given in the pioneering work of Albin (1997). Data from dynamical systems with computationally rational agents (CRA, for short) will not generate trajectories equivalent to limit points, limit cycles, and even just strange attractors. Computational universality will result in irregular 'structure changing' dynamics (*ibid.* p. xiv) well known in economic systems. A full demonstration of this is put off by Velupillai to a sequel to the book though he has discussed this elsewhere in the context of his mentor Richard Goodwin's view that structure changing 'dislocating innovations' (Hahn, 1996) of Schumpeterian business cycle *are* the consquence of capitalist growth. Decades of theorising based on the Ramsey/Solow type growth models that had cut loose from business cycle theory has been an unfortunate misreading of capitalist dynamics and certainly not in keeping with CRAs.

Inductive inference that is recursion theoretic with 'no need to rely on any *apriori* notion of probability' (page 68) could revolutionise our view of randomness and information. The pragmatism of defining randomness in data by some measure of the difficulty of extracting patterns by a general search program must strike one as more far-ranging than tests of lack of serial correlation that have dominated analyses, for instance, in financial economics. Indeed, an alternative formulation of the efficient market hypothesis along the lines of the complexity of algorithms for pattern extraction from asset price data has been proposed by Chen and Yeh (1997). In chapter 5, Velupillai gives a masterful discussion of the Kolmogorov-Solmonoff-Chaitin thesis on algorithmic complexity with the latter defined in terms of the shortest program needed to generate a given binary sequence. On inductive learning within a Bayesian setting, the Solmonoff universal prior is recommended for its least costs of inference compared to other arbitrary priors.

In chapter 7, Velupillai draws on early work by M. O. Rabin on arithmetical games in which winning strategies exist but for which no effective procedure can equip the players with these. Notwithstanding the well known riposte that because white can checkmate black in a number of moves from a given configuration is no reason for not playing chess, market games are effectively redundant unless this non-trivial property of the absence of an algorithmic decision procedure for winning strategies holds. This is in keeping with Velupillai's earlier insight that no arbitrage market equilibria must imply undecidable system dynamics.

For a book placed at the 'confluence of events that led to the fundamental results of Gödel, Kleene, Church, Post, and Turing' (page 8) there is, nevertheless, a major omission. There is no discussion of the consummate skill with with Gödel (1931) exploited the deep self-referential structure in meta analysis to construct the undecidable proposition from the simple but ubiquitous structure of opposition or falsification (an insight he drew from the Liar, see Markose (1999)). Gödel's undecidable proposition as Emil Post subsequently showed belongs to a domain of the set whose members cannot be exhaustively listed by any algorithm. Such *ex ante* non-listable objects which merit the epithet of innovations in the context of the Gödel result has the set theoretic property that Post calls productive in that the formalism can never be complete. That CRAs can by *contra* positions exit from any given listable formalistic structure is of utmost significance to the above mentioned rivalrous and dislocating nature of capitalistic innovations. Velupillai is ever mindful that it is precisely such epochal results on the limits of calculation that make creativity centre stage in economic dynamics. This book is not, therefore, for those who seek to see out their sinecure unchallenged by new vistas.

SHERI MARKOSE

University of Essex

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Nonparametric Econometrics By PAGAN (ADRIAN) and ULLAH (AMAN). (Cambridge and New York: Cambridge University Press, 1999. Pp. xviii+424. £47.50 hardback, US $74.95 hardback, £17.95 paperback, US $29.95 paperback. ISBN 0 521 35564 8, 0 521 58611 9.)

This book offers an impressive survey of the nonparametric and semiparametric econometrics literature. Given the tremendous amount of theoretical work done in this area, such a survey is an inherently difficult undertaking but at the same time all the more valuable to an interested researcher. The authors succeed in providing a solid and thorough theoretical treatment of various estimation methods. The book consists of two parts, with the first part (up to chapter 4) covering 'purely' nonparametric statistical methods and the second part covering semiparametric methods for models commonly estimated in the economics literature. ('Semiparametric' means that *part* of the model is specified parametrically).

The chapters on nonparametric estimation are quite thorough and will probably be the ones most used by applied economists. The authors adopt the familiar approach of first discussing nonparametric estimation of density functions (chapter 2) and then regression functions (chapter 3). Their treatment of the topic differs from other treatments in several respects, however. First, several nonparametric estimation techniques ranging from recursive kernel estimation to local likelihood estimation are discussed (with theoretical details reserved primarily for kernel and series estimators); though some of these techniques are rarely used in practice, the discussion of different approaches gives a better picture of the challenges posed by nonparametric estimation. Second, a full chapter (chapter 4) is devoted to estimation of derivatives, a topic which is oftentimes treated too briefly (especially for economists, who often want to estimate the effects of one variable on another variable). Aside from detailing estimation methods, there is a nice comparison between pointwise derivatives and average derivatives (i.e., derivatives averaged over a region of a variable). Third, the focus is not only on estimation but also on testing. For instance, tests for equality, symmetry, and independence of density functions may be very useful for applied research but are not discussed in common references on the subject (including Silverman (1986) and Härdle (1990)). The section on nonparametric specification testing (Section 3.13) is quite extensive, owing to the large literature on the subject. Given the large number of references, this section would have been more reader-friendly with some subsections; on the other hand, as they do throughout the book, the authors always provide sufficient overviews and references for the interested reader to pursue a given topic.

The chapters on semiparametric estimation cover a wide range of models including simultaneous-equation, discrete-choice, selection, and censored-regression models. The associated semiparametric estimators are discussed (with fewer theoretical details than the nonparametric chapters) and any relevant Monte Carlo evidence is nicely summarised. The chapters on discrete-choice and censored-regression models are probably most relevant for practitioners (and would fit nicely into an advanced graduate econometrics course).

The book's theoretical material, according to the authors, is at the level of a second-year graduate econometrics course. Readers without such a background will probably find this book too difficult to be of much value. For those with a solid econometrics background, however, the authors present the material in a way that allows the reader to decide how deeply to delve into the theoretical details. There are step-by-step proofs of many theoretical results (especially those involving kernel estimation), but the text provides enough intuitive explanation to allow one to skip these details. Moreover, the authors make a point of separating descriptions of estimators from discussions of their theoretical properties. Wherever possible, the authors introduce a nonparametric or semiparametric method by first discussing its parametric counterpart. For instance, in their discussion of the binary-choice model, the authors give a brief review of the parametric log-likelihood estimator, non-linear least-squares estimator, and efficiency bound before introducing their semiparametric analogues. Thus, even in cases where the theoretical details are not fully developed, the intuition provided by the comparison with the parametric case is valuable.

After reading this fine book, one is left wondering about the future of nonparametric econometrics in empirical work. Applied economists have been slow to adopt techniques that have been around for many years, in part due to the lack of 'canned' routines in econometrics software. Nonparametric estimation of density and regression functions has become a bit more common, but part of the reluctance to use these methods may stem from the dependence of estimates on the choice of 'smoothing' parameters. Despite several decades of research, there is no real consensus on how to choose these smoothing parameters. In fact, even though they spend many pages discussing the alternative methods for making this choice, the authors seem to advocate 'eyeballing' graphs (page 120) in order to make a reasonable choice. Whether many of the semiparametric estimators find their way into applied work remains to be seen, as evidenced by the small number of existing applications in the literature (with none even referenced for simultaneous-equation estimation). Econometricians will no doubt continue to develop and study more semiparametric estimators and testing procedures in the years to come, but perhaps more focus should be shifted to examining the empirical relevance of these theoretical advances.

JASON ABREVAYA

University of Chicago

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Integrating Financial Markets in the European Union. By LEMMEN (JAN). (Aldershot and Lyme, NH: Edward Elgar, 1998. Pp; xii+222. £49.95 hardback. ISBN 1 85898 730 X.)

In this book Jan Lemmen provides an encyclopedic investigation of capital mobility within the European Union using data from the 1970s to the early 1990s. The first part of the book estimates the degree of financial integration within the European community using three measures of integration: 1) deviations from interest rate parity; 2) correlations between saving and investment rates; 3) Cross-country correlations in consumption growth, based on Euler equation restrictions. In addition, Lemmen provides a nice discussion of the relationships between the three measures. The principal conclusion is that covered interest parity holds for most EU countries, but real interest parity does not, primarily because of exchange rate risk. The results for the other two measures imply less integration, although integration appears to be growing over time.

The second part of the book examines reason for the divergence from interest parity: 1) capital controls; 2) withholding taxes on foreign residents. The author concludes that within the EU, realised inflation rates, government instability, and gross capital formation help to explain onshore-offshore interest rate differentials, the author's proxy for the intensity of capital controls. The chapter on withholding taxes is different from the others since it is concerned with explaining how EU versus US or Japanese interest rate differentials are affected by nonresidents withholding taxes.

The book is valuable because it applies all of the standard tests of financial integration to the EU countries up until the early 1990s. At times, the description of results is tedious because of many paragraphs that enumerate results displayed in tables. In addition, it would be helpful if there had been more discussion of the relationship between the results for the three different measures. (e.g., interest rate differentials between the Netherlands and Germany are the smallest, yet the hypothesis of financial integration is frequently rejected based in chapter 4.)

In addition to the standard tests, Lemmen develops a number of variations. Unfortunately, in each case, I was unsure how to interpret the results of his tests. I will illustrate my problems with the empirical work in the chapter, 'The Fundamental Determinants of Financial Integration', which later he admits is really about the intensity of capital controls. He argues that previous work suffers from the fact that dummy variables or indices have been used to measure capital controls, and these are imperfect measures of the intensity of capital controls. He proposes that onshore-offshore short-term interest rate differentials are better measures of indicating the severity of the controls. Consequently, he employs a pooled cross section of five-year average values for 11 EU countries and tests for the importance of various economic and institutional factors in explaining the average interest rate differentials. From these regressions he concludes that realised inflation rates, government instability and gross capital formation are the most important determinants of the interest differential.

My first problem relates to the fact that significant interest rate differentials require capital controls, but capital controls may also exist with no interest rate differential. So for example, in Austria from 1974–78 the interest rate differentials average out to be close to zero, whereas they average to something over 1% in the 1984–88 period. The logic of Lemmen's regression equation is that import capital restrictions increased between these two periods, in spite of the fact that all of the IMF's indices of legal measures of capital controls are unchanged. Were capital controls more intense in the latter period or were existing controls more binding? In the absence of more direct evidence, I prefer the latter interpretation.

My second problem is that the intensity of the capital control measure takes on both positive and negative values. Lemmen interprets the negative differential as increasing capital export restrictions, and the *a priori* signs for most of the variables are justified on the basis of a propensity to impose capital export controls leading to a negative differential. While the majority of the observations are negative, there are a number of positive differentials and the equation is being fitted to all the observations. While his dependent variable is continuous, he is estimating two relationships with one equation: The intensities of capital export and import restrictions.

Finally, I have trouble interpreting the correlations that Lemmen finds as causal factors. For example, do high realised rates of inflation cause capital controls, or does the existence of capital controls allow a country to run a monetary policy that leads to higher realised rates of inflation. The latter interpretation seems more plausible.

While I had some concerns with parts of the empirical work, I still found Lemmen's book a valuable resource on financial integration in the EU. His finding that exchange rate risk was the primary cause of real interest parity deviations in the pre-EMU period has fascinating implications for the state of financial integration post-EMU.

GEOFFREY WOGLOM

Amherst College, USA

Economics, Entropy and the Environment: The Extraordinary Economics of Nicholas Georgescu-Roegen. By BEARD (T. RANDOLPH) and LOZADA (GABRIEL A.). (Aldershot and Lyme, NH: Edward Elgar, 1999. Pp. viii+155. £45.00 hardback. ISBN 1 84064 122 3.)

Nicholas Georgescu-Roegen was one of the most brilliant economists of the 20th century, but that does not imply that contemporary economists generally appreciate that fact, or agree as to the nature and significance of his contributions. This short volume by Beard and Lozada is the first comprehensive attempt to summarise his entire *oeuvre*, and provide an assessment for modern readers unfamiliar with his work. Usually such exercises tend towards hagiography or worse, but in this case, the authors provide a balanced evaluation of Georgescu's strengths and weaknesses – perhaps a little *too* balanced, since the average economist might come away from this volume wondering what all the fuss was about.

The book starts with a spare biography, but the substantial chapters revolve around Georgescu's philosophical position, his contributions to 'orthodox' neoclassical micro theory from the 1930s to the 1950s, and his big break around 1970 in advocacy of the central importance of the 'entropy law' to economics in general, sustained till his death in 1994. The authors should be commended for providing one of the clearer summaries of the second law of thermodynamics found anywhere in the popular science literature, and this lends force to their attempt to argue that Georgescu got some of the physics wrong (misunderstanding the relationship between matter and energy in thermodynamics, opposing the legitimacy of statistical mechanics, appealing to Helmholtz free energy as an economic measure), but in the final analysis, was broadly correct about the dire consequences of an underappreciation for entropy in neoclassical economics. Nevertheless, their primary quest is to figure out why someone who was so perceptive and so well-known has been essentially ignored by the economics profession at large. For anyone marginally more sophisticated than the credulous believer in the efficient marketplace of ideas, this is indeed the interesting question. Being right is no guarantee of being heard, much less being understood.

The authors proffer a number of reasons for the neglect of Georgescu, but most are tangential to the content of his project. They compound this omission by striving to rephrase some of his ideas in language more amenable to the modern orthodoxy of game theory and information economics, but I think this only muddies the waters. The one substantial claim they make is that Georgescu regarded the penchant for classical mechanics as a paradigm of explanation as the fatal flaw of modern economics, and therefore decided to use better physics to counteract outdated physics through stress on the entropy law, dialectical models, and evolution. The authors regard this choice as a tactical error, claiming that for most 20th century economists, physics was 'epistemologically irrelevant'.

If they were indeed correct about this, then it would seem their own volume is equally guilty of the same error, given that it devotes so many of its pages to explication and critique of the uses of thermodynamics in economics. But there is a better explanation, one that ties together the more 'orthodox' work on price theory with Georgescu's later crusade to bring thermodynamics to the economic masses. I would argue that Georgescu can be read as the Rip van Winkle of economics, intimately participating in the American development of neoclassical microeconomics in the 1930s, then absent for the crucial decade of the 1940s, only to return to America to discover that neoclassical price theory had taken a turn he found curious, and even repugnant, hardening into a Bourbakist orthodoxy around the Arrow-Debreu model. He spent the 1950s and 1960s puzzling over the causes of this unfortunate turn of events, and decided that the penchant for 'mechanistic dogma' was a major motivation; he hoped harping upon thermodynamics might convince at least some of the faithful that their 'science' was not so modern as they initially thought. But the joke was eventually on Georgescu, because the orthodoxy *did* come to some accommodation with thermodynamics in the later 20th century, not through explicit consideration of the irreversibility of production processes and the dissipation of matter, but rather through the Shannon information concept, an entropy analogue which he himself despised, and the present authors also disparage. They should have realised that their repeated insistence that Georgescu neglected information considerations was simply the obverse side of the coin of his rejection of statistical mechanics; and that the treatment of 'information' as a fungible commodity was a reflection of its reification within physics and computer science in the postwar period. Hence Georgescu was retailing a version of entropy which had gone out of fashion; probably the only reason he drew so much attention in the 1970s was that his theme of irreversibility and dissipation coincided with the oil crisis. It is no accident that the area in which he is now most fondly remembered is ecological economics, and not in fundamental production theory or price theory.

The authors realise that Georgescu was a much deeper thinker than someone who merely reflexively complained about the disappearance of scarce natural resources; but in the end, they seem unable to satisfactorily explain why that was. The key, I believe, is that he provides a glimpse of where neoclassical economics might have gone, had it not been so thoroughly diverted towards game theory, decision theory, and operations research during WWII and the Cold War. Thus we can understand the authors' lament that Georgescu looks so 'old-fashioned' in some respects, but so otherworldly and inspiring in others.

PHILIP MIROWSKI

University of Notre Dame

The Political Economy of Transition: Coming to Grips with History and Methodology. By VAN BRABANT (JOZEF M.). (London and New York: Routledge, 1998. Pp xvi+559. £75.00 hardback. ISBN 0 415 16946 1.)

Jozef van Brabant of the United Nations Secretariat in New York has written extensively on the former centrally-planned economies of Europe, concentrating on their foreign trade relations and attempts at integration. In this book he aims to provide an impartial and fairly comprehensive overview of the important policy issues in the transition from centrally-planned to successful market economies. His recurrent theme is scepticism towards the 'naïve optimism' of the 'Washington consensus' and an insistence that the 'straightforward application of experiments set up in ivory towers' is 'at best problematic'. He concludes with the message that policy formation requires 'a good dose of pragmatism'.

The book covers a broad sweep of themes with an outline of the historical background and discussion of key areas including stabilisation, privatisation, social policy reforms, the role of the state in the economy generally, and international policy issues. Unfortunately, however, the book remains a discussion of policy options without a serious assessment of what has actually happened. It would have made sense at the start of the transition, but it is less interesting today when evidence is available which could confirm some of van Brabant's scepticism while in other cases pointing to the need for him to revise his views.

Much of the discussion is at a very generalised level, hedged with numerous caveats and admissions of limited knowledge, with hard evidence on specific example rarely quoted. Themes are often discussed on the basis of basic economic theory or evidence from advanced market economies while plenty of material exists on what has happened in transition economies that would appear more relevant. Some of the discussion, particularly of definitions of terms and concepts, reflects the author's own preferences without adding much to debate. Space is also wasted on repetition, for example with whole sentences repeated in a generalised discussion of industrial policy that appears twice in the book.

The lack of hard evidence is justified early on with the claim, backed up by an anecdote, that statistics are so unreliable as to be of little use. The book contains only two tables of figures, both from UN sources. Elsewhere, however, the author resorts to generalised statements that clearly depend on the existence of figures – for example indicating that several countries face problems from current account deficits – but fails to give the easily available precision and detail. This annoying vagueness is coupled with a discursive style that often assumes detailed knowledge of events in particular countries.

Set against the stated objective of providing a comprehensive account, showing the distinctiveness of the countries concerned as a whole and their differences from each other, the book is not very successful. It cannot be objective without attempting a more rigorous confrontation against existing evidence. The limited amount of hard evidence obscures distinctions between countries and indeed few comparisons are attempted. Sometimes generalisations clearly refer only to some cases, when a serious comparison would be interesting. An example is the criticism of transition economies for excessive early devaluations, when the extents of devaluation actually varied considerably, even among the more advanced transition economies of east-central Europe.

Nevertheless, many of the author's judgements would stand up in the light of subsequent evidence. Parts of his analytical framework are helpful, with a useful table distinguishing between those measures that need to be adopted quickly, in which he includes monetary reform, and those for which time is required, such as large-scale privatisation. He thereby disposes of the 'pointless debate' over 'shock therapy' versus 'gradualism'.

The author's most surprising position is his scepticism over the benefits of FDI. He seems to have stuck to an early view that faith in multi-national companies was one more piece of the damaging over-optimism engendered by faith in simplistic solutions. He even describes means used to attract inward investment as 'dirigistic' and suggests that, far from bringing modern technology, foreign companies have come to exploit monopoloy positions, with the automobile industry quoted as an example. There are sectors in which FDI may bring little positive benefit, but it is very difficult to imagine the development of modern automobile and electronics industries in particular without inward direct investment. It is clear beyond any serious doubt that multinational companies have already brought substantial benefits to the host countries, and van Brabant effectively admits this with references to their share in Poland's exports. Some more precise facts and figures would have provided a better basis for discussion and more interesting conclusions on this, and many other points.

MARTIN MYANT

University of Paisley

The Economic Geography of Production, Trade, and Development. By JUNIUS (KARSTEN). (Bremen: Institute for World Economics, 1999. Pp. xi+182. ISBN 3 16 147251 9.)

At first glance, the timing of this slick volume was not exactly perfect. Publishing a book on the New Economic Geography in the same year that Masahisa Fujita, Paul Krugman, and Tony Venables produce the ultimate summary of their activities in the field over the last decade (*The Spatial Economy*) would almost inevitably seem to provoke unfavourable comparisons. As the plot of *The Economic Geography of Production, Trade, and Development* unfolds, these worries gradually disappear.

Admittedly, the first (theoretical) part of the book offers relatively little material that has not been satisfactorily treated elsewhere. The author introduces his 'prototype model', a combination of familiar models by Krugman and Venables, which he then augments to include congestion costs. The model serves to identify both the circumstances under which large-scale agglomerations are likely, and the forces that tend to destroy such instances of concentration. Even here the book is well written, and it offers a good introduction to the main ideas of the New Economic Geography. Also, the last section of the theoretical part adds an interesting twist to the familiar literature. Junius introduces a development model producing an inverted U-curve relation between economic development and geographical concentration: as the development level increases, production first becomes more concentrated, then less concentrated.

The second part of the book provides material that will be warmly welcomed by many students of economic geography. Here, the author attempts to link the recent theory to empirical observations. He proceeds in two steps. He starts by investigating the assumptions of typical Economic Geography Models. These models rely crucially on two pillars that not everybody believes to be very solid, at least not nowadays. They require increasing returns to scale (internal and external) and substantial trade costs. Junius therefore supplies evidence on the importance of these forces, leading him to conclude that they still matter. As direct evidence on New Economic Geography predictions is still relatively scarce, this kind of indirect approach appears valuable in spite of its obvious limitations. However, the author does not stop there: he moves on to add a more direct test of one particular result, the above-mentioned U-curve relationship between development and concentration, which seems to support the theoretical analysis.

Junius gives a convincing presentation of some important aspects of the New Economic Geography. It is neither a complete survey of the literature, nor does it claim to be one. Readers in search of a comprehensive introduction to the relevant theory instead of a brief overview should rather consult Fujita *et al.* (1999). Two other groups of potential customers, however, will find *The Economic Geography of Production, Trade, and Development* helpful. First, beginners in the area who would like to see a little bit more about the theoretical developments than Krugman (1991) offers, but are reluctant to invest into the deeper treatment of Fujita *et al.* will find some valuable information here. Second, and more importantly, for anyone who would like to get an idea about the relation between facts and theory, beginner or not, Junius provides a useful starting point. In brief, *The Economic Geography of Production, Trade, and Development* and *The Spatial Economy* are complements rather than substitutes.

ARMIN SCHMUTZLER

Socioeconomic Institute, Zurich

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Why Wages Don't Fall During a Recession. By BEWLEY (TRUMAN F.). (Cambridge, Mass. and London: Harvard University Press, 2000. Pp. viii+527. £34.50 hardback. ISBN 0 674 95241 3.)

The Adjustment of Wages: A Study of the Coal and Iron Industries of Great Britain and America. By ASHLEY (W. J.). (London, New York and Bombay: Longmans, Green and Company, 1903. (No longer available.)

Economics, like palaeontology, is a science which depends for the most part on the interpretation of an historical record. Just as palaeontologists struggle to unravel the significance of fossils whose survival is the consequence of a random mud-slide or inundation, so does most useful economic information arrive fortuitously on the economist's desk as the unintended consequence of some 'natural experiment'. The plausibility of hypotheses about the nature and dynamics of economic behaviour is assessed with imperfectly recorded evidence. The system being studied incorporates endogenously generated institutional arrangements, so that what might appear to be a natural experiment at first sight, turns out not to be. Most empirical economists are acutely aware of this. Some have attempted to mimic physics by conducting experiments under controlled conditions. With a few notable exceptions, these attempts have achieved little except a realisation that the control of conditions confronting economic agents is far from simple, and so we are stuck, for the most part, with our economic fossil record.

In his important book, Professor Bewley begins by rejecting much of recent applied economics as it relates to one of the central issues of post-war economics – the apparent downward inflexibility of real wage rates. He claims that either the work has not been done, or that the data necessary to do it are not available. Worse, the theories that it attempts to test are often observationally equivalent and cannot be tested anyway. His solution: Go back to the apparent source of decisions about wages – human resources managers and union officials – and ask them why, when sales and output prices fall, they don't, can't or won't cut wages; then reformulate the theory of wage setting in the light of their responses.

While one can have a lot of sympathy with an economic theorist's frustration at the impotence of economic theory when confronted with a central issue of public policy, the proposed solution seems odd. The theorists and empirical workers who have written so much about wage flexibility in the last fifty years (much of it referred to in Bewley's book) have relied heavily on contemporary data – much the same experience as informs human resources practice. What extra insight might one expect to gain by asking the question of another group of similarly-informed people? And indeed, the new theory outlined in chapter 21 of the book turns out to have a lot in common with some existing models – especially Akerlof's gift exchange model and Rotemberg's model of human relations in the workplace, both of which were inspired by similar misgivings about the excessively individualistic posture of much economic theory.

It is harder to have sympathy with Bewley's apparent ignorance of the experience of the past (cf. page 193). Contracts in which employers and unions agreed that wages should fall when output markets fell were common in both Britain and the USA in the 19th century. Their effects caused Tinbergen some difficulty in the construction of his pioneering model of the UK economy (Tinbergen (1956)). The contracts were resoundingly rejected by the workers of the early 20th century. The fact of that rejection forms the background for much of industrial relations history of the early 20th century, including, for example, the 1926 General Strike in Britain.

Professor Ashley can be excused for not knowing that history. Time's arrow points in one direction only. He was, like many modern economists and politicians, an enthusiast for flexible wages and eagerly advocated the development of institutions that enable them. He describes how sliding scales (which indexed the wages of workers to the price of output) flourished in British and US coalmining and iron and steel production during the four decades prior to the publication of his book. When Ashley was writing somewhat more than 1/8th of the British labour force had its wage rates fixed by sliding scale determined by collective bargaining. The sliding scale was apparently an American innovation: Ashley dates it to an agreement between The United

Sons of Vulcan and an association of Pittsburgh iron masters in 1865. Hicks (1930) dates the introduction of the first collective bargaining institutions to about the same time, in the Nottingham lace factory of Anthony Mundella.

Ashley was an enthusiast for these innovations, as were many of his contemporaries. With hindsight, one must conclude that the enthusiasm was misplaced, since sliding scales are not used widely in modern economies. (They still survive in Montana's nickel mining industry, where wages are indexed to the price of nickel in London.) He was not however, unaware of the difficulties encountered with them, and his book is a critical discussion of their operation. Interestingly enough, his insights echo in the reasons given by Bewley and his interlocutors for the reluctance to reintroduce something similar.

A comparison of these two books leads me to suggest the following answer to Bewley's title question: Wages don't fall in a recession because there is a social consensus that they should not do so. The sources and reasons for that social consensus are too complex for a complete discussion here, but an important element of them is revealed preference (by employees) for employment risk over risky income stream: 'Given a minimum, one of the strongest of the miners' current arguments against the sliding scale passes away. The chief popular objection to it – to quote a South Wales miner's phrase in conversation with me recently – was that "the confounded thing had no bottom"'. (Ashley, page 54.) Constructing a social preference is, of course, not a trivial problem, but some of Bewley's interviewees seem to have understood clearly the outline of how this one might work. Crucially, employers have to deal with the discontent of employees; they don't have to deal with the discontent of ex-employees: 'It's easier to lay people off than to cut pay. It's like soldiers. If the guy next to me is shot, I say, "Wasn't I lucky it wasn't me"' (Bewley, page 184) and '. . . The choice is between cutting pay and employees. I say, 'Cut bodies'. That is controlled surgery. If we cut pay, they will leave, or the best will . . . because they are pissed off'. (Bewley, page 185)

There are, of course, many avenues by which the empirical study of this consensus might proceed. Bewley suggests several in the final chapter of his book. I suggest two more. The first is to examine the history of the decline of the sliding scales, and their replacement by fixed wage rates. The second is to ask why the sliding scales were almost entirely restricted to coal, iron and steel, and cotton production. The contemporary literature and a large number of books and papers in economic history already have dealt with much of the first task. I do not know of any systematic treatment of the second.

I asserted above that Bewley's is an important book. This is true for several reasons: he provides a lucid and comprehensive exposition of the theoretical literature, the interviews constitute an interesting and unusual source of data, and the brief exposition of his model of wage setting and morale shows a possible way forward. The book is also important because it clearly articulates a crisis of confidence in modern economics. A leading theorist suddenly finds it necessary to take on the entire pack of empiricists because he believes they are missing something that he can find by taking lunch with a few hundred Connecticut businessmen.

The crisis of confidence is real, but I question whether the solution lies in lunchtime chat. A science that fails to use the full array of data available to it deserves a crisis of confidence. We would think poorly of a palaeontological profession which refused to inspect any of the fossil record prior to dinosaurs, but that is exactly what many economists do, including Bewley.

JOHN TREBLE

University of Wales, Bangor

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Competitiveness Matters: Industry and Economic Performance in the U.S. Edited by HOWES (CANDACE) and SINGH (AJIT). (Ann Arbor: University of Michigan Press, 2000. Pp. 207. £31.00 hardback, US $49.50 hardback. ISBN 0 472 10983 9.)

What is competitiveness and does it matter? In the view of most Governments and many academics competitiveness is key to growth, prosperity, and rising living standards. For Krugman (1994), on the other hand, it is a 'dangerous obsession'.

In this interesting and important book, Candace Howes and Ajit Singh argue that competitiveness does indeed matter. Debates around the issue of competitiveness reflect the earlier literature around deindustrialisation, to which Singh was of course one of the major contributors. In a series of important papers published during the 1970s and 1980s Ajit Singh, following the work of Nicholas Kaldor, argued that Britain's poor overall economic performance was due primarily to the relative failure of its industrial sector in the global economy. The key was to have an industrial sector which, 'given the normal levels of the other components of the balance of payments' not only meets the needs of consumers but 'generates sufficient net exports to pay for the country's required level of imports at socially desired rates of employment, output growth, and exchange rate, both in the short and long runs' (Singh, 1977). A similar definition can then be used to consider what is meant by an economy's 'competitiveness' – it means that the relative productivity levels of the traded goods sector are sufficient to allow the economy to operate at high levels of employment without running into unsustainable balance of payments deficits.

For Krugman there is no such thing as unsustainable balance of payments deficits, since an adjustment of nominal exchange rates will allow the price mechanism to achieve an equilibrium solution. The problem of course is that while the price mechanism may work well in theory – provided one maintains a sufficiently simplistic, pre-Keynesian theoretical outlook – in practice things are rather different.

Firstly, the exchange rate may fail to adjust. Certainly there have been cases in Britain's recent economic past when not only the exchange rate remained at an uncompetitive level rather than adjusting downwards, but those of us who pointed out that the exchange rate needed to adjust downwards were dismissed as not understanding that exchange rates no longer mattered in a world where most trade is internal to transnational corporations, or that any devaluation would result in interest rate rises to compensate for the loss of confidence, hence making matters worse. So there is always the danger that economic theorists, commentators, and policy makers will defend an over-valued exchange rate, either explicitly by saying that it no longer matters, or implicitly by arguing that nothing can be done to buck the markets.

Secondly, even if relative prices were to always adjust instantaneously and costlessly, there is more to life – and the economy – than prices. Vitally important to international trade are all sorts of non-price factors. If firms lose competitiveness in these areas then price adjustments alone may prove insufficient to rectify the problem.

These points are all made and discussed in detail by Howes and Singh in an excellent and comprehensive introductory chapter. The book then includes an interesting collection of contributions around these themes.

Particularly good are the chapters by Ajit Singh on 'The Anglo-Saxon Market for Corporate Control: The Financial System and International Competitiveness' in which he makes the case for reforming corporate governance arrangements to protect against the damaging effects of short-termism, and by Ann Markusen on 'Can Technology Policy Serve as Industrial Policy?'

Markusen argues convincingly that while technology policy can play a valuable role in boosting an economy's competitiveness, this cannot be achieved by technology policy alone. Rather, a package of complementary policies are required on both the supply and demand side. Far from being a thing of the past, replaced by cutting edge supply side policies, demand management is an important mechanism for ensuring that firms have the necessary confidence to invest in technological innovation. Similarly, in a separate chapter Steinmueller stresses the development of interdependencies across firm and industry boundaries, where technology policy can play an important role. This is certainly what my colleague Michael Kitson and I found, using data from the ESRC's Centre for Business Research; innovative firms could be distinguished from non-innovative ones in a number of ways, including by the degree of co-operation entered into with other companies and organisations – the more co-operative being the more innovative (Kitson & Michie, 2000).

Space prevents a discussion of all the contributions, but together they amount to an important contribution to the literature on the US economy – on which the contributors focus. But as indicated above, the topics analysed in the book have far wider significance, both for other countries and also for economic theory.

JONATHAN MICHIE

Birkbeck College

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The New Economic Criticism: Studies at the Intersection of Literature and Economics. Edited by WOODMANSEE (MARTHA) and OSTEEN (MARK). (London and New York: Routledge, 1999. Pp. xvii+437. £19.99 paperback. ISBN 0 415 14944 4, 0 415 14945 2.)

Woodmansee, and Osteen, both professors of English, have bravely ventured into the largely unexplored territory shared by an emerging group of literary and cultural critics who use their accustomed rhetorical methods to discover buried metaphors and fictions in economics, and economists who participate in an essentially parallel movement and uses similar methods towards the same ends. As an economist, I am not sufficiently versed in literary work to presume to offer an opinion as to how much progress literary scholars have made in exploring this territory. As an economist I do, however, venture to say that not only has such progress been minimal in the profession at large, but that it has been modest even among members of the dissident group to which I belong. One of the problems is that the authors of this book address issues of political economy, while the economics profession today is very largely concerned with their discipline as the science of choice.

This conclusion is not intended to suggest that the enterprise is necessarily futile. This situation might be hopeless if thoughtful economists had examined this body of work and had found it lacking. Instead, the great majority of mainstream economists have chosen to ignore it, just as they have generally neglected the work of dissident scholars. It may be that if they ever turn their attention to this body of work, they will find that it does make a contribution. Although feminist economists mainly focus on policy related issues, they are generally sympathetic and many take some interest in the relationship between rhetoric and economics, notably Donald/Deirdre McCloskey (e.g. 1985) and Diana Strassmann (e.g. 1993).

This volume is an extremely ambitious enterprise and, like so many ambitious works turns out to be of very uneven quality. The comprehensive introduction by the editors provides a useful overview of the remaining chapters, even though it can hardly be recommended as easy bedtime reading. For one, some of the language used is sufficiently obscure that I kept a large dictionary nearby while reading it. Anyone not familiar with terms like 'tropes', 'homologies', and 'syntagmatical', may want to do the same.

As for the 22 individual chapters, it is impossible to include even a brief critque of each in a review. Suffice it to say that they cover a wide variety of topics, as indicated by the titles of the seven parts: Language and money; Critical economics; Economics of the irrational: Economic ethics: Debts and bondage; Economics of authorship; Modernism and markets; and Critical exchanges. They range in length from 6 to 25 pages, and in quality from some that offer interesting insights to others that leave one in doubt as to how they are related to either literary or economic criticism, or for that matter, to rhetoric.

Among the chapters that present food for thought is Marc Shell's essay, 'The issue of representation', which raises the question why particular symbolic images and phrases, mainly patriotic and religious, are found on bank notes and coins. They even include the assertion 'IN GOD WE TRUST'. Similarly thought provoking is Richard T. Gray's 'Buying into signs: money and semiosis in eighteenth century German language theory', which discusses the frequent analogies between language and money. Examples are Quintilan's admonistion to spend words as carefully as money, Ovid's remark that words, like coins, are minted by public authority, and Nietzsche's comment that words, like coins, have lost their impact due to overcirculation. Finally, there is Elaine Freedgood's paper, 'Banishing panic: Harriett Martineau and the popularization of political economy'. There the Victorian statistician William Farrar is cited as an early example of defenders of the establishment who have expressed the hope that 'a better understanding of economic laws would quiet the growing unease of the middle and upper classes and the growing unrest of the laboring classes' (page 210). Some of the other chapters, however, leave one puzzled as to what we are to make of them and neither raise nor answer challenging questions.

The editors have taken on a formidable task in trying to find common ground between literary and cultural studies on the one hand, and economics, on the other hand. Among the obstacles they face is that literary critics know little about economics other than Marx and often use terms with little awareness of how they are used by economists. At the same time, economics is increasingly ahistorical and dominated by what is here termed 'scientism', as even critics from within the field acknowledge. As is true in the other sciences, the focus is often entirely on use of the scientific method to get at the truth, while the question as to which of these truths are important and useful in addressing the crucial problems facing the world is ignored. This makes a genuine dialogue between these two groups rather difficult.

Some readers will find the nuggets of wisdom sprinkled throughout this book rewarding; many others, however, are likely to be frustrated by the lack of logical organisation or cohesiveness as well as the unnecessary use of obscure terminology. In any case, this book is not likely to be the final word concerning the issues it raises, for these issues are well worth exploring further.

MARIANNE A. FERBER

University of Illinois

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Britain's Productivity Performance 1950–1996: An International Perspective. By O'MAHONY (MARY). (London: NIESR, 1999. Pp. vii+189. £75.00 paperback, ISBN 0 9526213 5 5.)

Following the revival of theory and modelling of economic growth since the mid 1980s, the gap between theoretical and empirical work in this field of economics has been revealed more clearly than ever before. Statistical offices are trying hard to provide more and better data on, but changing statistical practices is a slow process, the demands are plentiful, and the strong calls for reducing the burden on firms to provide data do not really help. It therefore requires academics to be creative in developing measures that proxy as good as possible what is needed, but who at the same time are careful in exploiting the data adequately. This study by Mary O'Mahony of the National Institute of Economic and Social Research is the clearest proof that such scholars still exist. Following upon earlier research at the National Institute on growth, education, and productivity which goes back to the 1940s, this book is the definitive recognition that the National Institute has remained a key player in this area between economic theory and empirics. Above all the book provides long term series of real output, employment, working hours, capital services, labour force skills, and resulting measures on labour and total factor productivity for five major economies (France, Germany, Japan, the United Kingdom, and the United States) from 1950 to 1996. O'Mahony breaks the series down into 22 sectors, and she provides a disaggregation for 17 manufacturing industries. The series are largely obtained from national statistical offices, but where necessary activities are carefully reclassified to make those internationally consistent. These reclassifications reveal some of the huge shortcomings of national statistical systems, such as the fact that it was not possible to distinguish German data for business services (one of the largest and fastest growing sectors in the economy) from 'miscellaneous personal services'. What does O'Mahony's database add to other existing databases, such as the ISDB/STAN database from OECD? Firstly, O'Mahony's sectoral and industry breakdown is very fine as it distinguishes between, for example, different types of transport (air, road, water, and other), financial services (banks and insurance), trade (wholesale, retail, hotels), and as it provides separate indicators for, for example, business services and office machinery. Secondly, O'Mahony uses a consistent methodology to estimate the capital stock series which she then uses to obtain measures of capital services. The latter is a major step forward in measuring the contribution of capital to output and productivity growth. Thirdly, O'Mahony adopts a fairly unique way of measuring the skill composition of the labour force (for Britain, Germany, and the USA), which differs from what is usually done in international databases. Instead of measuring enrolment or attainment rates or reallocating the functional distributions of the labour force to skill categories, she uses evidence, partly based on earlier work at the National Institute, which disguishes between high, intermediate, and low skills based on comparisons of levels of certified qualifications. Apart from general education of the labour force, these measures also take account of vocational qualification levels. Fourthly, the author develops purchasing power parities (PPPs) at sector and industry level, so that comparative measures of output and productivity levels take account of differences in relative price levels. For the 22 sectors (except agriculture and manufacturing) she uses detailed PPPs for 150 expenditure categories which were adjusted for value added tax and reallocated to producing sectors using expenditure weights. For agriculture she uses measures based on producer prices from the Food and Agriculture Organization (FAO), and for manufacturing industries she uses unit value ratios, which are bases on quantities and values at producer price level. There seems to be much potential to improve this set of PPPs, by exploiting producer price data to a greater extent, by improving the adjustments from expenditure to producer price levels, and by using weights which are more representative of production rather than expenditure. On top of her achievement to construct this comprehensive data base, O'Mahony provides a useful analysis of both labour productivity and total factor productivity as performance measures. By using a growth accounting framework, she finds that Britain's lagging in productivity relative to the United States and the European continental countries is due to a broad shortfall in physical and human capital investment. In analysing the results and given the focus of most previous work on manufacturing, O'Mahony points in particular at the relatively bad performance of British market services. Still it is a pity that the book's title suggests the emphasis is on British productivity performance. This may suggest that the author has less to say about the comparative growth record, and indeed this could have received more attention in the book. But this could be future work or left to be done by users of this data base. Fortunately the National Institute lets the user benefit from modern information technology by providing the data on a CD-rom which can be ordered separately from the book. One of the crucial elements in retaining the value of such databases concerns the need for updates. It is therefore hoped that these series will be extended to more recent years and make use of new macro data.

Bart van Ark

University of Groningen

Remaking Europe: The European Union and the Transition Economies. Edited by van Brabant (Jozef M.). (Totowa, NJ: Rowman and Littlefield, 1999. Pp. xix+269. £50.00 hardback, £19.95 paperback. ISBN 0 8476 9323 6, 0 8476 9324 4.)

Jozef M. van Brabant has edited an excellent and very welcome volume about the transition economies of Central Europe and their target of ensuring catch-up with the European Union. The importance of this volume is drawn from the mounting complexity of understanding the role of transition economies in the process of integration into the work market, and especially given that a selected enlargement is not a long way off. At least from the point of view of the European Commission such a deadline has been set for the year 2003.

Discussion focuseses on the economies of central Europe that seem likely to show larger chances of joining the European Union. Such economies have to compete with the western standards of production, productivity, and living standards. Brabant has been completely successful in collecting rigorous case studies and new interpretations on the economic convergence in Europe to spur new lines of debate. Although I cannot repeat in detail all the interesting contributions, I introduce the reader by spousing the ten chapters and make my own considerations.

I shall privilege the irresolute and indefinite criteria to be used in permitting transition countries to become full components of the European Union. The Maastricht treaty and two Councils held respectively in Copenhagen (June 1993) and Essen (December 1994) set forward some principles to perceive as a type of preaccession convergence strategy, among unequal countries, for integration to be worth pursuing. The Agenda 2000 was the last step of this strategy but we are still far from fleshing out clear criteria of accession. Additionally, the volume makes clear that the process of enlargement might cause complexity because of the call for economic convergence, on the political perceptions in the west on the desirability of such a process and, third, on the asymmetries between east and west likely to shift forward the process of enlargement.

Central to Lavigne's chapter is a plain criticism about the inconsistency of current criteria and other Commission statements from being of definitive help to decide the enlargement. Actually, Lavigne writes it is irrelevant that some candidate countries already meet Maastricht monetary and fiscal criteria but she does not formulate accurate alternative criteria, and reminds the reader that statistical data are far from complying with western standards too. Whilst agreeing with the criticisms of the absence of definitive criteria of enlargement I underline the importance of selected countries complying with Maastricht. Otherwise, it would be a contradiction to argue at the same time that the Maastricht criteria are irrelevant and that one has to dislike (as she seems to) nominal convergence but not real convergence of monetary, financial, budgetary, and exchange rate mechanisms.

However, Lavigne's chapter has been selected by Brabant with an eye to influencing the other contributions to bring about a debate about alternative or additional benchmarks to Maastricht by addressing the evidence on the convergence of nominal and real variables. While Brabant focuses on widening versus deepening, Orlowski discusses the nominal convergence and the contemporaneous managing of exchange rate and monetary policy. The chapter by Andreff favours the real convergence between income, productivity to fill the gap and keep down transfers to east. As to real convergence, Gabrish and Werner further analyse trade liberalisation and structural adjustments. Welfens corroborates analysis by introducing trade and foreign direct investment.

In the last two chapters, Brabant explores how this policy of eastward enlargement may affect Euromediterranean policies and suggests that the likely timing of such a process of membership would be a matter of guess work.

The volume edited by Brabant advances an extremely good analysis of the process of enlargement of the European Union. He presents all relevant aspects in a unified framework and argues that structural convergence is the essential aspect of a lasting Union. Although they do not give a definitive account of useful convergence criteria, they make a topical contribution to the understanding of the European enlargement strategy that might be developed in the future.

Jozef M. van Brabant has clearly distinguished between the politics and the economics of enlargement and turned attention on latter. A deeper structural convergence could prove useful to avoid asymmetric shocks and social and economics troubles. However, one can say that joining the European Union is only the first step of a full membership in to the European Monetary Union. Complete convergence of transition economies in western style is not possible because structural gaps exist among European economies and indeed were present at the time of the European Community southward enlargement in the 1970s.

This is an authoritative analysis of the widespread research on economic strategy in transition countries in Central Europe and European integration. It is very important volume and a valuable asset to anyone concerned with the various aspects of political economy to transition. All told, the volume is essential reading for area specialists as well as policymakers.

Bruno S. Sergi

University of Messina & CERC-University of Melbourne

Public Finance and Public Choice: Two Contrasting Visions of the State. By Buchanan (James M.) and Musgrave (Richard A.). (Cambridge, Mass. and London: MIT Press, 2000. Pp. vii+272. £16.95 hardback. ISBN 0 262 02462 4.)

James Buchanan and Richard Musgrave are, without doubt, among the most influential writers of the last half century in the general areas described by the title of this book. Furthermore, they clearly represent the two approaches that are identified by the labels 'Public Finance' and 'Public Choice'. This rather unusual book celebrates these two facts by bringing the two authors together in a form of debate. The book derives from a week of meetings hosted by the Center for Economic Studies in Munich in 1998, at which each author presented prepared papers and also acted as discussant for the other's papers. An invited audience acted to generate further discussion. All of this is faithfully recorded both in the book and in a series of videos of the event that can be accessed on the CES website (http://www.ces.vwl.uni-muenchen.de).

The book opens with a set piece lecture/essay by each author intended to set the scene and discuss the origins of their approach. Of these two essays, Buchanan's is the more autobiographical and personal, Musgrave's the more concerned with the history of thought. The essays exemplify, rather that explicitly state, the fundamental distinctions between 'Public Choice' and

'Public Finance' – with Buchanan emphasising the nature of public decision making, the roles of institutional and constitutional structures, and the links between economics and ethics; while Musgrave focuses attention on the problems of the provision of public goods, taxation, the trade-off between efficiency and equity, and the role of macro-policy. Although each author recognises the importance of the issues raised by the other – and in their long careers each has contributed across the areas – there remains a clear difference in their basic perspectives that is well illustrated here.

The remaining four sections of the book operate on the model of paper and discussion, and serve to further identify and deepen the contrast between the two approaches – as is clear simply from the titles of the papers presented – 'Fiscal Tasks' and 'Fiscal Federalism' by Musgrave; and 'Constraints on Political Action' and 'Morals, Politics and Institutional Reform: Diagnosis and Prescription' by Buchanan. I will make no attempt to provide a summary of the points made, beyond noting that the papers are best seen as providing overviews of the relevant issues in a public-lecture style. To the reader who is already broadly familiar with the major elements of public economics, the key interest in these lectures lies not so much in their first-order content seen in isolation, as in the second-order contribution to the debate between the contrasting perspectives. It is interesting that active or pointed debate between the two perspectives is never explicitly joined in any sustained way – either by the major protagonists or in the more general (but often not particularly helpful) discussion that is reported after each pair of papers. At every opportunity each author politely welcomes the contribution of the other and merely wishes to add to it, or to emphasise a rather different point. And, of course, there is a sense in which many of the insights of the 'Public Choice' and 'Public Finance' approaches can be combined to yield a more inclusive and general 'Public Economics'. But such accommodation may have its limits. Different perspectives can be conceived simply as alternative points of view from which a particular subject is studied. This reading suggests that the subject itself is independent of the perspective from which it is studied, and that different perspectives complement each other in providing a more rounded and complete understanding of the subject. However, it is also possible to conceive of different perspectives as being informed by very different and essentially incompatible philosophical and normative commitments. This reading suggests that there is more at stake in the choice of perspective, and that perspectives must be seen ultimately as substitutes rather than as complements.

The debate between Buchanan and Musgrave is, perhaps, rather muted by the nature and form of the event reported in this book; but the interested reader will find many of the ingredients for a more adversarial exchange, as well as finding much else of interest in the reflections of two major scholars. The less specialist reader will be presented with a flavour of the economic debates on both the role of government and on the analysis of government presented with clarity and wit by authors who can genuinely be referred to as authorities.

ALAN HAMLIN

University of Southampton

The Great Divergence: Europe, China, and the Making of the Modern World Economy. By Pomeranz (Kenneth). (Princeton, NJ: Princeton Unversity Press, 2000. Pp. x+382. £25.95 hardback, US $39.95 hardback. ISBN 0 691 00543 5.)

This dense, comparative monograph tackles with renewed vigour the issue of how and why western Europe developed so differently from the rest of Eurasia during the 17th to 19th centuries. This is a new path from the author's own field of Chinese economic history.

Pomeranz's volume has three interrelated parts, each having its own theme and conclusion. Part One deals with developmental similarities amongst the 'core regions' in Eurasia in terms of standards of living and market conditions. Part Two concentrates on the demand patterns and the role of institutions. Part Three pays most attention to the significance of the 'ecological windfall' from the New World in the formation of the world economy under the banner of western capitalism. To weave so many things together in a comparative framework is a remarkable achievement in itself.

Uncompromisingly critical towards 'pro-Western' literature on growth performance in the Eurasian past, this book abandons the commonly applied 'deficit approach' in judging Asia. So, rather than list factors for industrialisation missing from some societies, the author tries to indicate what western Europe and the rest of Eurasia had in common. Useful data, which alone serve as a valuable source of information, are painstakingly compiled and compared.

Pomeranz indicates, quite rightly, that several core regions in Eurasia reached the same *isoquant* simultaneously by allocating/substituting factors of production according to each society's own distinctive budget constraint. They probably even had the same *indifference curve* for consumption. But, there is a danger in overplaying quantitative similarities. Was western Europe more or less another Asia before its ecological blessing from the New World? Or, did the quantitative similarities only represent an intersection of two different growth curves, one characterised by a 'Ricardian stationary state' (for Asia) and the other by increasing returns (for western Europe) so that they looked quantitatively similar at the time of encounter? The questions have remained unanswered, although European military supremacy, a *force majeure* which effectively changed the rules of the game in favour of Europe, is repeatedly referred to as part of the qualitative divergence before, during, and after the industrial revolution (pages 19–20, 171, 182).

Pomeranz's re-interpretation of Eurasian history is based on two assumptions (1) that the chance to have capitalist industrialisation was once equal among the core regions (pages 62, 212), and (2) that it was the extraordinary ecological wealth available outside Eurasia during the crucial 17th to 19th centuries (here timing being also an economic resource) that bailed Europe out of universal growth constraints (pages 25, 211, 283, 296). To Pomeranz, this windfall outweighed a range of advantages enjoyed by the Asians including near-perfect market, near-free labour, and hyper-productive agriculture and so forth (chapters 1–2), giving Europe the upper hand in the world arena. This 'surplus accounting' takes the New World as a 'tipping factor' that made all the difference.

Although bold and largely factual in the western European context, Pomeranz's thesis is not completely novel: a similar approach was taken by Eric Jones 20 years ago who recognised the importance of the unprecedented endowment gain from the New World. The main difference is that Jones also took other factors such as the market and state with more or less equal weight in the making of the European miracle. Malthusian in nature, Pomeranz's mono-ecological explanation faces some tough challenges in history. For example, Qing China had a considerable ecological windfall from, especially, Manchuria, an area on the same latitude as Germany and England and the size of the entire western Europe (before the Russians took a cut along the Amur River), with good soil, water, timber, minerals, and easy access to land and sea trading routes. Manchuria was open for economic migrants from the interior (not to mention the government resettlement schemes which revitalised the depopulated regions such as the Sichuan Basin). Why and how did such a windfall do so little for China?

In addition, Pomeranz suggests that Europe's quantum leap was only possible *after* the ecological supply shock (chapters 5–6). But normally possessing a resource endowment is not the same as exploiting that resource. Given that the Great Discovery was an unintended consequence of the European bid for by-passing the Ottoman monopoly over trade with the East and that it took some time for the Europeans to figure out what to do with the ecological windfall, some degree of industrialisation (in transport and arms at least) must have taken place before the windfall became economically meaningful. Also, to efficiently cash in globally such windfall necessitated unprecedented industrial (and institutional) development in Europe. Empirical evidence seems to favour the view that the windfall was a result or *externalities* from a long string of industrial and commercial developments in Europe.

Less important, the Ottoman Empire, a powerful, prosperous and long-lasting core, is omitted in the analysis. Some relevant scholarship including Mark Elvin's recent works on China's economy and environment is overlooked. Nevertheless, this work will leave a significant mark on Eurasian comparison.

KENT DENG

LSE

The Current State of Economic Science: Voumes 1–5. Edited by DAHIYA (SHRI BHAGWAN). (Rohtak: Spellbound Publications Pvt. Ltd., 1999. Pp. xiii+2925. Rs.15,000 hardback, US $345.00 hardback. ISBN 81 7600 042 6.)

Occasionally students surprise their teachers by citing books about which the latter are ignorant or at best barely aware. The volumes in this set may well fall into this category, for while the articles they contain span just about all of economics, few are likely to become standard references. Even so, there is something here that might attract the attention of any student of economics, as a few indices of the work's dimensions will indicate.

The five volumes comprise nearly 3,000 pages devoted to 152 chapters authored by over 200 contributors. The average length of each chapter – about 19 pages – masks big differences, the range being from 4 to 57 pages. Some of the chapters are devoted to broad subject areas, e.g. 'Fiscal Policy', while others are highly specific, e.g. 'Regulating Banks in Australia: an Historical and International Perspective'.

Confronted with such a mixed bag, it is worth reflecting on the editor's objective. The Preface announces boldly that the intention is 'to produce a definitive source, reference, and teaching supplement for use by professional researchers and advanced graduate students'. To attain this ambitious goal each author was not only asked to survey an area but was also invited to make an original contribution representative of research at the frontier of the subject. Many evidently declined the invitation, perhaps by default, while the idiosyncratic interpretations of others present the reader with a highly variegated assemblage. The outcome is a set of volumes that have neither the encyclopaedic coverage of the *New Palgrave*, with its terse but authoritative entries, nor the depth found in the more specialised North-Holland *Handbook* series, with their magisterial and weighty surveys.

The arrangement of subject matter is apparent from the titles of the 16 parts into which the chapters are grouped (here followed by the number of constituent chapters): Teaching of economics (5); Methodology (8); Mathematical and quantitative methods (16); Microeconomics (17); Macroeconomics and monetary economics (12); International economics (19); Financial economics (10); Public economics (11); Health, education and welfare (5); Labour economics and industrial organisation (10); Business administration (5); Economic history (4); Economic development and growth (10); Economic systems (6); Agricultural and natural resource economics (10); Cultural economics (4). While the division corresponds roughly with the conventional categories of economics, it reveals a balance tilted towards the more applied areas of the subject. Although it is one of the longer parts, 'Mathematical and quantitative methods' must inevitably accommodate a wide range of material. In fact the overwhelming majority of its chapters are devoted to econometrics; and several of the remainder seem curiously out of place. For example, a chapter entitled 'Efficiency Rent of Hydroelectric Storage Plants in Continuous-Time Peak-Load Pricing' could just as easily, and perhaps more appropriately, be found a home in Microeconomics or Public economics. Worthy though this piece of research certainly is, the inclusion of such a specialised chapter in *any* part of these volumes again raises doubts about the ways in which the work's aims are implemented. In this case, as elsewhere, the impression is that the chapter focuses on a particular topic of research, which may illustrate issues or methods of general significance, rather than on a topic of general significance furnished possibly with illustrations chosen at the discretion of the authors. Given the stated goal, the emphasis seems the wrong way around.

Needless to say, in such a large and uneven collection there are exceptional contributions that do concentrate on principles of central importance, although most of these eschew the opportunity to report original research. An example is 'Auction theory: a guide to the literature' (chapter 39), which does exactly what its title implies. Others include 'Vector Autoregressive Analysis' (chapter 20), 'Optimal Dynamic Taxation' (chapter 90), and 'Applications of Game Theory in Natural Resources and Environmental Economics' (chapter 143).

Understandably, the coverage of chapter topics, their grouping into parts, and the weights accorded to the various parts are all matters of editorial taste. Although on the whole the result is not incompatible with the editor's objective, there are peculiarities. For example, Economic History merits but 4 chapters, all of which are rather specialised; none addresses broad themes in the development of the subject. Financial economics is more generously treated but, once again, the exposition of core principles is neglected in favour of specific applications. Central topics, such as asset pricing theory are hardly mentioned (e.g. Arbitrage Pricing Theory on pages 1654–5); the modern analysis of volatility is ignored as are derivative securities; portfolio selection appears only obliquely in the context of hedging. Subject specialists may well find that analogous remarks apply elsewhere in these volumes.

Attempts to produce a comprehensive overview of a subject like economics are never likely to receive the accolade of universal acceptance and, hence, are easy targets for criticism. *The Current State of Economic Science* represents a brave attempt, but the outcome resembles a giant *festschrift*, rather than a definitive reference source.

R. E. BAILEY

University of Essex

Unintended Consequences: The Impact of Factor Endowments, Culture, and Politics on Long-Run Economic Performance. By LAL (DEEPAK). (Cambridge, Mass. and London: MIT Press, 1998. Pp. x+287. £27.95 hardback. ISBN 0 262 12210 3.)

All experienced development economists sooner or later come to realise that investment in human and physical capital, and even investment plus technical progress, does not explain much about development and particularly not much about the unequal development of different countries. At that point, they struggle to incorporate 'culture' into economic analysis and the problem of course is how to accomplish this without falling into vague and soft headed story-telling. This book by Deepak Lal builds on his earlier *Hindu Equilibrium* (1989) in the effort to provide a well articulated inter-disciplinary and cross-cultural account of the rise of the West and the stagnation of the East over the last thousand years. The result is economic history on the grand scale with constant reference to the contrast between intensive and extensive growth, that is, growth in numbers with and without a rise in income per head. Along the way, it also raises the question of whether modernisation in, say, China and the Middle East, necessarily leads to westernisation, concluding agnostically that the two may be as easily disjoined as conjoined.

The book tells a story that carries us all the way from hunter-gatherers to the financial markets of the 20th century. The basic framework is the distinction between material and cosmological beliefs – dare we say 'base' and 'superstructure'? – and how both were initially shaped by factor endowments and then evolved in response to changing historical circumstances in different civilisations. But there are many other themes that appear and reappear in the course of the argument: how the factor endowments and cultural pressures interacted to produce the intensive growth of the West in recent centuries, which took off from an agrarian-based economy in the 18th century to become a mineral-based version of growth in the 19th century; the emergence of individualism as a legacy of the medieval Catholic Church – (Max Weber was right but his dates were off by as much as 500 years); the emotion of shame as the critical control on behaviour in the cultures of the East, whereas guilt performs the same function for the cultures of the West; the role of the caste system in India and the extended family in China in tying down the labour needed for labour-intensive plough agriculture; the corrosive effects of public transfers in both developed and developing countries in recent decades; and many, many more. The author is wonderfully well-read in the relevant secondary literature, in history, politics, sociology, and anthropology.

My only criticism is that he is sometimes slightly credulous when it comes to the 'magnificent dynamics' of many cultural and political historians; almost anything goes provided it sounds plausible. Still, 'because economists are persuaded only by formal models', the book ends with four formal models of the growth process; they do little for me in strengthening the author's argument but others may find them more persuasive.

Did I mention that the book is so well written that it is difficult to put down? It is the dull mind that will find this book dull.

MARK BLAUG

University of Amsterdam

Structural Unemployment and Real Wage Rigidity in Germany. By PAQUÉ (KARL-HEINZ). (Bremen: Institute for World Economics, 1999. Pp. xi+387. ISBN 3 16 147269 1.)

The 1980s witnessed a sharp rise in the rate of unemployment in western Germany, in line with experience in a number of European countries. The main thesis of this book is that the rise in West German unemployment is best understood in terms of a structural decline in the demand for unskilled workers in the industrial sector. The author argues that within industry, complementarities between capital and unskilled labour lead to an earnings premium for unskilled workers, relative to their potential earnings in the service sector. In the event of a shift in demand from industry to services, unskilled workers who lose their jobs in the industrial sector therefore prefer to remain unemployed ('queuing' for scarce industrial sector jobs) rather than accept low wage employment within the service sector. They are facilitated in this by the unemployment benefit system in Germany, which pays time unlimited benefits at a level linked to the worker's previous wage.

The theory advanced by Paqué is a persuasive one that potentially has relevance to the explanation of unemployment in a number of European economies. The author develops his analysis by first presenting a critique of traditional Keynesian and Neoclassical explanations of unemployment, and modern views on unemployment persistence. He argues persuasively that a simple Keynesian story cannot account for the long-run movements of unemployment in post-war Germany, and points to flaws in the traditional 'wage gap' measures used in a number of Neoclassical analyses. Explanations of unemployment persistence in terms of capital shortages, insider-outsider models, and the physical depreciation of human capital are rejected in favour of the structural unemployment hypothesis described above.

Following this, Paqué presents a critique of conventional mismatch indicators of structural unemployment, showing that measures based on the dispersion of unemployment across industries and/or regions, or the relative position of industry/regional Beveridge curves, will almost inevitably ascribe only a small proportion of total unemployment to structural factors. Instead, Paqué develops his theory of the importance of structural factors in West German unemployment by presenting figures that point to a dramatic skill intensification of production in West Germany during the 1970s and 1980s, that led in particular to a large scale decline of unskilled jobs in industry. In many respects, this explanation is similar to that of a number of authors who have attributed a primary role in the rise in the level of European unemployment to a decline in the relative demand for unskilled workers. However, Paqué is careful to differentiate his approach, by emphasising that it was the loss of unskilled jobs from industry, rather than the skill intensification of production *per se*, that was responsible for the rise in West German unemployment.

As Paqué observes, a shift in the relative demand for skilled versus unskilled workers need have no implications for unemployment if it is accompanied by an appropriate adjustment of relative wages. However, he produces a wealth of econometric evidence to show that, on the contrary, labour markets in western Germany have tended to be characterised by a significant degree of real wage rigidity. After the earlier chapters, this part of the book is something of a disappointment. The analysis is competently executed but the available data do not permit an examination of the issue that is central to the author's hypothesis, namely the degree of rigidity in the relative wages of skilled and unskilled labour. Instead, the reader is presented with a rather repetitive set of regression analyses investigating the degree of wage rigidity across regions, sectors, and industries.

The final substantive chapter of the book contains a discussion of competing explanations for the dramatic rise in unemployment in the former East Germany, following the reunification with the West. The explanation favoured by the author centres on the wage behaviour of the German trade unions. In Paqué's view, the unions' traditional desire for wage equalisation led them to press for a rapid rise in wages for workers in the East German *länder* towards levels prevailing in the West, even though prevailing productivity differentials meant that this rise could only be sustained at the expense of large scale job losses and a dramatic rise in eastern German unemployment. This explanation seems to be more consistent with available evidence than the alternatives discussed. However, a lack of sufficient data again precludes a formal test of competing hypotheses.

The book contains much insightful analysis and should be of interest to a wide range of economists attempting to find explanations for high levels of unemployment in Europe. However, if at the end the reader is perhaps left with a slight sense of disappointment, this may be because it seems that for the German economy at least, the available data at present preclude a proper testing of some of the interesting ideas developed by the author.

MARTIN T. ROBSON

University of Durham

Economics and Environment: Essays on Ecological Economics and Sustainable Development. By PEARCE (DAVID). (Aldershot and Lyme, NH: Edward Elgar, 1999. Pp. xii+363. £59.95 hardback. ISBN 1 85278 772 4.)

This is a book of essays, some of which were published more than a decade ago and have multiple authors. Nevertheless, there is a considerable degree of integration, with later essays providing support and illustration of the material set forth in earlier chapters. In Parts 1 and 2 the emphasis of the essays is on conceptual analysis, and most of the basic analytical problems faced by environmental economists are examined in a comprehensive and clear manner. The essays cover such issues as the valuation of environmental benefits and damage, the distinction between ecological and enviromental economics, the advantages and limits of cost-benefit analysis as a guide to environmental policy, sustainable development, and the relationship between sustainable development and economic growth. Part 3 covers the application of environmental econmics by environmental managers to pollution and human health in the United Kingdom, wildlife conservation, risk assessment, and determining the social rate of discount. Part 4 deals with a review of the literature on global warming and the application of environmental economics, including cost-benefit analysis, to global issues. The essays provide many references to the literature, and analyse the arguments both supporting the author's position and those disagreeing.

The essays make a strong case for 'orthodox' economics for providing the answers to environmental problems, as contrasted with the noneconomic ecological approach. Economics requires measurement and comparison of benefits and costs in contrast to the position that all damage to the environmental base should be prevented as a matter of principle. The author opposes the no-growth policy of Daly (1991) and other ecologists. Not only can economic growth be made consistent with preservation of the environment and the habitability of the earth, but a no-growth policy would be unacceptable to citizens in developed countries and condemn developing countries to indefinite poverty.

The author's strong defence of the economics approach to environomental issues requires the use of quantitative measurement of both market and nonmarket benefits and costs, the methodology of which he reviews in a realistic manner. The major conceptual problems of cost-benefit analysis relate to the determination of (1) the social rate of discount and (2) the benefits or costs experienced by future generations. The author rejects consumer preference as a basis for determining the social rate of discount on a number of grounds in favour of investment cost. Investment cost provides a means of not only comparing present costs with future benefits, but of making comparisons among investments for realising alternative social values, including alternative environmental objectives. If resources are limited, should we invest in the preservation of salmon or in reducing greenhouse gases? In both cases, the realisation of benefits must take into account risk and uncertainty. He estimates the social rate of discount for Britain at between 2 and 4% per annum as contrasted with the current rate of return on government bonds.

The author explores the limitations of cost-benefit analysis for cases where harm from certain pollutants arises only when nature's assimilation capacity is reached. He suggests alternative approaches for this and other cases of dynamic externalities. He also deals with the classic problem of calculating present values of benefits received by future generations, where any positive rate of discount will reduce millions of dollars worth of benefits to a present value of a few dollars. The author devotes relatively little attention to this problem, despite the importance given to it in recent literature (Portney and Weynant, 1999). He does not favour using a low or zero rate of discount for valuing intergenerational benefits, but suggests introducing a 'sustainable constraint' (page 153) in applying the social discount rate. It is not clear to the reviewer how these two concepts are reconciled in reaching environmental policy decisions.

Pearce devotes several chapters to sustainable development, which he regards as a basic social goal that should be guided by environmental economics. He defines sustainable development as non-declining consumption, but suggests that a broader welfare index might be used. However, he believes that in planning for sustainable development there should be a maximum time horizon, say, 100 years, subject to popular will, but does not believe that 'selfish altruism' (page 71) can guarantee sustainability. He discusses the current debate between supporters of 'strong sustainability' and 'weak sustainability'. He identifies the former with those that stress the preservation of ecological assets, while the latter recognise the substitutability between manufactured assets and natural resources.

What I find missing is a concluding chapter in which the major positions taken by the author on controversial issues are defined and fully integrated.

This is an excellent book which everyone interested in environmental problems will find enlightening. It is not a textbook, but could serve as valuable supplemental reading in environmental economics courses.

RAYMOND F. MIKESELL

University of Oregon

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The World Bank: New Agendas in a Changing World. By MILLER-ADAMS (MICHELLE). (London and New York: Routledge, 1999, Pp. xii+176. £50.00 hardback. ISBN 0 415 19353 2.)

The Earthist Challenge to Economism: A Theological Critique of the World Bank. By COBB, (JOHN B.) JR. (London and Basingstoke: Macmillan, 1998. Pp. ix+192 £42.50 hardback. ISBN 0 333 73088 7.)

Michelle Miller-Adams's book discusses three new policy themes that have emerged, in differing strength, in the work of the 1990s World Bank. They are private sector development, participation, and governance. On the basis of case studies of the fate of these three agendas, she then tries to answer the general question of how new agendas get adopted, and whether the main impetus to change is external or internal.

Externally, the Bank's management can come under pressure from the member governments that own it, of which the largest is still the United States. International relations theorists might be inclinded to suggest that changes in the Bank's agenda are in response to the changing preferences of the member states, adjusted for their relative power. Internally, the culture, norms, and attitudes of managers and staff influence whether new management initiatives take root. Sociologists of organisations might want to suggest that these factors determine the fate of new agendas, both in relation to the resources that are allotted to them and the urgency with which they are pursued.

This dichotomy is explored in chapter 2. Miller-Adams argues that, although neither explanation is adquate on its own, internal factors explain more about agenda changes than do external ones. She takes a strong position to the effect that the processes of recuitment, socialisation, and promotion have produced a homogeneous culture within the Bank, based on the values of explicitly apolitical and technocratic professionalism. These are the values, she argues, that have served the Bank well in its history to date, aiding its survival and growth, while establishing its legitimacy.

The adoption by the Bank of the agenda of private sector development, discussed in chapter 3, did not result from the US willingness to block a captial increase to the IFC in 1991, according to Miller-Adams. Rather, it was its congruence with the internal culture that allowed it to flourish, despite the large legal and financial obstacles preventing direct lending to the private sector. The author is especially good on the precise nature of these obstacles, and the various attempts that have been made by the Bank to navigate them.

Her presentation of the advance of the participation agenda (chapter 4) is, in my view, less successful. A variety of distinct issues get mixed up under the label of 'participation'. The growing experimentation in the 1990s with beneficiary assessment needs to be distinguished from the sporadic use of participatory poverty assessments, and both of these need to be separated out from the essentially US-based political struggles between the Bank and Northern NGOs over whether the Bank violated its own codes of conduct in large resettlement projects. Lumping these stories together does not shed as much light as would be desirable. Miller-Adams tends to rely too exclusively in this chapter on the Bank's own documents, and her interviews and correspondence with Bank staff. I do not imply that this material is carelessly weighed or presented uncritically – not at all. I suspect, however, that an author with a deeper background in the subject of participation itself could have made somewhat better sense of the Bank's attempt to digest this agenda.

Chapter 5, on the arrested development of the Bank's agenda on governance, does not suffer from these problems. The shaping power of the Bank's internal culture, especially the value of preserving its apolitical stance, is well demonstrated. One significant omission from this account, nevertheless, is the Bank's role in establishing the Africa Capacity Building Foundation, and in nursing it through a succession of embarrassing teething troubles. It has now reached the point where it may indeed be able to implement a broader governance agenda in Africa.

Overall, Miller-Adams's study is well documented and illuminating, being particularly instructive on the Bank's legal and financial constitution. Her conclusion that the Bank has a strong internal culture that filters new agendas, so that some attain more centrality than others is surely right. On the relative strength of internal and external pressures, one wonders whether external influences on the Bank are fully accounted for. Trust funds, which grew in importance in the 1990s, are mentioned only once, on page 76. These are a method by which member governments induce the Bank to pay more attention to the new agendas that they favour. Part of the work of the Bank has come to depend, not just on what the management allots to it from the regular budget, but also on whether individual task managers are able to access resources locked up in trust funds and other types of special contribution. In this situation, the neat boundary between external pressures and internal culture becomes a little blurred, and the issue is deserving of more analysis than it gets here.

The Miller-Adams book reflects the atmosphere of the early years of the Wolfensohn presidency, in which the latter positioned himself politically as the outsider, as the David who would slay the Goliath of Bank culture, as the mover and shaker who would put a stop to all those things about the Bank that people did not much like. The closing remarks of the book go along a little too easily with the apocalyptic vision that the Bank will survive only if it becomes a 'knowledge institution' and a necessary facilitator of private sector investment, and that this will depend on its constant embrace of new development agendas. Current politics may require this to be said, and the new development agendas may all be desirable in an ideal world, but it is still not clear that it is true, and that survival will not depend on the success of its more mundane lending activities.

The charisma of Mr Wolfensohn is a major theme of the book by John J. Cobb, Jr. In chapters 4 to 7, Cobb takes the reader through some highlights of the history of the Bank. His story leads up to the emergence, in the first half of the 1990s, of what he describes as 'coherent opposition' to the Bank. This was based on criticism of the structural adjustment policies of the 1980s and complaints against the Bank's handling of environmental and re-settlement aspects of its large dam projects. By 1994, the NGOs were able to unite for their 'fifty years is enough' campaign. This is important background to the Wolfensohn presidency, and chapters 8 and 9 seek to pinpoint changes in the Bank's 'paradigm' of thinking, in the context of asking whether Mr Wolfensohn will be able to transform it from an 'economist paradigm' to an 'earthist paradigm'. Cobb hopes so, but is not really sure.

Chapters 1–3 and 10 provide a more bizarre bill of fare. I pass over lightly the opening discourse on the ideological epochs of world history, as should the reader, unless Comte's positivism rendered in the style of Sellars and Yeats's *1066 and All That* seems likely to appeal. The main upshot is that the founding of the Bank in 1944 marked the opening of the 'age of economism' that Cobb hopes will soon be displaced, Mr Wolfensohn being both willing and able, by the 'age of earthism'. Earthism is an ideology that is centred on 'the betterment of the condition of the Earth, including its inhabitants' (page 147).

Does the book live up to its intriguing sub-title? Does it provide 'a theological critique of the World Bank'? In my view, it does not. What Cobb does is to expound the familiar litany of doubts that the Bank's opponents tend to have in common. These doubts are about consumerism as a *summum bonum*; about neo-liberal economics as scientific laws; about liberalisation and globalisation as the only way into the future; about the feasibility of unlimited economic growth and about the unsullied benevolence of transnational corporations. These are doubts which, to a greater or lesser degree, I share. However, I would not agree that they amount to a theological critique of the World Bank.

I could find only one paragraph in Cobb's book that was recognisable as theology in the sense of a systematic realisation of Christian religious belief.

> 'We understand God as the creative Spirit who through the ages has called life into being in all its rich panoply, who prizes the great variety of things

> ... To destroy casually and without justification what God has brought so painstakingly and lovingly into being is a sacrilege. It inflicts a loss not only on those that are destroyed and on other creatures who now miss the lost ecosystems and the extinct species, but also on God' (page 186).

And this theology of creation seems to me to have the much same weakness that the English student of fossils, John Ray, complained about in 1703.

> 'Philosophers have been unwilling to admit (that many species of Shell-Fish are lost out of the World), esteeming the destruction of any one species a dis-membering of the Universe, and rendering it imperfect; whereas they think the Divine Providence is especially concerned to secure and preserve the Works of Creation.'

It might be more persuasive, in the twenty-first century, if the theology of 'earthism' took some account of the theory of evolution.

JOHN TOYE

Centre for the Study of African Economics, University of Oxford

The Asian Financial Crisis: Causes, Contagion and Consequences. Edited by AGÉNOR (PIERRE-RICHARD), MILLER (MARCUS) and VINES (DAVID). (Cambridge and New York: Cambridge University Press, 1999. Pp. xxviii+417. £45.00 hardback. ISBN 0 521 77080 7.)

This book is based on conferences held in May 1998 at London University and July 1998 at the University of Warwick. The short space of time between the onset of the Asian Crisis and the conferences is both the book's strength and its weakness. Part One provides several interesing accounts of what actually happened and how these events can be interpreted. For example, the first chapter by Alba *et al.* documents the effects of the crisis, focusing on Indonesia, Korea, Malaysia, the Philippines, and Thailand. It also presents the consensus view on what happened. The crisis came as a surprise. The macroeconomic policies the governments of these countries were pursuing were not profligate and inconsistent as had been the case in a number of previous crises such as those in Latin America in the 1970s and 1980s. Many commentators identified financial sector weaknesses as the prime cause of the crisis. In particular, government guarantees and the associated moral hazard these engender were widely blamed. In addition, lack of transparency, corruption, and poor corporate governance were thought to be important.

The second part of the book goes beyond this conventional view and presents a number of alternative theories of the crisis. Chapter 5 by Aghion, Bacchetta, and Banerjee and chapter 7 by Morris and Shin are very good. The third part, which is in many ways the best, provides several empirical investigations of the contagion of the crises. Chapter 8 by Masson develops a useful framework for thinking about the spread of crises. Chapter 9 by Glick and Rose is an excellent analysis of the role of trade links and common macroeconomic factors in spreading crises. Trade links are found to be much more important than common macroeconomic factors. The final part contains a discussion of policy responses to the crisis.

The weakness of the book is that there is very little attempt to place the Asian Crisis in a wider context. Financial crises are not new. The combination of banking and currency crises that occurred in Asia is in many ways similar to crises that happened in the 19th and early 20th centuries. This point is made by Goodhart in the last three pages of the book. It is crucial because it is difficult to believe that government guarantees and the associated moral hazard played any role at all in the 19th and early 20th centuries. As Goodhart points out, there were major crises in 1873, 1890, 1893, and 1907 and the USA was at the heart of all of these. At this time the USA had no central bank and there was very little government involvement in the financial sector. The Federal Reserve System was in fact set up as an attempt to stop crises. How likely is it that government guarantees are the problem if very similar events occurred in the absence of such guarantees? There is also little mention of the Scandinavian crises of the late 1980s and early 1990s. These provide an interesting comparison to the Asian Crisis. Again they are mentioned briefly at the end of the book but only in passing. Stiglitz points out in chapter 12 that Scandinavian countries are models of transparency and yet they experienced a very similar sequence of events to the Asian Crisis. Bribery and corruption are also not important factors in Scandinavia. This makes it unlikely that lack of transparency and corruption were important causes of the Asian crisis.

The other weakness of the book is that it focuses to a very large extent on the countries in Asia that suffered crises. One of the most interesing questions is why countries like Taiwan and China avoided the crisis and others such as Singapore and Hong Kong were less affected by it than Indonesia, Korea, Malaysia, the Philippines, and Thailand.

Financial crises were historically important and economists expended a great deal of effort in trying to understand them. The regulatory frameworks put in place in the 1930s and 1940s after the Great Depression essentially eliminated banking crises in most countries until the 1970s. Although the Bretton Woods system of fixed exchange rates did not eliminate currency crises these were quite different from the twin banking and currency crises that were previously so prevalent and which the Asian Crisis is an example of. The documentation and analysis the book contains will make it required reading for anybody interested in the Asian Crisis but it is only a starting point. Much work remains to be done before we have a full understanding of what happened there and elsewhere.

FRANKLIN ALLEN

University of Pennsylvania

Global Economy, Global Justice: Theoretical Objections and Policy Alternatives to Neoliberalism. By DEMARTINO (GEORGE). (London and New York: Routledge, 2000. Pp. xiv+279. £19.99 paperback. ISBN 0 415 12427 1, 0 415 22401 2.)

George DeMartino has written a serious and intelligent treatise on normative principles, with reference to economic and social organisation. It represents the best of a still flourishing minor tradition of American liberalism, the liberalism of the university moral philosophy class. Along with the careful, reasoned argument, however, goes an inevitable rhetoric of coaxing, just as in Plato's time, because philosophy professors think that they know precisely where their arguments lead.

This book is not an attempted refutation of the principles of neo-classical economics. It is a critique of an attitude of philosophical 'essentialism' that identifies those principles with reality itself. DeMartino sets out to tackle what he describes as 'the neoclassical vision' and those economists who, in global policy debates, 'routinely reach beyond what the science of neoclassical theory proper permits them to say' (page 35). It is, he contends, a vision that is both objectionably reductionist and deluded about its own normative basis, and yet it is achieving the status of a legitimate secular worldview.

In the first part of the book, the author presents an account of how economists use the idea of Pareto efficiency as a welfare criterion and why they hold that a system of freely adjusting market prices produces Pareto-efficient welfare outcomes. This account, though undoubtedly cursory, is not misleading about simple intuitions of welfare economics. DeMartino certainly gets the point across that the Pareto criterion, while elegant, is morally sparse, ethically exiguous. On the other hand, he insists that this type of welfare criterion is not value-neutral or value-free. It is just one among a wide range of principles of normative assessment. Other such principles, offered by John Rawls, Michael Walzer, Karl Marx, and Amartya Sen, occupy chapter 3.

In the second part, DeMartino investigates the philosophical complexities involved in the idea of *global* normative assessments. In an interesting discussion, he dismisses the claim that such assessments can be objective as being based only on faith. However, he also gives powerful reasons that steer us away from any strong version of cultural relativism. Thus, while there is no global ethic out there, cultural differences may not be such as to prevent its construction. At this point, the rabbit comes out of the hat. Hey presto! A potential principle of global justice! It is the 'harmonisation of capabilities to achieve functionings at a level that is sufficient, universally attainable and sustainable' (page 144). It is suggested that this principle is 'somewhat tolerant of cross-cultural difference' because it 'allows for diverse communities to identify and rank the functionings that they value' (page 149).

Here be dragons. We cannot harmonise capabilities without first specifying the functionings of which people are to be made capable. We know that they have to be valued functionings, because it would not be just to empower people for valueless or positively harmful ones. What is the source of this valuation? Is it philosophers, who know what it means to be human and what is required to live a 'valued' human life? Is it communities who have this knowledge, meaning the representatives (however they come to be so) of communities? Or is it each individual who values a particular set of functionings, because they will, for her, constitute a valued life?

Suppose an individual wants to function as a 'neoclassical visionary', and lives in a community whose philosophers cannot be sure whether to do so is part of the Aristotelian good life, and whose political representatives hold that such a function is normatively bankrupt. Would it be just that she be denied the substantive freedom to do so? This conundrum, and others that arise from an ambiguous use of the word 'valued', would repay closer attention than they get.

DeMartino uses the harmonisation of capabilities principle to criticise the burgeoning literature on 'competitivenes' in chapter 5, and to advocate policies that reduce the scope of competition in international markets in certain areas. Competition that forces down social, labour, and environmental standards should, he argues, be outlawed. In chapter 6, this theme is re-worked as a debate between free trade and fair trade. In Part 3, three policy suggestions are made as to how the capabilities principle might be applied. A regime of 'social tariffs' is elaborated, and global conventions on corporate conduct and labour mobility are proposed. Those who want the details must consult the book.

In my view, and possibly also the author's, this book's main contribution does not lie in its exact proposals. They, to seasoned observers of international trade negotiations, may seem impractical enough. Instead, the book provides a very educational conspectus of normative principles of socio-economic life, and it also points to a practical problem – how to raise social standards when they create competitive disadvantage in international trade – that is real enough. The ILO has worked away at it for eighty years, but alas it is still with us.

JOHN TOYE

Centre for the Study of African Economies, University of Oxford

The Real Worlds of Welfare Capitalism. By GOODIN (ROBERT E.), HEADEY (BRUCE) and MUFFELS (RUUD). (Cambridge and New York: Cambridge University Press, 1999. Pp. x+358. £37.50 hardback, US $59.95 hardback, £13.95 paperback, US $22.95 paperback. ISBN 0 521 59386 7, 0 521 59639 4.)

This is a remarkable book. Not because it is well-written, uses interesting new empirical data to test theoretical propositions, and has clearly presented findings, although it is all of these. What is striking is that it comes to a clear and unambiguous conclusion: the 'social democratic' welfare regime typified by the Netherlands out-performs the two main rival regime types (in Esping-Anderson's classification), the 'liberal' regime typified by the USA, and the 'corporatist' regime typified by Germany. Not only does the social democratic regime perform best in its own terms (low inequality and poverty), but it also does as well as or better than the others in terms of their central goals (economic efficiency, social integration, and social stability). This leads the authors to strong conclusions: 'The social democratic regime turns out to be the best choice, regardless of what you want it to do . . . one upshot of our study is to cast doubt upon the necessity of Okun's 'big trade-off' between equity and efficiency, between social and economic objectives' (pages 260–1).

The book starts with an excellent discussion of the aims of welfare policy, distilled into six: promoting economic efficiency; reducing poverty; promoting social equality; promoting social integration and avoiding exclusion; promoting social stability; and promoting autonomy. The authors argue that different regime types embody distinctive values, giving different weightings to these aims, distinctive theories of what produces social welfare and why some people fail to benefit, and hence distinctive policy responses. The core is the use of matched longitudinal data to test the performance in relation to these aims for 1985–94 for Germany and the Netherlands (using the German and Dutch socio-economic panels) and 1983–92 for the USA (using the Panel Study on Income Dynamics). As the authors concede, these are not necessarily representative cases, although they make a good case for them being 'successful' examples of each regime. That they only examine three countries is a consequence of needing comparable longitudinal data, as well as the depth of analysis (although in their conclusions, this restriction is sometimes forgotten).

The book demonstrates the importance of longitudinal data to examine key aspects of welfare performance: persistent poverty, income inequality over longer time periods than just at one moment, real world 'replacement rates' (comparing incomes when depending on the state with those before), and income stability over time. This makes the empirical analysis particularly strong for efficiency, poverty, inequality, and stability. However, it is less so for the aims of 'integration' and 'autonomy', mainly because the datasets do not have good coverage of the issues identified as important. Given this, it would have been useful to have looked for some external, not necessarily longitudinal, sources – for instance, attitudinal data.

When it comes down to the striking conclusion, the Netherlands emerges triumphant on its low relative poverty (using half of median equivalent household income), especially when aggregating incomes over a 5 or 10 year period, and low income inequality. The US regime not only does as badly on these, as one would expect, but also on the core economic performance measure used: growth in median household income, which was also abysmal in this period. Germany fails to do as well as the Netherlands on these, but also does no better on what are taken as being the key aims of corporatist regimes, social integration, and stability (indeed on one key measure of stability – low mobility within the income distribution – the USA performs 'best' in having least mobility, although this is a two-edged accomplishment).

There are aspects with which one could take issue. Comparisons are nearly all in terms of social security and income maintenance, while what makes up welfare states goes much wider than, including education and health services, for instance, and it would have been good to have had these included. Defenders of US social policy and of an equity-efficiency trade-off might look for wider measures of economic performance than median household income growth between 1983 and 1992. They might ask for much more focus on levels of incomes, rather than just rates of growth or relative inequality – for instance, by looking at cross-country performance on purchasing power parities against a common poverty line. Similarly defenders of corporatist regimes might argue that the measures used here for goals like social integration and exclusion and personal autonomy are imperfect. There are problems in extrapolating from these three countries to some of the general conclusions given. None the less, for a sharply focussed use of powerful comparative data to answer some of the largest questions in social policy, this book offers invaluable analysis and evidence.

John Hills
ESRC Research Centre for Analysis of Social Exclusion, London School of Economics

Politics, Institutions and the Economic Performance of Nations. By Siermann (Clemens L. J.). (Aldershot and Lyme, NH: Edward Elgar, 1998. Pp. xi+256. £55.00 hardback. ISBN 1 85898 609 5.)

Siermann sets out with four aims. First, to introduce and critically review the empirical studies of the influence of political and institutional factors on economic policy and outcomes. Second, to investigate the indicators which serve as proxies for such notions as political instability and democracy. Third and fourth to provide a data-set which is then used to examine the influence of political and institutional factors on inflation, government fiscal policies, and economic growth. The central chapters are organised accordingly: review of theoretical literature; review of empirical literature; new evidence; and conclusions. The coverage is exhaustive although not complete – which would be unrealistic. For instance Jagdish Bhagwati's original conviction (1966) that developing countries face a cruel choice between rapid expansion and the democratic process is noted, at page 136, but not Bhagwati's more recent position – what he called his 'nuanced revision' (1995). Moreover China and the Soviet Union/Russia do not feature in the study at all, although it is unclear if this is because the statistical information was unavailable or judged unreliable.

The book achieves what the author sets out to achieve, cogently, methodically, and in a technically meticulous manner. The methodology is carefully explained. The author offers rationales for the choices he makes when operationalising key variables. In this it is an object lesson that could be profitably brought to the attention of junior researchers and students. The exercise is clearly sign-posted throughout. But was the journey worthwhile? On that score the exercise is more notable for the hypotheses that are already current in the copious literature where Siermann finds no convincing support, than the number of propositions where he believes there is firm evidence. Examples of the latter include the somewhat unsurprising conclusion that central bank independence could bring down inflation, and the conclusion that socio-political unrest is bad for growth. More interesting, among the propositions he rejects is the one that states left-wing parties are less concerned than other parties about the level of the budget deficit, and the theory that purports to explain cross-country differences in fiscal policy and public borrowing by the government's level of internal political cohesion. He also finds there is no systematic relationship between democratic freedom and investment or between democracy and economic growth. This last is particularly significant and has obvious policy-relevance, given that in the last decade international aid donors have sometimes given the impression that they believed otherwise.

While claiming not to offer statistical testing of causal relationships, *Politics, Institutions and the Economic Performance of Nations* is an economist's study. It is not directed at the concerns of the many social scientists more fascinated by the political and institutional consequences for social welfare and human development, and distributive issues, than by economic performance narrowly defined. The non-appearance of Sen's name in the bibliography is symptomatic. It is the sort of study that has to be written; but it is not a book to set the blood racing. The dry, one-paced mode of delivery, or, possibly, the largely quantitative nature of the exercise left this reader (who is not an economist) with a sharpened appetite for the sort of qualitative insights and more pleasurable reading that the best examples of the case study approach can provide. More particularly, at 67 pages long the final substantive chapter is over long and could easily have been divided. The subject index is too economical and could have been more helpful, for example by citing separately the pages where the author first presents and defends his preferred concepts for democracy, political instability, and so on.

PETER BURNELL

University of Warwick

Reference

Bhagwati, Jagdish (1995). 'The New Thinking on Development' *Journal of Democracy* vol. 6, pp. 50–64.

What Do Economists Know? New Economics of Knowledge. Edited by GARNETT, JR. (ROBERT F.). (London and New York: Routledge, 1999. Pp. xii+259. £19.99 paperback. ISBN 0 415 15260 7, 0 415 20750 9.)

What do economists know? The traditional response to this question has been to establish various criteria of what counts as economic knowledge. More recently, authors in economic methodology and related fields have replaced this normative orientation with a positive interest in the knowledge claims made by the practitioners themselves. Deirdre McCloskey's work on the rhetoric of economics is the most prominent example of this different outlook. The present collection of fifteen essays, motivated by the original presentation of five of the papers at an interdisciplinary conference in Minnesota in 1994, provides us with a taste of how the rhetoric discussion has developed since. Indeed, according to the introduction, it is partly intended as a sequel to McCloskey's books.

In the first essay, by Jack Amariglio and David Ruccio, the engagement with McCloskey's work holds centre stage. Her introductory economic textbook *The Applied Theory of Price* contains an attack on what she calls 'ersatz economics', the erroneous economic views held by the lay public. Amariglio and Ruccio convincingly take her to task for what they regard as an intolerable inconsistency: if one follows the rhetorical turn, alternative discourses need to be distinguished in terms of their respective persausiveness, and not on the basis of an *a priori* rejection of ersatz economics. Their essay, which is followed by a conceding rejoinder by McCloskey, provides some interesting evidence from opinion polls and surveys indicating that the general public holds at least some form of economic knowledge which does not derive from formal economic training. The linear model of knowledge production, according to which economists are the producers, and everybody else acts as intermediary and consumer, does not apply.

A related paper by Robert Blendon and seven collaborators goes a long way to empirically strengthen the existence of a gap between economic knowledge held in the academic and public spheres. However, they take their findings as an urgent call to economists 'to do a better job educating the public about economic matters' (page 97). This constitutes an archetypical example of what Robert Garnett's introduction calls the 'old economics of knowledge' (page 3), which subscribes to the linear model of knowledge production. Given that this volume advocates a 'new economics of knowledge', the reader wonders why the study of Blendon *et al.*, previously published in the *Journal of Economic Perspectives*, has been included.

The rejection of the linear model directs attention to the boundaries of competing spheres of economic knowledge, and how these boundaries change or are sustained. This question is explored by several other essays in the volume. Michael A. Bernstein, for example, provides a useful introduction to the role of the second world war and the cold war in the development and international diffusion of what he calls the 'Americanized approach to understanding economic life' (page 113). And Arjo Klamer and Jennifer Meehan give us a glimpse on the political dynamics that led to the North American Free Trade Agreement (NAFTA).

Conceptually the two most interesting papers in the collection are by Judith Mehta and John Davis. Although the structure of the volume does not take advantage of it, they engage in an implicit debate with each other over what constitutes a boundary and how it should be studied. Mehta embarks on an ambitious attempt to deconstruct the idea of privileged discourses in a highly experimental piece of writing, while Davis, following the tradition of the classical methodological essay, exposes the modernist legacy underlying the post-modern celebration of pluralism by proposing a 'trade model' of how discourses interact with each other. Both papers defend a notion of discursive diversity, but do so in ways which undermine each other's starting point. Similarly contrasting are their modes of exposition. This mutual entanglement of the two papers makes the pair welcome study material for a class in economic methodology.

Considered as a whole, however, the volume leaves the reader wondering what constitutes the 'economics' promised in the subtitle. The intriguing aspects of an economic approach to the content of scientific and other forms of knowledge reside in the prospect of an economics of economics, a meta-analysis of economics carried by its own tools. There is only one brief passage in the book which takes up this challenge, to be found in the paper by John Davis (pages 162–65) in the section 'Market exchange and discursive interaction'. Nevertheless, while readers interested in the economics of knowledge – old or new – will be disappointed by *What do economists know?*, it provides a welcome cross-section of current work in the rhetoric of economics tradition.

MATTHIAS KLAES

Keele University

Structural Adjustment: Theory, Practice and Impacts. By MOHAN (GILES), BROWN (ED) and MILWARD (BOB). (London and New York: Routledge, 2000. Pp. xx+215. £17.99 paperback. ISBN 0 415 12521 9, 0 415 12522 7.)

This book attempts to engage with a post-neoliberal discourse in development thinking. It is a forerunner of a future literature that will no longer feel it necessary to critique Structual Adjustment directly – and thus beg the immediate question of what is the implied counter-factual? Future non-neoliberal writing is likely to accept Structural Adjustment as a historical fact with substantial local variations in its effects. The challenge and seek to understand the room for manoeuvre at all levels of economic life for different agents that it has actually left and/or created. This book deserves credit for its pioneering efforts in attempting to bring this approach to a student audience. A pragmatic predecessor in this approach, Duncan Green (Green, 1995), is rightly acknowledged – so frequently that the reviewer wondered whether he should have been credited as a co-author!

But the book does have conceptual limitations. While the concept of 'capitalism' is at the core of the book, the concept is never rigorously developed. The reviewer found parallels with Immanuel Wallerstein's World System Analysis but there is no reference to Wallerstein in the bibliography. Also the frequent use of the term 'Third World' does not carry conceptual conviction in a text which equally frequently refers to the heterogeneity of the world today as part of the critique of a universalising tendency in Structural Adjustment.

The book has value as a subsidiary text for second, and perhaps non-specialist third year, students in development studies in providing an introduction to the counter-discourse on Structural Adjustment. The numerous quotes give immediacy to the voices of objection to continuity and the text is readable and pedagogically accessible. Part of the pedagogic virtue is that important points are repeated – though this may owe as much to the loose editing as pedagogic intent.

Unfortunately for a review for an economics journal, the economics analysis is the weakest part of the book. The models used are very simple, but even so there is a failure to distinguish between real and nominal values of variables (page 12). There are also unexplained concepts such as terms of trade and total factor productivity used as if they were self-evident, while a simple diagram of monopoly equilibrium is wrong (page 31). The book is not suitable for a specialist economics course at any level.

In the middle chapters, the logic behind the choice of case-studies is unclear and the chronology of presentations is rather haphazard at times. But these chapters do succeed in giving a framework that a student could use to assess processes of change in social inequality and poverty (including gender), the environment, and political life that Structural Adjustment has put in train. These frameworks could be used by students for case-studies of their own choosing.

The best of the book is the discussion of 'alternatives to adjustment' in Part Three. This constitutes an excellent, succinct statement on the state of debate about the direction of change and who will make it. In the reviewer's judgement, this statement will stand for a number of years as a description of the debate, though it is agnostic on what will actually happen. The description of arguments and their supporters generally eschews both demonising and utopian positions about the future – though there is something of a programmatic utopian flourish in the last four pages. But, up until that point, the argument is very hard-headed on whether there are any alternatives to further drift in which some neoliberal loss of ideological confidence is more than matched by much radical political impotence.

John Cameron

University of East Anglia

Reference

Green, D. (1995). *The Silent Revolution: the rise of market economics in Latin America*, London: Cassell.

Growth Theory and Sustainable Development. By Bretschger (Lucas). (Aldershot and Lyme, NH: Edward Elgar, 1999. Pp. xiv+244. £55.00 hardback. ISBN 1 84064 135 5.)

This is a very impressive volume destined to become a standard treatise in the latest attempts to combine growth theory with the issue of sustainable development. It is intended as a text for first year graduate students but the students in question would have to be highly competent in quantitative techniques for despite claims to the contrary the mathematics involved goes well beyond the norm expected of undergraduate courses in Mathematics for Economists. It is remarkably well written combining clarity with conciseness and giving no indication that the volume has been translated from the original German. There are two main focal points. Firstly, it attempts to demonstrate how modern growth theory can contribute to an understanding of the vast differences in living standards coexisting in the world economy. Secondly, the limited supplies of natural resources carries implications for the question of whether the present rate of development will be sustainable in the future. The book's emphasis seeks to relate the theory of economic growth to predictions about this key issue. Although essentially a book on economic theory, considerable empirical evidence is included which emphasises the need for theory to be at least consistent with the stylised facts.

The starting point for the formal analysis of growth theory is the Harrod-Domar model extended to allow for a Leontief production function and consideration of the conditions necessary for a steady state solution. A critique of the unrealism of the model and the fact that none of the parameters of the model are determined endogenously paves the way for an analysis of the neoclassical model. The Solow model is invoked to demonstrate the steady state solution and exogenous technical progress. This is standard enough but very clearly done and leads on to a distinction of Hicks, Harrod, and Solow neutrality and the policy implications implicit in the Golden Rule of capital accumulation. Two excellent chapters follow on intertemporal optimisation (in which the Keynes-Ramsey rule and the Ramsey-Cass-Koopmans model are formally derived) and on positive spillovers whereby various economic activities generate (essentially as a side product), a cumulative process with positive impacts upon the overall growth rate. In this way growth becomes more of an endogenous variable, being dependent in part upon economic policy.

This naturally leads into the primary focus of the book, namely the new growth theory where long term growth is endogenously determined. The role of the public services and human capital is then analysed and integrated into growth theory. Considerable attention is given to the importance of positive knowledge diffusion stemming from the research sector. The analysis is extended to the open economy with consideration being given to the scale effects of international trade. Further extensions take account of financial markets, interdependencies between business cycles and growth, multiple equilibria, and poverty traps. This is all comprehensively dealt with on both a micro and macro level in a manner which emphasises the evolution of economic doctrine as an historical process. The final chapters of the book culminate in its pioneering conclusion which is to demonstrate how endogenous growth theory provides an appropriate framework for the analysis of sustainable development. The essential conclusion is that a profound study of sustainable growth is not possible without a sound thesis of endogenous growth theory. Development is deemed sustainable if it meets the needs of the current generation without compromising the ability of future generations to meet their own needs. This broad definition leads to quite different conclusions when distinguishing between renewable and non-renewable resources but the criterion remains firmly grounded in promoting or maintaining individual welfare. Accordingly, environmental variables have to be added to the theory and the quality of the natural environment has to be included in the utility function one is seeking to maximise. A very pleasing feature of this volume is the use of one-page boxes included in the relevant chapters summarising recent theoretical controversies and the relevant emprical evidence. These complement the chapters in which they appear admirably and reinforce the central message in a remarkably concise manner. Sustainable development has become a key area in the development literature in recent years. This volume succeeds in integrating it with modern growth theory in a way which brings out the importance of institutional and environmental factors and highlights the relevance of the policy implications.

G. K. SHAW

University of Buckingham

The End of Finance: The Theory of Capital Market Inflation, Fiancial Derivatives and Pension Fund Capitalism. By TOPOROWSKI (JAN). (London and New York: Routledge, 2000. Pp. xvi+160. £50.00 hardback. ISBN 0 415 20881 5.)

This book is about the causes and effects of a phenomenon that the author describes as 'capital market inflation'. Its title, 'The End of Finance', has several meanings. First, it alludes to a conflict between what the author perceives to be the proper end, or social function, of capital markets and the ends, or purposes, of finance in the present era of market expansion. Second, it voices the prediction that the End of that era is nigh. The enormous rise in asset values that the world has seen over the last three decades is deemed to be unsustainable. Third, the 'End of Finance' also hints at the obsolescence of Finance as an academic discipline that is based on conventional views of the market values of financial assets as being 'determined either by the productivity of some underlying real capital asset, or by past market values and fluctuations, or, in ''equilibrium'', by both' (page 134).

In the first part of the book, Toporowski presents his theory of capital market inflation. He rejects the textbook view of financial markets as the central capital-allocating mechanism through which long-term investment in productive capacity is financed. Toporowski claims that non-financial companies use financial markets mainly to manage liquidity, re-financing fixed capital investments which they have made with internal savings. However, if there is a significant net inflow of capital into financial markets over a longer period, stock prices tend to rise. This may 'encourage companies to re-finance in excess of their current needs' (page 34). According to Toporowski, such 'over-capitalization' is mainly used for directly unproductive activities, such as mergers and acquisitions. Thus, asserting that net inflows of money into capital markets tend to raise the price level of assets but not the underlying real activity, Toporowski arrives at a specific version of the quantity-theoretical neutrality postulate.

Considering the recent debates about 'cash burn rates', the 'new economy' and all that, the reader may doubt that companies generally refrain from buying additional productive capacity with the money that they raise in the stock markets. However, the more interesting point of Toporowski's diagnosis of socially unproductive capital market inflation is that he identifies funded pension schemes as its main driving force. In the second part of the book, he argues that the large inflows of capital from pension funds in the UK, US, and elsewhere have contributed to an increasing fragility of financial structures. The concomitant rise in stock prices has created widespread expectations of permanent gains, and hence encouraged the taking of speculative positions which could only be sustained by larger and larger inflows into the markets. According to Toporowski, the boom is due to end in the not-too-distant future, because there are limits to the growth of pension funds. Surprisingly, Toporowski does not make much of the problem of 'ageing societies', the exogenous factor of demographic imbalance that is normally stressed in this context. Instead, he emphasises a number of factors that are co-determined by developments in the financial markets. Since these markets put a premium on anti-inflationary monetary policy, expenditure has been squeezed by high real interest rates. Much of the 'era of finance' has therefore coincided with slow growth and widespread unemployment which has reduced inflows to pension funds. In order to preserve the stability of financial markets, new funds must be attracted, mainly by expanding the system of pension funds to countries that used to be on pay-as-you-go schemes. But the 'circle of sufficiently well-paid, stable employment' is narrowly confined. Moreover, the globalisation of markets has made it shrink by the demand for greater flexibility of labour, e.g. in terms of temporary or part-time contracts. 'Flexible workers' tend to have a higher liquidity preference in order to smooth consumption; they are less able or inclined to contribute to long-term oriented investment funds.

'The End of Finance' is a thought-provoking book. It challenges conventional wisdom by arguing that the 'sound principles' of funding pensions, fighting inflation, and reallocating risks contribute to unsound, or unsustainable, developments in financial markets. However, the main points are not always well-argued; they are often put forward as simple assertions, rarely supported by (anecdotal) evidence. It is obvious that Toporowski's idea of proper finance differs from reality and mainstream thinking, but the benchmark is not clearly defined. Moreover, Toporowski is so anxious to differ from what he describes as neo-classical orthodoxy that he overlooks that his observations about the role of conventions in asset price formation are now standard fare in the mainstream literature; or that the notion of 'fundamentals' in much of that literature has been extended beyond the 'taste & technology'-sets of Walrasian general equilibrium theory – to include, for example, the money supply. On the whole, the arguments put forward in 'The End of Finance' merit a more systematic treatment of the relevant theories and data.

HANS-MICHAEL TRAUTWEIN

University of Oldenburg, Germany

Corporate Governance and Financial Performance: A Study of German and UK Initial Public Offerings. By Goergen (Marc). (Aldershot and Lyme, NH: Edward Elgar, 1998. Pp. xii+183. £49.95 hardback. ISBN 1 85898 978 7.)

The choice between 'insider' and 'outsider' systems of ownership and control lies at the heart of the corporate governance debate. The 'insider' – or 'stakeholder' – system is characterised by high concentrations of voting share control, a limited role for external stock market transactions, publicly qualified support for the objective of shareholder value maximisation, and the near complete absence of hostile takeovers. By contrast, the Anglo-American 'outsider' system relies on dispersed equity ownership by shareholders with access to highly liquid and efficient stock markets and with the threat of hostile acquisition facing those corporate managers who depart too far from shareholder value maximisation. In essence, the two systems embody the alternative political and economic control mechanisms of *voice* and *exit*, respectively – see the 'Introduction' to Keasey *et al.* (1997). Thus the governance debate hinges on the effectiveness of each mechanism in defining and achieving corporate objectives.

This short monograph by Marc Goergen, a development of the author's DPhil thesis, makes an interesting empirical and policy contribution to the governance literature by examining the evolution of ownership and control changes among samples of newly-quoted German and UK companies. Goergen follows recent contributions in financial economics, in the wake of Demsetz and Lehn (1985), in allowing that share ownership concentration may be endogenous. In particular he considers how it may be related to the monitoring benefits of internal and external equity blocks as well as the ambitions and liquidity requirements of the firm's entrepreneurial founders. These issues are explored in some depth, initially in the context of recent theoretical and empirical findings and then as the author develops his own research questions.

There has been a great deal of empirical work on IPOs, much of it trying to disentangle the signalling and agency hypotheses associated with outside equity sales and satisfactorily explain the under-pricing phenomenon. By contrast, Goergen uses the IPO experience as a form of natural experiment that generates similar cohorts of newly floated companies in Germany and the United Kingdom. Since there are fewer IPOs in Germany, Goergen uses the entire population of such transactions between 1970 and 1995 as his basic data set. He then performs his comparative analysis with a UK sample, matched alternatively by size and industry. Although there are some differences in the two samples at and immediately after flotation – most obviously in the much greater mean age of the German sample – the similarities appear sufficiently strong to justify the author's approach. He then explores the subsequent evolution of share ownership and control changes across the two samples and tries to analyse some of the consequences for performance.

It is difficult to do justice to the author's empirical findings in a brief review. However, to this reader four results were particularly noteworthy. First, familial control declined much more slowly in newly-floated German companies than their UK counterparts, not merely because of lower outside equity sales but because the former also used non-voting shares to reduce dilution. Second, control changes occurred in both samples. However, in Germany they were associated with the consequences of block trades while in the United Kingdom they resulted from a very high rate of takeovers. Third, contrary to most perceptions in the literature, the role of German banks as shareholders appeared to be fairly minimal. (Whether this is consequence of the size of these newer companies or a reflection of disengagement by the German banks in corporate governance since the 1980s remains an open question.) Finally, in neither country was the author able to find any robust significant relationship between share ownership and operating performance among the newly floated firms.

In this, as in any empirical study, it is always possible to query individual findings. The performance equations, for example, rely on very rudimentary industry controls, suffer from sample selection problems through attrition in the UK sample, and do not appear to address the endogeneity issues associated with ownership that are raised elsewhere. Similarly, it was surprising that the author did not consider the role of the venture capitalist industry in the two countries. However, the overall contribution of the volume is considerable. It deserves to be read by anyone interested in comparative aspects of the corporate governance debate.

STEVE THOMPSON

University of Leicester

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The Economics of the Mind. By RIZZELLO (SALVATORE). (Aldershot and Lyme, NH: Edward Elgar, 1999. Pp. xxi+198. £49.95 hardback. ISBN 1 84064 163 0.)

Although the title might suggest otherwise, *The Economics of the Mind* does not advance an economic theory about the functioning of the mind. As Rizzello imediately makes clear in his *Introduction*, the book rather deals with how real men and woman decide and act and what implications this has for things economists are interested in. The central premise in the book is that as '... the foundations of human behaviour are inside our minds' (page 168), we cannot understand how people act unless we understand how the human mind works. As I see it, the central thesis argued for is that once we understand how the human mind works, it immediately follows that asymmetric information and path dependency are common, if not omnipresent phenomena in economic processes.

The book is in three parts, with an *Interlude* linking parts two and three. Part one discusses Hayek's criticism of the neoclassical paradigm. Rizzello pays special attention to Hayek's *The Sensory Order* (1952) and stresses the crucial role Hayek attributes to subjective interpretations in individual behaviour and in the spontaneous evolution of social order. The second part addresses Simon's criticism of orthodox economics' assumption that human decision makers have unlimited computing abilities. Simon's notions of satisficing and of bounded and procedural rationality are discussed in relation with Simon's views on organisational behaviour. Next comes the Interlude, primarily dealing with the replacement in the 1950s of behaviourism by cognitivism as the dominant doctrine in psychology. Part three fleshes out the implications of Hayek's and Simon's views on issues figuring prominently on the research agenda of heterodox economics (mainly neoinstitutionalist and evolutionary economics). The general idea here is that Hayek's and Simon's views nicely complement one another. Whereas Hayek highlighted above all the link between mind and institutions, Simon concentrated on the link between mind and organisations (page 40).

At times Rizzello suggests that he is looking for microfoundations of heterodox economics and that a solid microfoundation is all that is needed to get a more respectable heterodox economics off the ground. This is somewhat surprising, since orthodox economics is usually criticised by heterodox economists precisely for its alleged exclusive focus on individual decision making. One of the few common and recurrent themes in heterodox economics is that economic processes are affected pervasively by how society at large is organised. This suggests that what is more badly needed is a proper 'macrofoundation'. But Rizzello argues that the type of decision theory underlying orthodox economics is incompatible with the mechanisms of the human mind. More importantly, he argues in effect that a proper macrofoundation is included in the microfoundation he assembles from Hayek's and Popper's views. For according to both Hayek and Simon the mechanisms of the human mind cannot be understood without taking the important feedback loop into account that runs from the institutionalised environment to the development and functioning of the human mind.

Unfortunately, several misprints and false statements tarnish the book. In an attempt to characterise path-dependent models (on page 124), for example, it is written that '... trifling fortuitous events may lead to situations of [...] efficient equilibrium', while what should have been written is: inefficient equilibrium. Moreover, some of the main things Rizzello argues for seem to be on shaky ground. For example, Rizzello seems to argue that results of current research in the brain sciences only confirm, or at most enrich what Hayek and Simon already foresaw. What is confirmed in particular, Rizzello argues, is the view that every single human being is unique in its subjective interpretations and cognitive limitations. For Rizzello this seems to be sufficient reason to argue for the ubiquity of asymmetric information and path dependency in economic processes.

Current research in the brain sciences does not seem to suggest or sustain this view, however. What it rather suggests, it seems, is that similarities in brain circuits and mind processes among human beings are at least as significant as their dissimilarities. Rizzello may well be right that asymmetric information and path dependency are ubiquitous phenomena. But it is questionable that their ubiquity can be founded on the present state of the art in the brain sciences.

It seems fair to say that research in the brain sciences has only started. We still may have a long way to go before we come to understand how the mind works. And it will even take longer before we come to understand its implications for the things economists are interested in. But I think Rizzello must be given credit for drawing our attention to ongoing research in this fascinating and exciting area.

JACK VROMEN

Erasmus University Rotterdam

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Thinking about Inequality: Personal Judgment and Income Distributions. By AMIEL (Y.) and COWELL (F. A.). (Cambridge and New York: Cambridge University Press, 1999. Pp. xiv+181. £40.00 hardback, US $64.95 hardback, £14.95 paperback, US $24.95 paperback. ISBN 0 521 46131 6, 0 521 46696 2.)

Suppose that a certain amount of income is transferred from a person who has more income to a person who has less, without changing anyone else's income. After the transfer the person who formerly had more still has more. Do you think that income inequality has fallen? According to the modern theory of inequality measurement the answer should be unequivocally 'yes'. Opinion appears to be far from unanimous, however, when the same question is posed to the layperson. Only 3 students out of 5 – interviewed in a bunch of universities around the world – would verbally support that conclusion, and only a third would answer accordingly when faced with a numerical example. This evidence shakes the very foundation of the economists' approach to measuring inequality. But let us take a step back.

The 'principle of transfers' – whereby inequality is diminished by a progressive transfer from a richer to a poorer person that does not alter their relative positions – was originally stated in a pioneering article published in this JOURNAL by Hugh Dalton in 1920, picking up a lead by Arthur Pigou. Thanks to the seminal work by Anthony Atkinson (1970), it became the cornerstone of the modern theory of inequality measurement. As put by Amiel and Cowell: 'The greater part of the received wisdom on the positive and normative approaches to economic inequality is founded upon this principle and its corollaries' (page 138). It is no exaggeration to claim that researchers on income distribution tend to treat with some disdain inequality measures that fail to meet this principle, a good case in point being the variance of logarithms (e.g. Foster and Ok, 1999), an otherwise popular measure among labour economists.

Nowadays, a 'good' inequality index is one that satisfies the transfer principle as well as a few other 'reasonable' properties: symmetry or anonymity (people identical in any respect other than their incomes are treated equally), the population principle (invariance under replications of the population), scale independence (inequality is unaffected by a proportionate increase or decrease in all incomes). Though additional (e.g. decomposability) or alternative (e.g. translation independence) criteria have also been suggested, it is generally agreed that identifying a set of relevant properties is the way to restrict the number of reasonable inequality indices or, analogously, welfare functions.

Do ordinary peoply agree with these 'reasonable' properties? This is the central question addressed in Amiel and Cowell's book. *Thinking About Inequality* recounts the authors' experiments over eight years, in direct or indirect interviews with about 4,000 university students in Australia, Germany, Israel, New Zealand, Poland, Sweden, the United Kingdom, and the United States. Students were chosen because they are '... halfway between the unprejudiced but innumerate and the hidebound expert' (page 29).

Each student was given a questionnaire containing both numerical problems, involving pairwise comparisons of inequality between two simple distributions, and verbal questions, closely matching the numerical problems. A typical example of numerical problem is the comparision between distributions $A = (1, 4, 7, 10, 13)$ and $B = (1, 5, 6, 10, 13)$, the verbal counterpart of which is the question above. The 'orthodox' answer would be that B is more equal than A, since it can be obtained from the latter by transferring 1 unit from the third richest person to the fourth richest person. However, as mentioned above, only a third of students opted for this answer. Agreement with other properties was mostly higher, but none gained overwhelming support. The authors' conclusion sounds discomforting: 'The basis of inequality comparison that is almost universally adopted in the theoretical and applied literature seems to be at variance with the way untrained people interpret inequality comparisons' (page 128).

Is it really so? Not necessarily. Firstly, however insightful, the evidence from students' responses is clearly partial. It is one thing to compare inequality across simple distributions comprising 5 or 10 round numbers, and to explain the reasons underlying the ranking; it is another to make comparisons across large samples with hundreds or thousands of widely divergent observations. In the latter case, setting a number of principles may be the only way to obtain interpretable answers. In no way does this imply that these principles should be unanimously shared, or coincide with 'received wisdom'. Secondly, the true lesson from Amiel and Cowell's experimental evidence is not that there exists a conflict between theoretical hypotheses and people's own judgements, but that people disagree on how they evaluate inequality. It would be wrong to conclude that the theory of inequality measurement must be re-founded. Rather, it should be extended by allowing for alternative principles – possibly conflicting, but certainly clearly understood – to enlarge the range of informed comparisons.

Thinking About Inequality is very worth reading. It provides a neat and comprehensible introduction to the principles of the modern approach to inequality measurement. It questions their meaning and shows that they have no universal acceptance. It indicates a promising avenue for future research, when it stresses that income differences are typically judged not in isolation but in relation to the entire distribution.

ANDREA BRANDOLINI

Bank of Italy, Research Department

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The Evolving Rationality of Rational Expectations: An Assessment of Thomas Sargent's Achievements. By SENT (ESTHER-MIRJAM). (Cambridge and New York: Cambridge University Press, 1998. Pp. x+242. £35.00 hardback, US $54.95 hardback. ISBN 0 521 57164 2.)

Although the concept of rational expectations first appeared in the modern economics literature in the early 1960s, its impact on macroeconomics was negligible for nearly a decade. Having gained attention, however, controversies about rational expectations occupied centre stage, remaining an obsession of many macroeconomists until the 1980s and beyond. That policy pronouncements reliant upon the hypothesis of systematic errors in expectations held by decision-makers should be regarded with suspicion is unexceptionable. What is more open to debate is how expectations should be modelled to steer a passage between the Scylla of perfect foresight and the Charybdis of interminable error.

The book charts the route taken by one of the foremost contributors to the field, Thomas J. Sargent. A simple-minded perspective on Sargent's quest is that he is a social scientist endeavouring to understand the world around him, adopting – and adapting – the tools at his disposal. His is a journey founded on problem solving: if evidence conflicts with predictions, then the underlying model should be replaced; if the model's implications are too weak to be testable, it should be modified; if the techniques become intractable, more manageable ones should be sought. But Sent's goal is altogether more ambitious in attempting a detailed historical analysis of the development of Sargent's ideas up to the mid-1990s.

Following an extensive introductory chapter, the core of the book comprises a set of four case studies, each of which examines a particular phase in the development of Sargent's ideas, in more-or-less chronological order. The four chapters are entitled: Accommodating Randomness, Accommodating Prediction, Accommodating Symmetry, and Accommodating Learning. The first two are fairly straightforward studies of the problems Sargent addressed when building models that include random errors and in devising forecasting equations that respect the *a priori* restrictions implied by his theories. In the second pair, Sent ranges more widely into the philosophical questions of the interaction among theorists, econometricians, and the decision-makers whose actions they seek to explain. The notion of 'symmetry' is utilised in several ways to explore a variety of correspondences including those that exist among economic agents, theorists, and econometricians. In the context of learning, Sent probes yet more deeply into how economists attempt to model learning behaviour, drawing on theories of bounded rationality and artificial intelligence. Ultimately, the author makes Sargent the object of her analysis using the very same economic principles.

Having reached this point, the reader will wonder what Sargent himself makes of it all. Appropriately, the next chapter is devoted to an interview with Sargent by the author (one of his erstwhile students). While the conversation covers most of the topics raised in the preceding chapters, colloquial incoherence mars the clarity of several answers – no doubt the overriding intention is to preserve the responses from possible editorial distortion. Even so, Sargent emerges clearly enough as a pragmatic academic economist, one who is driven by a desire to understand real-world phenomena, who is intrigued by the tools of his trade but not bewitched by them, who reflects upon methodology but is not inhibited by it.

Fortunately, Sargent is still an active researcher and economics can expect to benefit further from his continuing odyssey. In a sense this book presents an interim report, timely as a record of events while they are still fresh in the minds of the participants but too early to form a definitive judgement of Sargent's achievements, let alone about the broader significance of rational expectations in macroeconomics.

While the author asserts early on that her 'approach is different from that of methodologists' (page 14), there is much in the book that has a bearing on economic method. Indeed, readers may be tempted to infer the existence of a prescriptive methodology lurking not far beneath the surface. Evident care has been taken to avoid venturing into that realm, although it is claimed that 'Sargent was unable to meet his own standards' (page 183). The implied criticism seems to target Sargent's pragmatism: when insurmountable obstacles are encountered (e.g. early in his career with parameter estimation in Lévy stable distributions), he changes direction. Sent does not presume to suggest what else he should have done.

With its focus on a single individual, *The Evolving Rationality of Rational Expectations* could provide a misleading impression of the field as a whole. The author does emphasise, however, that rational expectations can be interpreted in various ways. Also, several other important players (especially Lucas, Hansen, and Sims) receive at least passing mention. These caveats aside, the book constitutes an exceptionally thorough intellectual deracination of one person's work. As such, the exercise is likely to find most appeal among historians of science in general and of economic thought in particular. Macroeconomists will probably carry on regardless.

R. E. BAILEY

University of Essex

Paths of Innovation: Technological Change in 20th-Century America. By MOWERY (DAVID C.) and ROSENBERG (NATHAN). (Cambridge and New York: Cambridge University Press, 2000. Pp. x+214. £19.95 hardback, US $27.95 hardback, £12.95 paperback, US $16.95 paperback. ISBN 0 521 64119 5, 0 521 64653 7.)

Two approaches to understanding economic growth dominate the literature. One is to construct models based on a macro production function in which technological change acts as a single force. The other holds that technological change must be understood in detail before either the macro growth process can be understood or efficient policies designed to influence it – those who theorise on the basis of this approach often use evolutionary models.

Nathan Rosenberg, and his one-time student David Mowery, have long been advocates of the second approach. While not denying that insights may be gained from the first, their work suggests that to model growth without a detailed knowledge of technology, and technological change, is to attempt to write Hamlet without the Prince of Denmark. Rosenberg also understood the importance of treating technological change as endogenous, and provided voluminous evidence that it was, long before endogenous growth entered marco growth models.

The volume is important reading for those who follow either of these approaches. Because its 179 pages cover four major areas of advance – the internal combustion engine, chemicals, electric power, and the electronics revolution – plus a chapter on the institutionalisation of innovation, the book provides an overall perspective rather than a detailed study of any one area.

The central theme is the importance of two sets of forces whose influences, the authors argue, '... cannot and should not be separated' (page 179). The first set is US natural resources, market size, smaller income dispersion, and less class structure compared with Europe, all of which encouraged the development of the 'American system of manufactures'. It is relatively simple standardised goods were produced by methods that were more conducive to technological advance than were the methods of the more craft based European system of manufacturing. The second is the US system of innovation which combined private sector initiatives, university research, and government policies on anti-trust, procurement, and R&D. Anyone who believes that all that is needed for growth is to get the government's dead hand out of everything will find this book a much needed antidote.

Chapter 2, on the institutionalisation of innovation, documents among other things the mixed relation between firm size and innovation and the importance of US public policy and procurement initiatives. Anti-trust policy has contributed, sometimes inadvertently, to the unique structure of the US innovation system, while state funding of R&D has often been important. After 1940, the American R&D system diverged markedly from those of the major European countries in three ways: small new firms were important in the development of many new technologies; government funding exerted a pervasive influence on many sectors, especially hi tech; and US anti-trust policy was particularly stringent, among other things, forcing firms to use internal R&D rather than acquisitions to develop new technologies.

Here are a few of the book's other interesting themes. First, several key technologies were developed mainly in Europe but most fully exploited in the United States. The list includes chemicals and the internal combustion engine, which enabled two of America's great 20th century successes, the automobile and the aeroplane.

Second, research '... that is responsible for technological improvements tends to be highly concentrated in a small number of industries, but each of these few industries generates technologies that often diffuse throughout the economy'. (pages 68–69)

Third, new start-ups have been an important force for change in many US industries, in contrast to Schumpeter's view that technological change would fall increasingly into the hands of large corporations, and in contrast with the European experience where start ups are less common.

Fourth, general purpose technologies (GPTs), such as the internal combustion engine and electricity, have the power to reconstruct the entire society. The automobile did this to US society by 1939, while remaining a relative luxury good in the rest of the world until after WWII. ITCs are working similar changes today.

Fifth, GPTs tend to enter by a few doors and then spread slowly through the whole economy, being improved and adapted as they go and often requiring the invention of many complementary technologies and major restructuring of institutions, organisations, and ways of doing things. Thus their effects on productivity growth are often delayed, even if they are eventually dramatic.

Sixth, government activity has been important in many US technological advances. 'The entire biochemical industry has been supported by huge federal expenditures on R&D'. (page 99) The US synthetic rubber industry was created by the government during WWII. The '... rapid growth in U.S. polyethylene output after WWII is attributable in large part to the liberal licensing of polyethylene patents mandated by the US Department of Justice as one of the terms of settlement of its antitrust suit against Du Pont and Imperial Chemical Industries'. (pages 87–88) Post war government procurement and research support did much to assist the development of the computer and software industry. Unlike European governments, the US government has been willing to award contracts to new inexperienced firms thus aiding start-ups and encouraging the dynamism of several industries.

These are just a few of the lessons suggested by this fascinating study of 20th century US technological change – lessons that need to be heeded by model builders and policy makers alike.

RICHARD G. LIPSEY

Simon Fraser University, Vancouver BC

Foreign Direct Investment: Firm and Host Country Strategies. By BLOMSTROM (MAGNUS), KOKKO (ARI) and ZEJAN (MARIO). (London and Basingstoke: Macmillan, 2000. Pp. xii+253. £45.00 hardback. ISBN 0 333 82012 2.)

Let me tell you a story. Once upon a time there was a good firm that came into the economy and spread economic prosperity. It was more productive than the other firms so aggregate productivity improved, it brought in new capital and created employment where before there were unemployed resources. It brought with it new technology, management techniques, and human capital which benefited everyone, it limited the power of domestic monopolies and encouraged firms to be more competitive. The good firm exported a large proportion of its output, helping the balance of payments and encouraging other firms to export. It even paid higher wages. And the name of this firm? A multinational.

Let me tell you another story. Once upon a time there was a bad firm. It brought with it economic exploitation. It caused domestic firms to go out of business leading to greater market concentration. It transferred all its profits abroad and used the most nefarious methods to avoid paying any tax. It paid low wages, used second-rate technology, worsened the balance of payments by importing all its inputs, and hired only foreign workers for top jobs. And what's the name of this firm? Well this is also a multinational.

The diversity of opinions held about multinational companies, from blind faith in their positive impact on the economy (governments offering financial incentives and tax breaks) to the personification of evil (anti-WTO protestors and some governments) is astonishing. The need for sound empirical evidence to clarify the issues is overwhelming and that is exactly what this book contributes. The collection of papers – by some of the leading figures in this field – addresses both the behaviour of multinational firms, and the impact of that behaviour on the host country. The majority of the papers are published elsewhere, but collecting them together provides an invaluable service to the interested reader by showing the connections between the various subjects. The authors have provided a short introduction that admirably summarises the main themes and gives the theoretical background. The remaining chapters are all empirical – either using data descriptively or estimating econometric models.

The first half of the book considers the behaviour of multinational firms:

whether they choose majority or minority ownership, whether they enter into joint ventures, the choice between acquisition and greenfield investment, intra-firm trade, and R&D expenditure in affiliates. One empirical characteristic that emerges is that even among multinationals from the same country – in this case Sweden – there is a great deal of variation in behaviour. More diversified multinationals are more likely to opt for acquisitions and joint ventures, although generally the latter is an unpopular form of investment. The characteristics of the host country also play a part, with joint ventures decreasing in probability and imports from the parent company increasing with the development of the host country. This half of the book relies mainly on the excellent Swedish data on multinational firms. The wealth of detail in this data set allows the authors to consider issues such as intra-firm trade; however, it also potentially limits the generality of the results. Do all multinational firms behave like Swedish ones, or does originating in a small home country differentiate them from say US and Japanese multinationals? Until other countries implement similar surveys we have no way of answering this.

It is the second half of the book that considers the issue of welfare: are multinationals a good or bad thing from the perspective of the host country? The answer appears to be a qualified 'maybe'. As the authors point out in the introduction, the *potential* welfare gains to the host country are considerable, but they are not always realised as variation in the type of multinational companies and host countries and the strategies they adopt can influence the welfare gains. The two chapters on Mexico illustrate this diversity. Chapter 9 finds evidence of positive spillovers from multinationals to domestic firms and convergence between the two groups of firms. However, chapter 10 qualifies this enthusiasm, showing that when sectors have both a large technology gap between domestic and foreign firms and the foreign firms have a high share of output, domestic firms appear to be 'crowded out'. In this case the foreign affiliates exist in 'enclaves' separated from the rest of the economy. Likewise the chapter on Uruguay (chapter 11) finds that spillovers are small when the technology gap between domestic and foreign firms is large. It appears that unless the host country has reached a certain technological level (or absorptive capacity) will not gain from the technology of multinational firms. This message is reinforced by the two chapters on technology imports that conclude the book: technology imports (which are assumed to raise the potential for technology transfer) increase with both the level of competition and the level of education in the host country.

The main policy conclusion to emerge from these chapters is that perhaps governments should try to positively influence the economic environment – reducing technology gaps, increasing competition, raising human capital – rather than enacting specific legislation for multinationals. The answer to whether multinationals are a 'good thing' appears to be that they are only as good or bad as circumstances allow.

Katharine Wakelin

University of Nottingham

A. W. H. Phillips: Collected Works in Contemporary Perspective. Edited by Leeson (Robert). (Cambridge and New York: Cambridge University Press, 2000. Pp. xvii+515. £60.00 hardback, US $95.00 hardback. ISBN 0 521 57135 9.)

Robert Leeson has produced a very important collection. Its format is unusual. All the surviving professional writings, published or not, of Bill Phillips are here. Interwoven are comments from no fewer than twenty-nine contributors, some short (less than a page from Phillip Cagan to estabished Phillips' priority as the discoverer of adaptive expectations) and others quite long (twenty six pages from Nicholas Barr on the history of the Phillips machine) and they ensure that this book is not just an archive of Phillips' contributions, but also a penetrating study of their place in the recent history of economics.

Phillips' widespread fame rests on that eponymous curve. An inverse relation between inflation and unemployment was known to exist long before 1958, but Phillips may have been the first to draw a curve through the scatter, thereby unintentionally providing an empirically based functional relationship for others to interpret as a policy menu. It is one of the less pleasant ironies of the history of economic thought that, because of a paper that was peripheral to his own much deeper research agenda, Phillips' name will forever be associated with damaging fine tuning policies whose many pitfalls his own work was designed to reveal and warn against. It is this work, which less than fills one volume, that makes him one of the most important economists of the last half-century.

Phillips brought to macroeconomics the perspective of an electrical engineer, used to analysing the control problems posed by dynamic system. The famous hydraulic machine, the first fruits of his efforts to make sense of macroeconomics for himself, was primarily a device for conveying this vision of the sub-discipline to others, and only incidentally a pioneering analogue computer; his analytic models were designed to investigate the extreme sensitivity of their stability properties to apparently small variations in parameter values and in the feedback mechanisms used to control their behaviour; his research in econometric theory was aimed at increasing the reliability of the quantiative information that real world policy makers would need to carry out stabilisation policy; and so on.

In common with Milton Friedman, with whom he seem to have first had contact in 1952, Phillips understood from a very early stage just how difficult it would be to implement systematic macro-stabilisation policy, but unlike Friedman his initial response was to seek to understand those difficulties with a view to overcoming them. By the late 1960s, he had almost given up the effort, though it is not quite clear to me why, even after reading through this book. Perhaps he knew how badly his health, already irrevocably damaged by his time as a prisoner of the Japanese, was failing; perhaps he felt that he had said all that he had to say on the topic; or perhaps he had come to believe that the technical problems inherent in applying optimal control techniques to real-world economies were insuperable. Or perhaps it was a bit of each: certainly, his last unfinished paper, dater 1972, develops an econometric result somewhat similar to the Lucas critique: once an economy is subjected to optimal control techniques 'observations during the period of control cannot be used to obtain improved estimates of the parameters [of its reduced form for national income], which is a serious drawback' (page 486).

But this isn't quite the Lucas critique, at least as I understand it, for there is no suggestion here or anywhere else in Phillips' writings that the basic problem with optimal control is that economic agents are intelligent maximisers. His maximisers are the policy-makers, and the economy is always treated as a structure that constrains their choices. That doesn't rule his work as far out of court for me as it does for many. Tight links among deep parameters, expectations and reduced forms only exist in macro models when aggregation questions are evaded by use of the 'representative agent' assumption. This procedure is defensible on an 'as if' basis, but so is Phillips' approach. The choice here is an empirical one.

Nor does policy-relevant evidence all go in the same direction. If, in the 1970s, some took too little notice of policy dependent expectations, did others not perhaps expect too much of them in the following decade? And even if most economists have given up optimal control, investigations of the stability properties of, for example, economies subjected to monetary policies implemented according to Taylor rules and the like, are quite routine in central banks and university classrooms these days. It is hard to imagine what modern macroeconomic policy analysis would look like had it not been for Phillips. Every departmental library should therefore have this record of his remarkable life and work, and every macroeconomist should spend a little time reading it.

David Laidler

University of Western Ontario

Capital Ideas and Market Realities: Option Replication, Investor Behavior, and Stock Market Crashes. By Jacobs (Bruce I.). (Oxford and Cambridge, MA: Blackwell, 1999. Pp. xx+399. £55.00 hardback, £19.99 paperback. ISBN 0 631 21554 9, 0 631 21555 7.)

Bruce Jacobs' *Capital Ideas and Market Realities* is an important contribution to a fundamental debate concerning the workings of financial markets. Jacobs' polemical but scholarly critique of orthodoxy is endorsed enthusiastically by Markowitz, no less, in a powerful foreword so it has to be taken seriously. Jacobs himself is an erstwhile academic who has become an important market player. Orthodoxy is taken by Jacobs and Markowitz to be the view that the efficient market hypothesis is a good guide to the workings of financial markets, and that in consequence they are benign in their impact on society. In particular financial innovation is likely to generate both private gains and social benefits through improving informational efficiency. The alternative is that markets over-react and may be better characterised by bubble models. Markowitz focusses on the basic point that instability (in the Micro 2 sense) can result if market participants buy when prices rise, and sell when prices fall. Hence any financial technique is to be viewed with suspicion if it induces such behaviour. Various portfolio replication *modus operandi* inspired by Black-Scholes do this and are prone to destabilise market strategies, whereas strategies inspired by Markowitz' own work stabilise markets. Marokwitz' brief piece is extremely useful in putting Jacobs' work into context. Jacobs' main proposition is that unlike portfolio, insurance was a major cause of black Monday in 1987 when stock market prices fell by over 20% in single day. Jacobs devotes 25% of his book to an exposition of portfolio replication in general, and portfolio insurance in particular.

Over half of the book examines 1987 and the rest of the book – called an epiloque by the author – to what Jacobs terms 'sons of portfolio insurance'. As the latter included the collapse of *Long-term Capital Management*, I think this balance of the book is disappointing. I do not wish to decry the importance of debates about the nature of Black Monday; classic bubble or efficient reaction to news. Jacobs' collates an extremely lucid and useful survey of the literature on the topic. He presents it in an exceptionally non-partisan manner given the strength of his own opinions. However, Jacobs adds little fresh to this debate. On the other hand the implications of the collapse of *Long-term capital management* are still an open question. There is much written about it ranging from a Presidential task force through Dunbar's up-market journalism to Edwards' *Journal of Economic Perspectives* and Mckenzie's pioneering synthesis of economics and sociology. A survey and critique of this would have been both useful and telling. There is a danger that Jacobs' work will be viewed as dated or at best history. This is a shame as his insights are still crucial and relevant.

DAVID GOWLAND

University of Derby

Main Currents in Cumulative Causation: The Dynamics of Growth and Development. By TONER (PHILIP). (London and Basingstoke: Macmillan, 1999. Pp. xiv+228. £50.00 hardback. ISBN 0 333 74688

Main Currents in Cumulative Causation is a fine book without an obvious audience. Philip Toner's exegesis of the ideas of Allyn Young, Paul N. Rosenstein-Rodan, Albert O. Hirschman, Gunnar Myrdal, and Nicholas Kaldor is careful and detailed, but does not address the main questions of its most likely readers.

Toner devotes one chapter to each of the aforementioned economists' theories of economic growth. Driving all of their theories is the principle of circular and cumulative causation, whereby 'a change in an economic and social system induces further self-supporting changes' rather than counteracting ones. Having embraced this principle they necessarily removed themselves from the mainstream of neoclassical economics, with its privileged position for stable equilibrium, and charted their own courses.

According to Toner, all five theorists agreed that the basis of growth through cumulative causation is increasing returns to scale, and to a lesser extent that increasing returns are manifested most importantly in pecuniary externalities. Their common idea was that as the domestic market expands, firms will find it profitable to produce such goods, or to produce in such a way, that will extend further profit opportunities to other firms, fueling the market's continued and cumulative expansion. The theorists disagreed on several key details of the rationale, though, and they had different policy prescriptions.

Young argued that growth allows greater division of labour, so that ever more and varied intermediate goods can be introduced as productive inputs, defying diminishing returns and allowing further growth. Rosenstein-Rodan emphasised the need to achieve 'balanced growth' simultaneously across many industries, each of which faces economies of scale, because their products are complements in demand. Hirschman to the contrary advocated 'unbalanced growth', prioritising investment in sectors with the greatest backward linkages to intermediate goods and basic inputs, for which domestic production could then be substituted for imports. Myrdal was a broad thinker who was less specific on the economic mechanism of cumulative development but more willing to consider social and institutional factors; in addition his critique of mainstream economics was more profound. Finally, Kaldor, in whom the theory of cumulative causation 'reached its apotheosis', criticised Young for neglecting the importance of effective demand, which cannot be taken for granted as production possibilities increase. Kaldor's unease about domestic effective demand led him to advocate export-led growth rather than deliberate import substitution.

At times Toner's exposition becomes a bit tired and inattentive: in at least three instances he duplicates, word for word and apparently unknowingly, a quotation he introduced some pages earlier. And while the chapter on Myrdal is concise (at eleven pages) and very well done, the longer chapters, particularly the forty-four pages on Kaldor, wander and substitute myriad section headings (e.g. '6.7.4.2 Criticisms of the Two Sector-Two Stage Model') for careful organisation of thought. Overall, though, his style is good enough to let the reader cull ample knowledge of the five's growth theories.

But to what end? Toner seeks to demonstrate 'the continuity and evolution of fundamental principles' of the five men's ideas, to identify and evaluate their contributions to thought about cumulative causation (CC), and to argue 'that contemporary CC research would greatly benefit from drawing on many of the ideas and methods of the doctrine's pioneers'.

The book is unlikely to be successful among historians of economics because it does not meet its first purpose, to reveal *how* ideas of cumulative causation 'evolved'. It does not explain in detail how the ideas came to be conceived, believed, or rejected. It is not so much history as capable exegesis.

Its success, then, will depend on its fulfillment of the second and third purposes. With its generally favourable evaluation of the five economists' contributions, the book must convince contemporary theorists to let those contributions inform their work (or at least evince from them some passing curiosity).

Here it stands a chance – but not a great one. Toner affirms briefly in the conclusion the book's pertinence to recent endogenous growth theory and East Asian development studies, but he does not mention the contemporary theorists who might otherwise be his most attentive audience. Authors of the 'new economic geography' purport to capture the mechanics of cumulative causation driven by pecuniary externalities in their models of location and trade; indeed Paul Krugman's (1995) history of the genre he helped create claims the mantle of Young, Rosenstein-Rodan, Hisrchman, and Myrdal as his own. Toner might have argued at chapter length that there is something essential in their work that the new economic geography is missing but should incorporate. Alternatively he might have addressed historians of economic thought, questioning whether Krugman's history, wherein Young and the rest were 'lost' (because they did not model mathematically) and are now 'found' (because Krugman, via Dixit and Stiglitz, has worked out the math), bears scrutiny. As Toner has not taken aim at these likeliest targets, he does not score a hit.

STEPHEN MEARDON

Williams College, USA

Reference

Krugman, Paul (1995). *Development, Geography, and Economic Theory.* Cambridge, MA: The MIT Press.

Credit, Money and Production: An Alternative Post-Keynesian Approach. By ROCHON (LOUIS-PHILIPPE). (Aldershot and Lyme, NH: Edward Elgar, 1999. Pp. x+341. £59.95 hardback. ISBN 1 85898 895 0.)

This book is an exposition at some length, of the debate among the Post-Keynesians about the nature of money and credit. The central proposition is that money is endogenous i.e., demand determined rather than exogenous as [according to the post-Keynesians at least] neo classical economics asserts. But around the agreed proposition of the endogeneity of money, there are differences within the post-Keynesian camp. There are circuitists and structuralists. These differences are discussed with copious references to the writers of the two camps.

The first chapter is introductory. The Franco Italian circuitist school is described and put in the context of Keynesian writing. The second chapter describes the two schools in some detail. The next two chapters go into the history of these ideas and the writings of Minsky, Kaldor, and Tobin as well as Davidson, Rousseas, Joan Robinson, and Kahn are dealt with. Of these only the last two meet with the author's approval. The next chapter describes the horizonalists/circuitists and structuralists and contrasts them. Then follows a chapter on New Keynesian monetary theory as compared with post-Keynesians. A concluding chapter deals with the post-Keynesian Circuitist theory of banking.

The essential notion is that it is producers who decide to demand credit to finance production and undertake investment. This demand is met by banks who face no other constraint than that of exogenous interest rate administered by the Central Bank. [Hence a horizontal supply curve of credit and horizontalists.] This is a mild constraint since the Central Bank worries about systemic stability. The loans are spent on purchase of inputs and become deposits with banks. When spent on final output they return to the producers who pay back their loans, the Banks then extinguish their loans, and the whole thing starts all over again. [Thus a circle and hence circuitists.]

Thus production leads to credit which in its turn creates money. Structuralists however, believe that it is the consumer's portfolio decision to hold money or bonds which plays some role in creating the demand deposits. The horizontalists think this is giving too much causal primacy to money rather than credit. Structuralists also think banks may constrain credit for portfolio reasons. Horizontalists do not believe in constraints on credit. The circuitists are Keynesian but they also believe that the Multiplier is one. So there is Say's Law and credit.

This is all there is to it. But it is repeated several times with quotations from all the participants in the debate. Keynes is admired for his post General Theory articles in this JOURNAL on the finance motive and even more for *The Treatise*. Portfolio theories are rejected since they give consumers any role in determining the course of money. Although the book seems scholarly, there is no mention of Henry Thornton who said a lot about credit in 1802 nor to Marx who was the first to draw up circuits of capital with money.

The method is purely quotation based. No algebra or geometry, much less econometrics or even statistical tables are allowed to dirty the flow of argument. Indeed no event of recent monetary history of the last seventy years is as much as mentioned though the author claims against the neoclassicals that the post-Keynesians practise realism. The enormous growth of financial markets and the many innovations of financial assets are as nothing. We have money and bonds as old Maynard told us. There is no discussion of open economy problems and hence the Central Bank can fix any interest rate it chooses. Globalisation has not yet hit the world of the post-Keynesians as far as this book is concerned.

The author is obviously a bright young economist who has read everything on his topic. But the literature he describes is entirely self-referential. The neoclassicals may have faulty methodology; they may abstract too much, put too much faith in consumer rationality. But they never lose sight of the real world problems. They test their hypotheses. Lucas does not need to quote his friends or even his enemies endlessly. The only whiff of realism comes when the author discusses new Keynesians since they worry about credit rationing and the Fed, but then they are not pure Keynesians.

There is a clear need for a heterodox view and some post Keynesians – Minsky or Davidson or Chick – have even tried to address real world issues. But they are castigated here as not truly purely Keynesian. This kind of book is only for the deeply converted. No one who is outside the sect will be much interested in it except as an illustration of the causes for the sterility of much heterodoxy. Such a pity!

MEGHNAD DESAI

London School of Economics

Education and Development: Measuring the Social Benefits. By MCMAHON (WALTER W.). (Oxford: Oxford University Press, 2000. Pp. xiv+299. £35.00 hardback. ISBN 0 19 829231 7.)

This book is the culmination of more than a decade of research in the United States and in major international organisations. As the final chapter makes clear the author hopes it will influence policy makers throughout the world. In many ways it is an econometric *tour de force* in the economics of education as well as containing much well argued and convincing text.

In plain terms McMahon develops, in two main ways, the economic growth models of Lucas and Barro which posited, and demonstrated empirically, a central role for education in the economic growth process through the dissemination of new knowledge and of human capacities to adapt. One development is simply useful replication of Barro's work using an impressive amount of time series data, covering the early 1970s to the late 1990s, in ninety countries around the world. The second, and the central theme of the book is what McMahon calls a 'logical extension of the concept of endogenous growth ... in the form of a closely related but different concept of endogenous *development*' (my italics). The difference McMahon defines as consisting of other benefits of a public good nature, such as population control, health, democratisation, human rights, political stability, reduction of inequality, reduction of crime, and various environmental factors, on all of which the author has assembled an impressive list of indicators in each of his ninety countries since the early 1970s. The McMahon hypothesis is that just as, with suitable time lags, education can be shown to contribute positively to economic growth, and does not need to be treated as exogenous technical progress as it was in the older Solow type models, it also has net beneficial effects, that can themselves become endogenous to the model, on many other factors that improve the quality of human life. McMahon is well aware of the conundrum that quite apart from data problems, there is the interpretative difficulty of distinguishing between direct effects of education on the various public goods and the indirect effects through raising income. Is it being more educated, or being richer as a result of more education that results in better health? He also accepts that the effects of education, on some of the variables may be negative as well as positive at least some of the time.

The first two thirds of the book spell out the model theoretically and empirically in fairly rigorous terms, but the text is accessible to any education economist who is prepared to put a bit of effort into it. The third part, *The Compete Model* consists of a series of simulations in which the endogenous growth model predicts for the next quarter century the likely paths of development in the ninety countries. The conclusion broadly is that continuation of present economic and social policies is as likely to lead to a widening as it is to a narrowing of the development gap between the rich and the poor countries with relatively rare exceptions in countries that have recently shown signs of making wise education decisions.

The model is then used in selected countries to assess the predicted effects of increased expenditure on education, and improvements in its efficiency and equity, on GNP and the main development indicators. The implications in the fifteen countries selected for this part of the exercise are almost uniformly beneficial. For example GNP rise, often spectacularly over 25 years, infant mortality and homicides are down, there is less water pollution, human rights indicators improve.

In passing McMahon comes up with a definition of *social rates of return* to education which is conceptually an advance on the traditional calculation of private rates of return minus public costs plus income taxes paid. McMahon's social rate of return is estimated from the total direct and indirect effects of education on *per capita* GNP compared with the percent of *per capita* GNP devoted to education (plus income foregone). In practice, of course, that is a rather blunt instrument useful only for long run growth models, and does not allow calculations of whether the social returns to one kind of education are greater or less than another type.

Education policy makers will undoubtedly quote McMahon's findings at second and third hand for many years to come. Therefore, much depends on the validity and robustness of the empirical data fed into it. There are five main reasons why caution is in order. One is that the author is at pains to point out on several occasions that this is a long term model and cannot take into account short term fluctuations like the Asian financial crisis of the mid-1990s. It is possible that to some extent endogenous growth is simply claiming long run exogenous change that has, on the whole and with many exceptions, raised the quality of human life in several ways simultaneously. In similar vein variables are introduced at various stages to take account of unique or one-off factors like being oil rich or a period of civil strife. Third, although the signs of most coefficients are in the desired direction the variances are considerable. The direction of predicted changes may be robust but the extent is much less so. Fourth liberal use is made of time lags. Finally there appears to be a certain amount of *ad hoccery* about measures of the efficiency of education.

That said this is an extremely ambitious project that very largely succeeds. It should be a staple of economists of education and economists of development for several years to come. It is to be hoped that it will be followed by much more rigorous economic analysis of the wider benefits of learning. The big message of the book is that knowledge based economic growth is the partner and not the competitor of broader social development.

GARETH WILLIAMS

Institute for Education

Monetary Transmission in Europe: The Role of Financial Markets and Credit. By KAKES (JAN). (Aldershot and Lyme, NH: Edward Elgar, 2000. Pp. ix+154. £45.00 hardback. ISBN 1 84064 241 6.)

Most expositions of the quantity theory of money presume that banks are simply distributors of money. They make no contribution to financial intermediation, which is costless, and their involvement in activities beyond their basic function endangers economic stabilty – hence the Chicago School proposals for strict control through 100% reserves.

Although the quantity theory has never been short of attention, its adherents have always been a minority. Most economists and all central bankers and financial practitioners have always subscribed to various forms of the *credit view*, in which banks make direct contributions to economic activity by searching for, receiving, and evaluating loan applications. Money was created in the process of financing economic growth. Bankers authorise people, in the name of society as it were, said Schumpeter, to form new combinations. He called them the *ephor* of capitalism.

The quantity theory has had the expositional advantage because its abstraction from transaction costs has allowed greater simplicity and rigour (if less realism, although that does not bother Professor Friedman), but the credit view has lately received more analytical attention along with the rising general interest in information problems. This development was reinforced by the US recession of 1990–91 that was accompanied (caused?) by the shift in bank portfolios from loans to Government securities forced by the adoption of risk-based capital requirements. There is still no formal model, however, which has led the author in his search for empirical distinctions between money and credit views to rely on VAR methods that do not require structure.

The objective of this monograph is to determine whether the effects of monetary policy in a small open economy (the Netherlands) are transmitted through money (paying attention only to the liability side of bank balance sheets) or through imperfectly substitutable bank assets. The author's line of arguments and his findings are carefully laid out, and are probably best summarised by following the order of the book. The informative chapters 2 and 3 would make useful supplements to a Money and Banking or Monetary Policy course. Chapter 2 summarises theoretical and empirical literature on the money and credit views of the transmission process, and the first part of chapter 3 is an excellent introduction to VAR methodology. The remainder of the chapter introduces us to the Dutch economy through a VAR analysis of variables suggested by an IS-LM model of a small open economy with a fixed exchange rate *vis-à-vis* an anchor country (Germany) during 1983–96. Dutch interest rates are governed by German rates, and Dutch monetary policy consists of actions on the small differential between the German and Dutch short-term rates. No significant effects of Dutch monetary policy on production or inflation are found. This is to be expected in a small open economy with a fixed exchange rate subject to external shocks. More surprising, perhaps, is the absence of any effect of German monetary policy measured by shocks from the German long-term interest rate.

IS-LM may be a useful beginning, but as the author points out, it cannot capture the distributional effects of bank lending or, therefore, distinguish between money and credit transmissions. In preparation for the key empirical chapters (6 and 7), the characteristics of bank lending and its interactions with the credit markets are examined in chapters 4 and 5. The principal finding is that bank investments in open-market securities are used as a buffer to bank loans. Presumably because of the bank comparative advantage in loans and the associated customer relationships, a tight monetary policy, for example, exerts no significant effects on bank loans or, therefore, in an open economy, on production or inflation. The US credit crunch of 1966 was a result of bank sales of municipal securities in order to meet customer loan demands in an inflationary, booming economy. In the Netherlands, apparently, the securities markets are stabilised by German and other foreign investors.

The imperfect substitutability of credit instruments asserted by the credit view is said to imply greater effects of tightening than of easing monetary policy. This is checked in chapters 6 and 7 for several countries, and consistent with earlier studies, asymmetries are found for larger countries – but not for the Netherlands. The author concludes that Dutch banks insulate their loans from monetary shocks throughout the cycle.

A more extensive study might yet turn up significant effects of Dutch monetary policy, but until then we recommend the conclusions of the careful, clear, and fair approach presented in this book.

JOHN WOOD

Wake Forest University

Models as Mediators: Perspectives on Natural and Social Science. Edited by MORGAN (MARY S.) and MORRISON (MARGARET). (Cambridge and New York: Cambridge University Press, 1999. Pp. xi+401. £42.50 hardback, US $64.95 hardback, £15.95 paperback, US $24.95 paperback. ISBN 0 521 65097 6, 0 521 65571 4.)

Recent years have seen a growing recognition of the importance of models in the natural sciences and economics, the one social science in which models are truly central. This book reflects this trend by exploring the idea that models are *mediators* between theories and the world. The two editors provide an introductory chapter in which they describe several different philosophical views about theories and models, but without definitely adopting any one of them. They follow this with a chapter presenting in some detail views of their own about models, as well as connecting these views with the subsequent ten essays in the book. There is a wealth of material in these ten essays by different contributors. Four of them treat models in economics, while the other six focus on models in the physical sciences. The models discussed are typically presented in considerable detail. Anyone interested in the role of models in the sciences will find it worthwhile to read this book.

The editors (page 24) believe that there are various ways in which models mediate between theories and the world. The different forms of mediation are reflected in the diverse ways in which models function as *instruments*. The editors (pages 18–25) distinguish three broad kinds of uses to which models are put in their capacity as instruments: (a) the construction and exploration of theories; (b) displaying measurements, structuring measurements, or actually making measurements; and (c) designing and producing technologies. The editors (page 18) say that it is the *autonomy* of models that enables them to be instruments; the autonomy of models is a necessary condition for fulfilling the functions that constitute their being mediators between theories and the world. And the autonomy of models consists in their partial independence from either theories or the world. The editors (page 11) think that typically models include elements drawn from theories *and* bits of observational data (the world). Thus models are neither just theory nor the world but something in between the two.

The editors' emphasis on models as instruments may suggest that they are instrumentalists about models (take models to be just tools that should not be interpreted as conveying truths or information about anything). But they (pages 24–5) reject instrumentalism about models. They claim that models can *represent* theories or the world or both, thereby providing information about them. The editors (pages 25–30) discuss several kinds of representation of which they take models to be capable. And the representational aspect of models enables us to learn from them about both theories and the world.

The editors (page 12) explicitly eschew any effort to provide a general theory about the nature of models and the model/theory distinction, although they do say a little about this distinction as they believe it to hold in some cases. They (page 32) allow models to be physical objects, computer programs, diagrams, mathematical structures, and perhaps many other things as well. But the amorphous condition in which the book leaves both the general concept of a model and the model/theory distinction creates a difficulty for the editors' claim (the mediation claim) that models are mediators between theories and the real world. Presumably, for the mediation claim to be true, models must be *distinct from both* theories and the world. The editors themselves seem to acknowledge this with their claim that models are autonomous. Yet, the amorphous, under-analysed notion of models employed by the editors (and some of the other contributors to the book) makes it difficult to preserve the model/theory and model/world distinctions. (Mauricio Suárez is the only contributor to explicitly deal with this difficulty in chapter 7).

Space limitations restrict me to citing only one case to illustrate the problem. In chapter 3, Morrison treats what she regards as a model constructed in 1904 by Ludwig Prandtl to study properties of fluids. Prandtl's model was a physical object, specifically, a machine consisting of a hand-operated water tank in which a paddle wheel set water in motion. But this model, being a concrete physical object, seems to be part of the real world itself. So, it is not distinct from the world in the way apparently required for it to mediate between theory and the world. To be sure, Morrison (pages 26–7) says that Prandtl's model was used to construct a mathematical model of fluid flows – see use (a) of models above. This may be, but Prandtl's model having this use is entirely consistent with the model being identical with a certain bit of the world.

Steven Rappaport

De Anza College

An Entrepreneurial Theory of the Firm: Foreword by Israel M. Kirzner. By Sautet (Frédéric E.). (London and New York: Routledge, 2000. Pp. xviii+181. £50.00 hardback. ISBN 0 415 22977 4.)

Austrian economics lacks a theory of the firm. Frédéric Sautet was alert to an entrepreneurial opportunity for arbitrage between Austrian theories of the market process as a response to the Hayekian problem of incomplete and dispersed knowledge and the conception of the firm as a pool of resources, and presents this book as an extension of both Austrian and resource-based theory. He begins with a helpful distinction between the uses of equilibrium as a foil to theories of non-equilibrium behaviour, as a depiction of reality, and as a benchmark for policy, and demonstrates that even distinguished economists can slip between them – and be misinterpreted by other economists. Both Coase and Williamson, failing to appreciate the force of Knight's argument that neither entrepreneurship nor the firm have any substantive role in a rational choice equilibrium, developed analyses of the firm as an organisation which, although presented as contrasts to market theory and encompassing important elements of a theory of process, still encouraged assimilation to theories of efficient allocation in a closed model. The 'absence of surprise' in well-designed Williamsonian hierarchies is a clear signal of what is still missing. This section defines and illustrates a problem of wide application, and may inspire others to make similar analyses of economists' practice in other fields.

In the second part of his argument, Sautet argues that even if transaction costs are zero, a world of uncertainty, in which possibilities unthought of may invalidate present knowledge, and there are no reliable premises for rational choice, provides a role not only for the entrepreneur but also for a firm as a means of co-ordinating the development of new systems of production in order to realise a specific entrepreneurial vision. (But if transaction costs are knowledge costs, how can they be zero if knowledge is uncompletable?) If productive knowledge is problematic, firms have a distinctive role as organic organisations, generating and selecting context-specific routines. This argument would be strengthened by elaborating his treatment of entrepreneurial visions as fallible conjectures rather than perceived truths, for the testing and revision of conjectures requires a set of maintained hypotheses which may conveniently be provided by the particular institutions of the entrepreneur's firm. His suggestions that we should think of the firm not as a substitute for markets but as a maker of markets also invite further development.

The switch from a contract-based explanation of the firm to an emphasis on devising and managing productive operations leads naturally to an exploration of the relationships between capabilities and entrepreneurship in the final section. Resource-based theories appear to link the firm to differentiated knowledge, but the emphasis, particularly among writers on strategy, on the search for rents diverts attention from the importance of discovery – and the threats generated by different discoveries in different firms. Rents of ability are no doubt more comfortable to defend than rents which depend on blockading entry; but in this version of resource-based (or even knowledge-based) theories we are still contemplating equilibria which rest on factors that are determined outside the model. Sautet perceives that Penrose's theory of the firm has much more to offer, and is indeed the outstanding candidate for his arbitrage proposal; it was clearly differentiated by its author from equilibrium theory and relies explicitly on entrepreneurial 'images' to connect evolving capabilities with new uses. Nevertheless Sautet finds it in need of a specifically Austrian improvement by insisting on the costless perception of opportunities; 'alertness' has no relation to 'search'. Yet it is a necessary part of the Austrian story that alertness is as differentiated as knowledge, and it is a particular merit of Penrose's exposition that the distinctiveness of each firm shapes both its development of capabilities – new productive knowledge which is created or perceived, not deduced – and the interpretative framework which is a necessary element in alertness. There may be no cost at the point of discovery, but if, as Sautet believes (pages 106–7), alertness depends on knowledge and capabilities which are path dependent, then the choice of path necessarily has an opportunity cost, even though that cost cannot be known until too late. Sautet's own valuable discussion and illustrations of the effects of organisational form on the possibilities of discovery rests on such costs.

Sautet's contribution is not quite as novel or as complete as he claims; but that there is significant added value in approaching the firm from an Austrian perspective need not be doubted. Moreover, just as Sautet rightly perceives the firm as an institution which shapes structures of complementary knowledge and provides distinctive contexts for discovery, so we might expect that there is unrealised potential for academic entrepreneurship in imagining new connections between Austrian and other perspectives on the working of economic systems.

Brian J. Loasby

University of Stirling

European Industrial Policy: The Twentieth-Century Experience. Edited by Foreman-Peck (James) and Giovanni (Frederico). (Oxford: Oxford University Press, 1999. Pp. xv+466. £60.00 hardback. ISBN 0 19 828998 7.)

This is an extremely interesting book, providing a broad historical account of industrial development in Western Europe and Russia during the 20th century and assessing the role played by industrial policy in that development. While it may be too general for the specialist, it is an invaluable source of information for those wishing to get at least some empirically based feel for the similarities and differences between countries that have characterised the European experience from the late 19th century to the present day. Industrial policy is defined as ‘every form of state intervention that affects industry as a distinct part of the economy’ (page 3). The core of the book consists of twelve case studies of individual countries. The editors contribute an introduction and conclusion and there is a chapter on a ‘cultural theory’ of industrial policy.

Running through the book there is an underlying tension between the neoclassical welfare economics concept of efficiency, which is explicit in the editor’s introductory theoretical discussion and implicit in most of the case studies, and the rich historical material that is surveyed. Since the actual industrial policies followed cannot for the most part be explained in terms of policies to deal with market failure, recourse is made to public choice theory and the power of vested interests as an alternative. This in principle enables policies to be judged in terms of their effectiveness in achieving their stated objectives. Nevertheless, the overwhelming impression of the theoretical discussion in the book is that industrial policy, particularly interventionist policy, has been a mistake. The editors quote Neven and Vickers (1992, p. 194) approvingly: ‘competitive market forces do an imperfect job but one that the instruments of industrial policy are unlikely to improve on’ (page 10).

The case studies remind one in detail of how central the role of the state has been and continues to be. Some chapters are overly descriptive, documenting what is broadly periodised as pre-1914 liberalism with an emphasis on infrastructure, wartime and inter-war interventionism, the apogee of industrial policy in the third quarter of the century (referred to by the editors as its ‘hubris’, page 437), and a return to liberalism from the 1980s onwards. Others attempt to provide an explanation for the changes in state policy in terms of the interplay between the international context, the level of development of the country, the internal class relationships, and the objectives of the ruling elites. The chapters on Sweden, the Netherlands, and Ireland are particularly interesting in this respect. The chapter on Russia warrants special mention. It is exceptionally well written and documented, with a clear structure and a more systematic presentation of data than elsewhere. However, one wonders whether it really fits with the rest of the book. Its inclusion is justified by the argument that the Soviet command model constituted the most extreme form of state intervention, yet the Soviet experience throws little, if any, light on the role of the state in developed capitalist economies, the principal focus of the book.

There is space for discussion of one point of substance. The main reasons (not mutually exclusive) why industrial policies have been adopted may be summarised as infrastructure development, military capacity, industrialisation, international competitiveness, and the problems of declining industries and regions. In the 20th century ‘reactive’ policies associated with declining industries have absorbed the largest share of resources devoted to industrial policy. They have virtually never been successful in halting or reversing decline and have generally been regarded by mainstream economists as irrational capitulations to vested interests. However, the editors note that such policies can be justified 'by public opinion ... only being willing to tolerate so much pain in the name of market freedom. There will always be a point beyond which the willingness to accept the laws, customs, and conventions on which markets depend, breaks down. ... In short, reactive policies might sometimes be warranted to maintain public support for a market economy' (page 444). This recognition that what people want may not coincide with the imperatives of competitive market forces introduces a new dimension to the discussion of the objectives of industrial policy, albeit with somewhat negative connotations. A more positive approach is provided by the insightful conclusion to the chapter on Portugal which raises the problem of 'trying to define concepts of public interest, for regions and communities' (page 292) in the wider context of national and European Union policy making.

Finally, there are a few typos, the most creative of which is in the chapter on a cultural theory of industrial policy. The author argues that trust is a crucial component of successful industrial policy intervention. The role of elites is emphasised and attention is drawn to the paradox that the impersonal institutions and procedures informing modern attitudes to intervention ignore the impersonal interactions that are necessary for the cohesion and efficiency of elites, even of a 'modem [sic] elite' (page 413).

PAT DEVINE

University of Manchester

Reference

Neven, D. T. and Vickers, J. (1992). 'Public policy towards industrial restructuring: some issues raised by the Internal Market program'. In (K. Cool and J. Walter, eds.) *European Industrial Restructuring in the 1990s*, London and Basingstoke: Macmillan.

Rational Choice. By ALLINGHAM (MICHAEL). (London and Basingstoke: Macmillan, 1999. Pp. xi+143. £45.00 hardback. ISBN 0 333 74512 4.)

There are now so many key assumptions of economic theory built on the logic of choice that there is no excuse for ignoring the subject matter of this book. Michael Allingham has condensed the theory of choice into six short and challenging chapters. His treatment moves from choice by the individual under conditions of certainty, uncertainty, and risk, to strategy, knowledge, and, finally, social choice. Its treatment is abstract and axiomatic. Hilbert and Debreu are quoted with approval, for their method as much as for their ideas. None of it is easy reading. A quick glance through the pages of any chapter will confirm that few concessions are being made to the uninitiated. Propositions, definitions, and proofs are all expressed in symbols. Nor is it surprising to find yet another appendix on Sets and Numbers. One obvious comparision is with *Notes on the Theory of Choice* by David Kreps. His book is longer (although it does not cover social choice) and equally difficult. It does, however, offer some discussion of the limits to the theory of choice and of its wider consequences. Kreps gives us Trade-Off Talking Rational Economic Person, both to admire and worry about. He admits that there are deep philosophical issues involved in the way in which choice is treated, and even refers to some empirical evidence. Allingham confines himself to the pure theory. His text has fewer diversions and elaborations. It keeps to the straight and narrow path of mathematical logic. Such brevity can be frustrating. What is the reader to make, for example, of something called Nature, whose choices maximise 'her expected utililty'? Allingham grants that the study of logic is worthwhile in itself but insists it is also the central component of 'eudaimonia'. Although Benthamite sentiments are relevant, we are still left in the dark about the merits of prescriptive and descriptive theories of choice.

Potential readers should not be put off by these reservations. As this reviewer has discovered, by tackling the chapter on social choice, there is much to be gained from sticking it out. Allingham starts by dividing the requirements he wants to impose on the aggregation of individual orderings into two. First are those relating to the information included in the operation. Second are those relating to how social choice responds to this information. He then distinguishes between weak and strong versions of both requirements. Thus the informational requirement is met by the independence of irrelevant alternative axiom as the weak version, and by the neutrality axiom as the strong version. The responsiveness requirement is met by the unanimity or Pareto axiom as the weak version, and by the monotonicity axiom as the strong version. Being strong means that neutrality implies the independence axiom, and montonicity implies the unanimity axion. Weak versions, by contrast do not imply the strong. Allingham calls social choice rules that pass the weak independence and unanimity requirements 'admissible'. Those that pass the strong neutrality and monotonicity requirements are called 'acceptable'. This is only part of the story. In addition to the aggregation requirements, social choice should be consistent. It should at least be 'reasonable', and if possible be 'rational'. In keeping with his earlier examination of individual choice, he defines reasonableness to mean that social choice satisfies some reasonable ordering relation, (e.g. better than or indifferent to), and rational to mean that the revealed preference axiom is satisfied. Rational, therefore, is stronger, or stricter than reasonable. There are now four kinds of test for social choice rules: the admissibility or acceptability options in the aggregation spectrum; and the reasonable or rational options in the consistency spectrum. He claims that those rules which satisfy all the weak tests are too numerous to be of interest. At the other extreme, there are no rules which pass all the strict tests. This last statement is proved by examining those rules which might meet a mixture of weak and strong tests. In other words the acceptable (strong) and reasonable (weak) rules, and the admissible (weak) and rational (strict) rules. Allingham analyses each in turn, with the aid of definitions of veto, quasi-veto, quasi-dictatorship, decisiveness, and dictatorship. He shows that a rational social choice rule is admissible if and only if it is dictatorial, and because dictatorial social choice rules do not satisfy the monotonicity requirement, then there is no acceptable social rule that is rational. Those acquainted with the literature on social choice, and who have read through the details of at least one impossiblity proof, will find the chapter clever and stimulating.

JOHN BONNER

Leicester

Reference

Kreps, David M. (1988). *Notes on the Theory of Choice.* Boulder, Colorado: Westview Press.

Development Theory and the Economics of Growth. By ROS (JAIME). (Ann Arbor: University of Michigan Press, 2000. Pp. xvi+429. £40.00 hardback, US $65.00 hardback. ISBN 0 472 11141 8.)

The book attempts to blend the recent developments in growth theory, e.g., endogenous growth with its emphasis on dearth of physical and human capital and technological backwardness as impediments to growth, with the earlier contributions to development theory with its emphasis on the paradigms of increasing returns to scale, imperfect competition, and an elastic labour supply that may generate development traps in the form of low level equilibrium output and income. The book argues that a moderate dose of increasing returns to scale combined with the presence of labour surplus can provide substantial improvements over both the Solow-type and endogenous growth models of today in their explanation of cross-country developments. The author has made a bold attempt to justify this argument both theoretically and empirically, though in the empirical front he is hindered by the usual lack of reliable and homogeneous data.

The book comprises four parts. The first part comprising chapters 2 to 6 explores in some detail the relations between modern growth theory and the classical development economics. In the latter area the contributions of Lewis, Rosenstein-Rodan, and Nurkse are discussed in relation to the neoclassical growth models developed by Solow and also the endogenous models due to Romer, Lucas, and others. While Lewis assumes a constant returns to scale technology and perfect competition in the capitalist sector with exogenous technical progress, Rosenstein-Rodan and Nurkse took the more radical step of analysing the dynamic implications of increasing returns to scale in the technology of the capitalist sector. This latter step brings it closer to the modern endogenous growth theory, which emphasised learning by doing, knowledge spillover and innovation under rivalrous competition as sources of rapid growth. By using an exogenous increasing returns technology the author presents an extended version of the Rosenstein-Rodan model, which in the framework of an elastic labour supply generates multiple equilibria, so that depending on initial conditions, the economy can be stuck in a development trap. He shows analytically that these circumstances are not confined to low levels of economic development alone but can be usefully applied to more general economic situations of developed economics, where demand and factor supply elasticities interact with a dose of increasing returns in new industries so as to hold back the incentives to invest.

The second part comprising chapters 7 to 10 develops the open economy links between resource reallocation due to openness in trade, factor accumulation, and technological change. Here the author explores the formulation of a model with multiple short-run equilibria in which the pattern of trade specialisation affects both the steady state level of income and the rate of convergence to the steady state. The third part comprising chapters 11 and 12 discusses the problems of effective demand facing a developing economy. These problems are very different from the typical Keynesian situations. Here the author considers situations where foreign exchange constraints operate directly on the domestic supply of goods through the rationing of imports so as to generate the vicious circles of economic decline as in the cases of stagnation of a number of sub-Saharan countries in recent decades.

The fourth part comprising chapter 13 provides a broad framework of both empirical and analytical defence of classical development economics and concludes by arguing the case for an integration and synthesis of early development theory in its generalised framework with those of the contemporary economics of growth.

The book makes a significant contribution to our understanding of the classical development economics in the light of the recent models of endogenous growth. The author has achieved great success in generalising the framework developed by Lewis, Rosenstein-Rodan, and Nurkse so that it is more directly comparable to the major premises of modern growth theory, e.g., increasing returns and openness of trade, imperfect competition and multiple equilibria with development, and foreign debt traps.

To this reviewer the book has one missing link in that it ignores the contributions of Schumpeterian innovation by saying that this is not relevant to the underdeveloped economy. Since Schumpeter's concept of innovation is very broad and inclusive of new knowledge, new markets, and new organisations, its dynamic role cannot be ignored. The rapid growth episodes of the NICs in South East Asia bear eloquent testimony to the innovative growth of the modern sector based on software and other modern technology. A number of researchers have recently generalised Schumpeter's contributions by developing nonlinear growth processes, which exhibit multiple equilibria, positive, and negative feedback systems and knowledge diffusion through creative destruction and new innovation in a framework of rivalrous competition. The rapid growth episodes of NICs and the development traps are easily explained in this framework. One may note that Schumpeter's innovation is profit driven and it provides the key to dynamics of growth and this is comparable to the central theme of development in Lewis's view of an increasing profit share during the labour surplus phase. For Schumpeter however, innovation in different forms is a disequilibrating force, which when combined with increasing returns technology and rivalrous competition may generate various types of multiple growth patterns.

JATI K. SENGUPTA

University of California at Santa Barbara

The World Bank: Structure and Policies. Edited by GILBERT (CHRISTOPHER L.) and VINES (DAVID). (Cambridge and New York: Cambridge University Press, 2000. Pp. xxvi+335. £45.00 hardback, US $74.95 hardback. ISBN 0 521 79095 6.)

This volume, the third in a series arising from the ESRC's Global Economic Institutions research programme, presents twelve papers (ten specially commissioned) on the theme of the future of the World Bank. Christopher Gilbert and David Vines, the editors, describe it as 'an ''insider's'' book'. So it is, in more sense than one. It adopts a deliberately sympathetic view of the Bank's purposes and activities. In addition, seven of the twelve papers, plus the Introduction, were authored or co-authored by current or recently departed Bank staff members. Much can be learned from both the insiders' and the outsiders' chapters about the Bank's operations, and aid policy generally.

The reader might do best to begin with Part Two, on the effectiveness of (previous) World Bank assistance. On project lending, Devarajan and Swaroop point out the considerable evidence for the fungibility of finance. This implies that 'standard project appraisal techniques ... tell us little about the development impact of that assistance' (page 207). Projects with high rates of return are important, but they are important primarily for the Bank, to ensure loan repayment, to maintain its triple A credit status, to attract future borrowers and thus to secure its income and its reserves. Projects can be improved by good economic and sector work, and this is cost-effective (Squire, page 127). Isham and Kaufman report that a good policy and institutional setting can add between 6 and 10 percentage points to the economic rate of return on Bank projects (page 256).

However, the positive developmental impact of Bank finance depends not on what is happening to its projects, but whether the entire economy is performing well (which in turn improves returns on its projects).

The Bank's response to this problem was the policy-based lending of the 1980s, reviewed here by Ferreira and Keely, which tried to improve performance by changing economic policies. They conclude that, while individual policy instruments were well chosen, there was no significant relationship between improvements in medium term economic performance and the successful implementation of adjustment programmes designed and financed by the Bank. This outcome is attributed firstly to 'complex issues of timing and sequencing, to which adequate attention was seldom paid' and secondly to 'the nature of a country's political economy (which) needs to inform the optimal feasible design of the timing and sequencing of reforms' (page 189).

It is good to have these theses of *Aid and Power*, which seemed quite bold when first published in 1991, confirmed.

Recent econometric analysis of the relationship between aid and growth has re-emphasised the importance of the policy environment in fructifying aid. Burnside and Dollar reprise this analysis in their paper, and extend the result from growth to the reduction of infant mortality, as a proxy for poverty reduction. Hansen and Tarp have questioned the robustness of the Burnside–Dollar result on aid and growth, but their challenge is not reflected in this book. That aside, and given the Bank's over-riding commitment to reducing poverty, one might think that it should now refurbish the loan conditionality of policy change in the present more favourable circumstances. However, no inside contributor favours this. Collier (pages 299–305) argues that the Bank lacks the political clout to do so, so that any promises in that sense would not be credible.

If this is right, the Bank has little choice but to retreat to its pre-1980 position of relying on policy dialogue when it does not approve of country policies. In a nutshell, all borrowing countries will be the Bank's 'partners', but partners whose policies are approved will get little dialogue but much money, while partners whose policies are disapproved will get the opposite. This proposed strategy of 'selectivity' is rationalised as the result of the ineffectiveness of conditionality by the Gilbert, Powell, and Vines paper (but, confusingly, not by the Hopkins, Powell, Roy, and Gilbert paper, which argues in a contrary sense that loan conditionality is simply unavoidable). The difference in Bank lending policy is best expressed as shifting from making *policy change* conditions to making *policy threshold* conditions. None of the papers discusses the practicality of the latter, surely an urgent task.

The catch phrase for the new approach is 'the Knowledge Bank'. Squire argues ingeniously for the Bank's retention of its in-house research department, despite the 'good governance' rule of purchaser-provider separation in public institutions. However, the political legitimating function of 'knowledge' for the Bank is well explained by Ngaire Woods (pages 139–144) and a perceptive contribution by Ravi Kanbur – now ex-Bank after an editorial difference of opinion – and David Vines shows just how variable the Bank's 'knowledge' about poverty reduction has been over the years.

In sum, these chapters deserve to be widely read by Bank watchers, and carefully digested.

JOHN TOYE

Centre for the Study of African Economics, University of Oxford

Regional Integration and Trade Liberalization in SubSaharan Africa: Volume 2: Country Case-Studies. Edited by Oyejide (Ademola), Ndulu (Benno) and Gunning (Jan Willem). (London and Basingstoke: Macmillan, 1999. Pp. xv+663. £80.00 hardback. ISBN 0 333 66105 2.)

Regional Integration and Trade Liberalization in SubSaharan Africa: Volume 3: Regional Case-Studies. Edited by Oyejide (Ademola), Elbadawi (Ibrahim) and Yeo (Stephen). (London and Basingstoke: Macmillan, 1999. Pp. xiv+323 £55.00 hardback. ISBN 0 333 66106 0.)

Regional Integration and Trade Liberalization in SubSaharan Africa: Volume 4: Synthesis and Review. Edited by Oyejide (Ademola), Ndulu (Benno) and Greenaway (David). (London and Basingstoke: Macmillan, 1999. Pp. ix+167 £45.00 hardback. ISBN 0 333 66107 9.)

Despite recent reductions of trade barriers and a long history of regional integration initiatives, Sub-Saharan Africa's shares of international trade and investment flows remain low. These three volumes (along with the first volume of the set, published in 1997) analyse causes of Africa's marginalisation, and they explore how countries in the region might better exploit opportunities offered the world economy. Sub-Saharan Africa has been under-represented in most comparative studies of trade liberalisation and regional integration. These volumes – products of a major collaborative project sponsored by the African Economic Research Consortium (AERC) – help to fill the gap. They include country and regional case studies, along with a set of essays that synthesise and review the project's main findings.

The contributors focus on how African countries can benefit from closer integration into the world economy, but they go well beyond conventional discussions of the economic costs of protectionism. A recurring theme is the tendency for macroeconomic considerations to drive trade policy in Sub-Saharan Africa – with governments tightening trade restrictions in response to balance-of-payments pressures and relaxing them when payments pressures ease. This tendency helps explain rising barriers amid the macroeconomic imbalances of the late 1970s and early 1980s, along with subsequent liberalisation in countries where structural adjustment has brought greater macroeconomic stability. An important challenge for African governments (explored in detail by Collier and Gunning's in volume 4) is to achieve credible policy reform despite their heavy exposure to external shocks and associated temptations for opportunistic policy reversals.

Volume 2 contains ten country studies of trade liberalisation. The case studies apply a sophisticated analytical framework sensitive to interactions between trade policy and the macroeconomic environment. The authors focus on trade liberalisation 'episodes', identified jointly by explicit policy changes and changes in intersectoral prices and quantities. They also assess the credibility of liberalisation attempts using indicators such as balance-of-payments, fiscal, and current account deficits. The case studies capture the experiences of a range of African countries – including Côte d'Ivoire, Ghana, Nigeria, Kenya, Mauritius, Tanzania, Uganda, South Africa, Zambia, and

Zimbabwe. They combine narratives of trade policy changes with a wealth of descriptive data, averaging roughly twenty tables per chapter. The data help in identifying trade liberalisation episodes and assessing their consequences. The case studies are generally well executed, although the breadth of the data occasionally outstrips the depth of interpretation. For example, the authors use official figures to measure trade flows, without adequately considering Azam's observations (in volume 3) about the prevalence of smuggling in Sub-Saharan Africa and its sensitivity to policy-related incentives. Changes in official trade levels during liberalisation episodes may reflect substantial 'trade diversion' from illegal to official channels, not simply the 'trade creation' effects usually assumed in the case studies. Illegal transactions are difficult to measure, but Azam shows that they cannot be ignored when assessing trade liberalisation – especially its impact on trade traversing Africa's administratively porous national borders.

Volume 3 contains six regional case studies, along with Azam's chapter on smuggling. The authors address a common set of questions about the context and consequences of regional initiatives, within a looser framework than the country studies. Two chapters focus on efforts to develop (sub-) regional common markets (ECOWAS in West Africa and PTA-COMESA in Eastern and Southern Africa), two on integration efforts amond Francophone countries (UEMOA in West Africa and UDEAC in Central Africa), and two on regional groupings in Southern Africa (SADC and SACU). The case studies provide informative accounts of diverse initiatives. They yield some insight into African policymakers' continued embrace of regional integration despite a history of disappointing results.

Volume 4 is a synthesis and review of the collaborative project. Early chapters by McCarthy and O'Cléireacáin summarise major findings of the volumes on regional integration and trade liberalisation, respectively. They conclude that attempts at regional integration have generally been ambitious but unsuccessful while, conversely, trade liberalisation efforts have been modest but beneficial. McCarthy argues that, to avoid repeating past failures of 'grandiose' regional schemes, integration should be pursued incrementally and always be grounded in sound policies at the national level. O'Cléireacáin sees steady but 'faltering' progress toward trade liberalisation in Sub-Saharan Africa. Yet he laments the institutional weakness of African states, which he sees as a major obstacle to the sustained pursuit of outward-looking development strategies.

The final three chapters of volume 4 look to the future. Collier and Gunning focus on credibility problems that weaken private investment responses to policy reform in Africa. They argue that African governments cannot create viable domestic institutional restraints on their own policymaking discretion, and that donor conditionality has failed as an external substitute. They advocate mechanisms based on 'intergovernmental reciprocal threats', in which African regional groupings link themselves to Europe through free-trade agreements that credibly penalise policy reversals. (Such an approach is controversial, and McCarthy compares it in his chapter to 'politically indigestible' proposals for Africa's recolonisation by Europe). While Collier and

Gunning's essay can be read as a 'how to' guide for credible policy reform in Africa, Helleniner's asks 'how far' trade liberalisation should go. Helleiner shares Collier and Gunning's concern about maintaining macroeconomic stability, but he warns against a 'mindless globalisation' in which governments fail to use state power to mediate links with the world economy. Oyejide adopts a similar perspective in his concluding chapter. Helleiner and Oyejide do not recommend a 'mindless' return to the inward-looking development strategies of the 1960s and 1970s. Yet their essays show that a restoration of macroeconomic stability is more likely to reignite debates about appropriate trade (and industrial) policies for Africa than to extinguish them.

This collection makes timely contributions to the economic literature on trade policy in Sub-Saharan Africa. The AERC brought together researchers based throughout Africa and elsewhere to explore crucial questions about the past, present, and future of the region's involvement in the world economy. The country and regional studies provide empirical context, and the concluding volume captures current economic debates about African trade policy. Importantly, the collection also begins to redress the neglect of Sub-Saharan Africa in broader comparative studies. Some themes – such as the domestic political dimension of the current round of African trade liberalisation – await more systematic research. Still, this collection should be extremely valuable not only to specialists on Africa's economies, but also to those seeking a comprehensive overview of regional integration and trade liberalisation in Sub-Saharan Africa.

ROD ALENCE

Stanford University

Integration and the Regions of Europe: How the Right Policies can Prevent Polarization. By BRAUNERHJELM (PONTUS), FAINI (RICCARDO), NORMAN (VICTOR) et al. (London: Centre for Economic Policy Research, 2000. Pp. xv+115. £25.00 paperback, US $37.50 paperback. ISBN 1 898128 46 4.)

This volume is the latest in the authoritative series, published annually by the CEPR, which looks into different facets of European integration. The topic this year is the regional impact of integration and the question that runs through the volume is whether polarisation is inevitable, or can be either avoided or attenuated by appropriate policy responses. The answer given is, not surprisingly, that there is no such inevitability and that European integration will only lead to unbalanced regional development if 'misguided policies' are adopted. Whether or not equity and efficiency are incompatible aims is one of the defining questions of economics as a discipline. In asserting that 'growth and cohesion are not enemies' the answer provided by this book is resoundingly that they are not.

The intellectual thrust of the book will be familiar to anyone who regularly browses the list of CEPR Discussion Papers. The Krugman and Venables 'new economic geography' model (NEG) is to the fore and the policy conclusions contain few surprises: governments are enjoined to promote competition in product and factor markets and to enhance infrastructure, education, and training. Comparisons are, largely, with the United States, but there is little mention of the burgeoning literature on innovation or institutional depth as factors in regional development. In these respects, the analysis presented very much follows what, to adapt a well-tried phrase, might be called the *CEPR consensus*. The book is, however, enriched by having obvious contributions from two specialists in the analysis of foreign direct investment (Pontus Braunerhjelm and Frances Ruane) and an expert on migration (Riccardo Faini).

In the first of the six chapters, the continued fragmentation of European markets is contrasted with the much greater integration of the United States. The inference to draw is positive: the EU has considerable scope for gains from greater specialisation and the promotion of clustering. The challenge for policy-makers, namely to overcome the obstacles that inhibit the realisation of this potential, is also clearly set out. Chapter 2 then presents an overview of relevant NEG theory which many lecturers in the economics of integration will find to be a useful crib, although one might have hoped to see at least some reference to the wider literature on the determinants of regional advantage and disadvantage.

Chapter 3 explores the role of foreign direct investment in shaping regional effects of European integration, and focuses on a case study of Swedish FDI, obviously drawing predominantly on the research of Braunerhjelm. Mobility of labour is discussed in chapter 4 in which the authors try, especially, to account for the decline in both cross-border and intra-country migration since the 1960s. Various explanations are canvassed, ranging from social and cultural barriers to a lack of incentives, but the authors plump for the high level of unemployment overall as the most persausive explanation. While this is eminently plausible, other explanations such as the mismatch between a supply of unskilled workers and a demand increasingly for new skills might have been worth exploring as well.

The two concluding chapters discuss policy issues and develop the authors' proposals for avoiding polarisation. Chapter 5 outlines the EU approach to regional policy and dwells on the use of state aids, before presenting case studies of regional policy in Italy and of Ireland's economic development success story. The presentation of EU policy, though broadly correct, is marred by being both out of date and inaccurate in describing the Structural Funds, the principal EU regional policy instrument, while the Cohesion Fund is not mentioned, in spite of being an important addition to the post-Maastricht policy armoury. A report published in February 2000 should, surely, take account of changes agreed in the summer of 1999 which not only recast the objectives of the Funds but also redrew the map of assisted areas. Reference is made to the transitional arrangements for Ireland, indicating that the authors are aware of the post-1999 arrangements, but is again incomplete.

The discussions of Italy and Ireland in chapter 5 provide overviews of the contrasting approaches to economic development policy in the two settings, but have obvious gaps: the effect of social partnership in Ireland, for example, is only hinted at. Moreover, the role and influence of EU policy is only obliquely mentioned in both cases, with the authors mentioning interventions by the Commission to challenge labour subsidies in the Mezzogiorno or low corporate taxes in Ireland, rather than examining the impact of EU regional spending. This is unfortunate, because a key policy question is whether the substantial resources that flow to the most intensively assisted EU regions represent money well spent.

The book concludes with a list of policy recommendations, described in the penultimate sentence as 'good economic management'. The trouble with these prescriptions is that they are rather too general and take too little account of contemporary thinking on regional policy. Thus, although the book provides a succinct, if incomplete, discussion of the policy issues surrounding European integration and regional specialisation, it is ultimately somewhat disappointing.

IAIN BEGG

South Bank University

Organisations with Incomplete Information: Essays in Economic Analysis: A Tribute to Roy Radner. By MAJUMDAR (MAKUL). (Cambridge and New York: Cambridge University Press, 1998. Pp. ix+346. £40.00 hardback, US $64.95 hardback. ISBN 0 521 55300 8.)

This collection of essays grew out of a conference at Cornell University in 1992 in honour of Roy Radner's 65th birthday. There is a very impressive list of contributors including Radner himself. The volume is not designed to be a *Festschrift*, but reflects three important themes of Radner's work, a general equilibrium of plans, prices and price expectations in sequence economies (the *Radner* equilibrium), dynamic and repeated games, and the design of organisations particularly when there are limits to information processing or bounded rationality. There is an introductory chapter by the editor plus eight chapters, three on general equilibrium, two on dynamic games, three on design problems. The chapters were anonymously refereed and there is a consistently high standard throughout.

The section on general equilibrium begins with a chapter by Wayne Shafer which presents an overview of the literature on plans, prices, and price expectations. It presents a canonical model and addresses issues of existence, local uniqueness, and efficiency. This is a very nice chapter with clear explanations of the key results, illustrated by simple examples. The key distinctions between nominal and real assets and complete and incomplete markets are clearly explained. A simple example with one asset and one state (complete markets) is presented to show the non-existence of a Radner equilibrium (and hence the difference between a Radner equilibrium and a competitive equilibrium). Genericity results in both the complete and incomplete markets cases are given. The indeterminacy of the incomplete markets case with nominal assets is clearly exposited explaining how the choice of numeraire can affect the equilibrium. Shafer also provides a general discussion of results. He explains the weakness of results couched in terms of numbers of assets and states when there is nothing in the model to determine the actual number of traded assets and when the precise notion of a state of nature or how they are enumerated is left unspecified. The next two chapters take up extensions to the Radner equilibrium. Beth Allen and James Jordan consider the differential information case where the revelation of information through prices can impact on trade itself (rational expectations equilibria). Lawrence Blume and David Easley address the stability issue of whether traders can learn, by appropriate Bayesian updating, to hold rational expectations. Answers here are generally negative. The key difficulty is that the dynamics of belief revision interact with the other intertemporal connections in extremely complicated ways.

The next two chapters consider dynamic games. Roy Radner himself considers a repeated partnership game and principal-agent games in both discrete and continuous time. The chapter by Prajit Dutta and Rangarajan Sundaram explains the importance of Markovian (or stochastic) games in economics but also the paucity of general results. Markovian games are repeated games where there is associated with each stage game a state which describes the action sets, payoffs of the players etc., and the state changes as a (stochastic) function of the action of the players. A typical example involves an intertemporal externality such as the joint exploitation of a common resource where the state variable is the level of the stock. There has been considerable success in analysing repeated games where the same stage game is simply repeated but there has been much more limited success in analysing Markovian games. Dutta and Sundaram summarise the state of the literature. A natural starting point is to examine Markovian strategies where the past history only influences strategies through the state variable. While it is possible to show that Markov-perfect equilibria (MPE) exist for finite games, in general it is not possible to prove the existence of a stationary MPE even when the underlying environment is stationary. Approximation results can be obtained for truncated games or by restricting best response requirements to nearly all states. An alternative approach is to use an iterative procedure on auxiliary stage games parameterised by the state whose limit is a sub-game perfect equilibrium. A general problem with all these approaches is that existence results apply only to stochastic and not deterministic dynamic games.

The last three chapters deal with design issues and bounded rationality. Andrew Schotter considers a problem of mechanism selection with special attention paid to mechanism for the allocation of baseball players to teams. Kenneth Mount and Stanley Reiter consider a modular network model of bounded rationality. Tim Van Zandt's chapter on organisational design considers the important issue of information processing within firms, that is to explain why a firm has many administrators to process information. It is obvious that there is the benefit of division of labour if the information processing capacity of a single agent is limited (bounded rationality) and the reduction in delay enables decisions to be made with more up-to-date information. But there are costs too as information has to be communicated which may be costly and information may itself be used strategically by the administrators and create familiar incentive problems. Radner was one of the first to address these issues and show how hierarchies could emerge endogenously.

I have two minor criticisms of the collection. Firstly, most of the material can be found elsewhere, e.g. van Zandt has a similar broader but less detailed survey elsewhere, Schotter refers extensively to his paper with Nalbantian in the *Journal of Labour Economics*, 1995. Secondly, some of the chapters cannot be read independently but refer to other work for details, e.g. Mount and Reiter have an example of the complexity of computing the equilibrium price in an Edgeworth box economy, but refer the reader to their unpublished working paper from 1982 for a derivation. These minor criticisms aside, this is a very good collection with a consistently high standard.

TIM WORRALL

University of Keele