BOOK REVIEWS

Joost Jonker, *Merchants, Bankers, Middlemen. The Amsterdam Money Market during the First Half of the 19th Century*. Amsterdam, Nederlands Economisch-Historisch Archief, 1996. Pp. 384. Dfl. 59,90

During the 19th century, the Amsterdam economy changed radically following the collapse of the city as world trade entrepot and international financial centre setting in at the end of the 18th century. To picture this process solely in terms of continuous decline and stagnation is a mistake, Joost Jonker explains in his thesis defended at Utrecht University late last year. True, Jonker admits, Amsterdam never regained its leading position, but for a city of moderate size, possessing a large accumulation of capital but with otherwise no particular comparative advantages, situated with its back to the main trade routes, it did not do quite so bad.

To substantiate this view Jonker analyses the operation and development of the Amsterdam money market from 1814 to 1863, a market despite its sharp decline still predominant. The approximately fifty years under consideration are a logical demarcation, and certainly possess, as Jonker puts it, ‘a certain pertinent coherence as the formative period of the Dutch financial system’. The foundation of the Nederlandsche Bank, the Dutch central bank, in 1814 by King William I marks the beginning of the era and it ends with the appearance of the first joint stock commercial banks and the link up of Amsterdam with the rest of the country through the establishment of a branch network by the Nederlandsche Bank in 1863.

Jonker concentrates on the conditions for trade and industrial finance, as most historians (used to) consider the lack of it to be one of the main causes of Hollands tardy industrial development. More specifically, he examines the supply side of the Amsterdam money market, trying to assess whether or not this market provided sufficient scope for economic growth and the development of large industrial companies. In this effort he has been rather successful. In his tautly composed book Jonker gives a more or less complete round up of the existing financial services in the period under review. After a general economic outlook and an overview of structural features such as interest rates, credit facilities and conditions, and attitudes towards credit, Jonker comes about the main ingredient of his book, a detailed survey of the pillars of the market, the Stock Exchange, the Nederlandsche Bank, the ‘handelshuizen’ or merchant bankers, the ‘kassiers’ (cashiers) and the Jewish bankers. His extensive research of numerous private and corporate files provides us with a wealth of information about the day-to-day operations of the Amsterdam financial institutions.

After reading the consecutive chapters Amsterdam emerges as a brisk commercial town, be it on a very moderate scale compared to the golden 17th and 18th centuries. Due to all sorts of initiatives of the merchant bankers, trade began to recover from the 1830s onwards, and a new entrepot system built on colonial commodities came into existence, boosted by the foundation of the Nederlandsche Handel-Maatschappij (NHM) in 1824. New processing industries arose, others revived, all in all generating manufacturing, commercial and financial activities. In the 1850s a promising industrial sector appeared, of which businesses like sugar refining, mechanical engineering and shipping were in an advanced stage of mechanisation.

Jonker points out in some detail that the 19th century Amsterdam money market offered sophisticated, ample, and very flexible finance in a wide range of diversity stretching from lombards, bill discounting, trade credit arrangements and mortgages to direct equity investment in the course of which money and capital market linked up with each other perfectly. Merchants and manufacturers used trade credit to secure business, granting favourable terms and often facilitating the transformation of credit into capital, by regular rollovers of formal equity participations. A substantial part of advances and equity participations were, incidentally, transacted on the private market, and often remained obfuscated, simply because borrowing was not done.

Within months of Filarsky's conspicuous dissertation *Kanalen van de koning-koopman*, Jonker's survey demonstrates once more the vigour of the first half of the 19th century, which appears to be a fascinating period of Dutch economic history. It is now time to say goodbye once and for all to Jan Salie, the personification *par excellence* of this period.

Corry van Renselaar

P.A.G. van Bergeijk and R.C.G. Haffner, *Privatization, Deregulation and the Macroeconomy*, Edward Elgar, Cheltenham, 1996. Pp. 256. £ 45

This is a useful book. It provides a good combination of theory and empirical and policy analysis. The book targets a broad audience and is of interest to both academics and policy analysts. It consists of four parts. The first discusses structural change in OECD economies. The second looks at the theory of industrial organisation and discusses the structure-conduct-performance paradigm, while the third part deals with macroeconomics and the modelling of structural reform. The final part of the book discusses policy.

The theme of the book is the analysis of structural reform or, more precisely, privatisation and deregulation. Structural reform is driven by the need of industrialised economies to adjust to changing circumstances in the face of rapid technological change and globalisation. Inflexible labour, capital or product markets can slow down adjustment and reduce the growth potential of economies. Starting with the United States, and followed by the United Kingdom and New Zealand, several OECD economies have recently engaged in major efforts to reduce rigidities in their markets. The analysis of such reforms is complicated, as these reforms may change the way economies work, making the empirical analysis of the effects of reform quite difficult.

The second part of the book covers the theory of industrial organisation, and looks into the structure-conduct-performance paradigm. This paradigm is a useful tool to describe the market process. It asserts that performance in a particular market depends on the conduct of buyers and sellers, which in turn is dependent on the structure of the relevant market. The authors discuss a wide range of indicators for the three aspects of the market process, and provide a substantial amount of empirical information. These include indicators of the persistence of profits, mark-up ratios, indicators of the speed of price adjustment, and product market inertia coefficients. Unfortunately, and this is my main criticism of the book, these indicators mainly cover the manufacturing sector. Most of the work on industrial organisation has focused on manufacturing, but the main debate on deregulation and privatisation deals with the service sector. The structure-conduct-performance paradigm is more difficult to apply to this part of the economy, mainly due to data constraints. However, empirical analysis of service sector reform, for several countries, is emerging in the literature and some attention to this work would have been helpful.

The third part of the book covers the macroeconomics of structural reform and the available modelling tools. The perspective on macroeconomics depends on the way in which microeconomic rigidities influence macroeconomic performance. The authors suggest three criteria to distinguish the theoretical approaches, namely whether or not agents have market power, whether prices and wages adjust immediately to exogenous shocks, and whether investment has any effect on capacity. These criteria allow a classification of the available literature into eight different strands of analysis. An important element is the time horizon of the analysis. It is well known that the static efficiency gains of structural reform are quite small. However, the dynamic gains may be more substantial, but more difficult to quantify.

Next, the book discusses the modelling strategies that have been followed, based on available work at the European Commission, for inter alia Germany, Australia, and The Netherlands. Most studies are based on a detailed sectoral analysis, followed by an integration in a multi-sectoral general equilibrium model or a macroeconometric model. All studies suggest that the removal of microeconomic rigidities has substantial macroeconomic impacts in the long run. The methodologies followed are often driven by the tools and data available and are therefore subject to some constraints. In addition, these studies typically say little about the costs of transition, the dynamic benefits of reform or the effects on welfare and quality of life. A contentious issue is also whether reform of product markets has any positive effects on employment. To some extent, this may depend on the functioning of labour markets. A simultaneous or sequenced reform of labour and product markets may be needed to enhance employment in the long run.

The final part of the book talks about the art, as distinct from the science of structural reform, and discusses important themes such as the credibility of reform, the sequencing of different structural reforms, the speed of reform, and the political economy of reform. This is an area where the answers are not always very clear, and politics appear to matter more than the academic debate. The remarks by Sir Roger Douglas, the main advocate of the reforms in New Zealand, are interesting in this respect. On sequencing he finds the debate fundamentally irrelevant, as politicians will implement certain policies when they can, not when they might be best from a sequencing point of view. On many of these areas the debate has evolved a bit since the work by Van Bergeijk and Haffner, and some agreement on the principles of regulatory reform appears to be emerging among OECD governments (OECD (1997)). Most countries now realise that structural reform is necessary and often vital to their long-term survival. In addition, the experience of several 'reformist' countries, now also including The Netherlands, demonstrate the potential of welldesigned regulatory reform and privatisation programmes. From this policy perspective, but also from an academic perspective, the book by Van Bergeijk and Haffner is a useful addition to the literature.

Dirk Pilat

REFERENCE

OECD (1997), *The OECD Report on Regulatory Reform*, Paris.

Salvatore Zecchini (ed.), *Lessons from the Economic Transition; Central and Eastern Europe in the 1990s*, Kluwer Academic Publishers, Dordrecht, 1997. Pp. xxix+590. £ 92.50

Zecchini's *Lessons from the Economic Transition* is a collection of articles written by scientists and policymakers involved with the creation of a market economy in Central and Eastern Europe. The company of authors contributing to the book is impressive and includes Balcerowicz, Fisher, Kornai, Leamer, and Stern, to mention just a few of the names. The problem addressed is to what extent there are lessons to be drawn from the countries' reform experience in the first half of the 1990s in order to improve their economic transformation and performance in the second half of the decade. Consequently, there is a focus upon policy recommendations rather than upon theoretical underpinnings of transformation. This is due to the fact that Zecchini as Deputy Secretary-General of the OECD took the initiative to organize a conference with the support of the Centre for Cooperation with the Economies in Transition (CCET).

The book has four constituent parts. Part I gives a general indication of the main successes and failures in the transition strategy. It basically addresses conditions to overcome the transition crisis and to realize sustainable growth. With the notable exception of the contribution of Laski and Bhaduri, all authors underline the success of quick liberalization in trade, production, and prices. Most forthright are De Melo and Gelb, who construct indices of economic liberalization and correlate these with economic performance. Without denying the relevance of the chapters related to the importance of liberalization, Roland's contribution on the nature of political constraints as decisive factor for the path of transformation is most persuasive. This chapter stresses the interdependency of politics and economics. Whereas the other contributors in this part raise the question why a country is performing better or worse than its neighbour, Roland asks why countries take different roads to a market economy. In other words, not the relative performance of institutional settings, but the political constraints to change the setting is the line of approach. The constraints are dependent upon the legacy of the past.

Part II addresses developments in microeconomic restructuring. Whereas in the beginning of the process of transformation the issue was how to transfer ownership rights from the state to the public, the emphasis has shifted towards corporate governance. This shift seems due to the fact that in countries where privatization was fast, the restructuring did not necessarily follow at the same speed. In their analysis of corporate governance, Frydman and Rapaczynski expose the danger of postponement in restructuring in case the new shareholders are the banks which, under the former regime, supplied many credits to the enterprise involved. This is the main fear regarding the Czech privatization. In the Hungarian model of privatization, based upon selling of state property, foreign investments are important. Hunya addresses this issue and concludes that privatization through foreign investment is the best guarantee for restructuring. Here, the sequencing problem applies. Whereas in the Czech Republic, restructuring has to follow the transfer of ownership, in Hungary, the two more or less coincide.

The third part of the book is devoted to the social impact of transformation. It addresses the costs of transformation, and unemployment is perceived as a substantial part of that. Commander and Tolstopiatenko scrutinize unemployment and focus upon the difference in rates of unemployment between the Central and Eastern European countries and the former Soviet Union. Whereas in the former countries, unemployment levels increased to approximately 12 per cent of the labour force, the transformation in the latter did not coincide with massive unemployment. The difference is explained in terms of budget constraints. Workers in the former Soviet Union are implicitly subsidized. If the budget constraints were to be hardened, unemployment would rise to the levels prevailing in Central and Eastern Europe. Another category of transformation costs is presented by Ellman. He pictures amazing demographic trends in Central and Eastern Europe and in the European part of the former Soviet Republics. Since the start of the transformation, the rise in mortality is really astonishing. In Russia, the increase in death rates was most extreme. Life expectancy fell from 70 in 1989 to 64 in 1994. The fall was predominantly due to the decline in life expectancy of men, which, considering the same period of time, declined from 64 to 58. Of course, one is well aware of the fact that transformation has a definite impact upon the division of welfare, but these statistics are striking and point to the fact that it is extremely costly, especially when taking into account that the increase in death rates seems due to violence and substance abuse. Ellman's conclusion is more ambivalent. On the one hand, he ascribes observed demographic trends to the economic transformation, but, on the other hand, he argues that the trends can be seen as an extrapolation of developments in the past.

The last part of the book elaborates external economic relations. There are three chapters on trade flows and foreign investments, whereas two chapters focus upon monetary aspects. The latter are written by Cooper and Rosati. These are excellent chapters. Cooper compares the introduction of currency convertibility by the OECD countries in the 1950s with that of the Central and Eastern European countries in the first years of their economic transformation, whereas Rosati addresses the selection of an exchange rate regime. The authors perceive convertibility as an important step in the transformation rather than as a symptom of a minimum level of economic development which enables the countries to bear the costs of currency convertibility. So, whereas in the fifties, the OECD countries faced the problem of hard currency shortages, the Central and Eastern European countries used currency convertibility to introduce competition. Under the condition of the application of a well-functioning exchange rate regime, the negative consequences of currency convertibility have proven to be within acceptable margins.

Zecchini edited a magnificent book, which is to be highly recommended to all scientists and policymakers interested in the problems of formerly centrally planned economies, provided that they have basic skills of reading quantitative economic analyses. There is only one point of criticism and that relates to the lessons to be drawn. Zecchini divides world economic history into what happened before and after 1989. This magic year was the starting gun for the transformation from centrally planned economies to market economies in Central and Eastern Europe. According to Zecchini, the events following 1989 were so quick and encompassing that these allow him to speak of 'the' transformation rather than the transformation in Central and Eastern Europe. Here, Zecchini seems to suffer from research myopia. His book does not include the examples of China and Vietnam, which also experienced a deviating transformation. Moreover, several '*anni miserabiles*' followed 1989 as the '*annus mirabilis*'. The fact that the crisis was more severe and lasted longer than anticipated reveals that the changes in the transition economies are perhaps not as drastic and quick as the political events in 1989 made it look like. This seems to be an important lesson, but that conclusion has not been drawn.

Herman W. Hoen

Keith Cuthbertson, *Quantitative Financial Economics: Stocks, Bonds, and Foreign Exchange*, Wiley, Chichester 1996. Pp. 470. £ 24.95

In most areas of science, with the exception of mathematics, the predominant research output is empirical in nature rather than theoretical. Economics is slowly shrugging off the plague of being a study about imaginary constructs in the arts tradition that produces theoretical toy instruments which are abused in the hands of politicians, moralists, and economists. The lack of sufficient data may be the explanation and excuse for the many normative dead alleys that have been produced by economists in the past. Proper macroeconomic analysis, one might argue, requires a couple of hundred years of cross-country data before the main issues can be decisively sorted out. Until then, theory and belief will reign the macroplayground by discretion. Fortunately, in areas like finance empirical research is thriving thanks to the wealth of data that come from organized exchanges. These data, more than in any other field of economics, directly relate to the theoretical constructs, and vice versa. Finance may be the field which transforms economics research into a scientific enterprise like the natural sciences.

The past year produced two valuable textbooks on this interaction between theoretical and empirical work. The prime purpose of these texts is to describe how one goes about testing financial theories. The graduate book is the treatise-like text 'The econometrics of financial markets' written by Campbell, Lo, and McKinley. The complementary undergraduate textbook is the one being currently reviewed. Taken together, both books provide a well written sequence from the undergraduate to the graduate level. The book by Cuthbertson has the added value over the graduate text that it treats three classes of assets: Equity, fixed income, and foreign exchange; the last topic is not covered by the graduate text. The foreign exchange market, though, provides an important link between finance and macroeconomics. Macrovariables, for example, explain the asset price behavior of exchange rates. Hence, macrovariables are the factors in an APT view of the forex market.

Inclusion of this topic gives the book by Cuthbertson a distinct flavor, and makes the book also suitable for a course in open macro and monetary economics.

The first part of the book covers the basic concepts of finance: The CAPM and the rational valuation formula. Part three deals with fixed income particulars like the yield curve. The forex market is covered in part four. It covers finance-type issues like the forward market efficiency hypothesis, and it discusses macroexplanatory models for forex behavior such as the monetary model. The forward-looking solution to the exchange rate model bears a striking relation to the rational valuation formula for pricing equity.

Part two provides a first glance at the empirical work. It discusses tests for the martingale property and treats the work on asset bubbles and excess volatility. In part five the discussion of market efficiency is continued by using multivariate tests on cross-equation restrictions which follow from the assumption of rational expectations. Concepts like cointegration play an important role in this latter part of the book. The last part of the book, part six, is devoted to the topical issues of a variable discount factor and risk premium. The closing chapter treats a number of econometric issues including the GARCH model, non-stationarity, estimation of rational expectation models, and cointegration. The book covers a lot of ground and provides a much needed text for sound undergraduate training in applied economics and finance.

Although I have little to quibble about the book, there are some matters of taste. The book nicely covers the different areas of equity, bonds, and foreign exchange, but there is little integration between the three areas. This is somewhat disappointing because the forex market easily lends itself to an integrated approach. The home bias puzzle, for example, may be related to the dominance of foreign exchange rate risk over equity risk. And the bond market is used to hedge forex risk, so that bond markets are internationally linked. Thus a discussion of currency overlay issues might have tied the three areas together. The book also spends no time at all on treating nonlinear derivatives like options. Derivative pricing of course is one of the foremost important current issues. Nonlinear asset pricing has also spilled over to areas such as the foreign exchange market. The target zone model is essentially an exercise in the pricing of options. It may be a matter of taste, but instead of discussing such highly nonlinear models of chaos for which there is little evidence, my choice would have been to devote some attention towards the empirical work on the pricing of derivatives. Nevertheless, I am convinced that this book will be highly valued in the market place, and this is all what counts in positive economics.

Casper G. de Vries

Tobias F. Rötheli, *Monetary Regimes, Risk, and International Capital Accumulation*, Berner Beiträge zur Nationalökonomie, Verlag Paul Haupt, Bern, 1995. Pp. 105. Sfr. 34

One line of thought concerning monetary policy addresses questions of how to operate within a given regime, that is within a given set of rules and procedures. This includes questions regarding the quantification of these rules and the choice of actions necessary to achieve the announced targets. The choice of the monetary regime influences the variability of inflation and exchange rates which, in turn, affects capital accumulation. Rötheli has evaluated various monetary regimes with respect to the long-term effects on savings, capital accumulation as well as allocation, and welfare. The main theme of his research is the effect of uncertainty surrounding the inflation and exchange rate responses. Risk-averse agents react to this uncertainty by altering their savings and portfolio allocation decisions. The effects of monetary regimes are analysed in a number of models that take individual utility maximisation as point of departure.

In fact, the core of the study consists of two (complementary) approaches which concentrate on inflation and exchange rate risk. First, chapter 2 analyses the effect of the monetary regime choice on inflation risk, savings, capital accumulation, and welfare. The four regimes under investigation are monetary targeting, price level targeting, a one-sided currency peg, and a two-sided currency peg. In a model of two identical economies an ordering of these regimes with respect to their effect on investment and welfare is derived, where the main focus is on inflation risk. This analysis deliberately abstains from real exchange rate risk, by (first) assuming that purchasing power parity holds at all times. The various monetary regimes differ in their effects because the return on investment is assumed to be influenced by the inflation rate. It turns out that investment is highest under the regime of the one-sided exchange rate peg followed by the regime of money stock control, the two-sided peg, and the regime of price level control. The ordering with respect to welfare is exactly the reverse, as the increase in (precautionary) savings and investment merely reflects the increase in uncertainty.

Chapter 3 relaxes the PPP assumption by exploring the notion that a regime of floating exchange rates leads to larger fluctuations in real exchange rates than a regime of pegged currencies. The model builds on the idea that investors are interested in diversifying their savings internationally. However, variations in the real exchange rate add risk to the foreign investment. Given that the expected change in the real exchange rate is assumed to be zero, the interest rate differential corresponds to the foreign exchange risk premium, which also determines the cost of hedging activities. After the transition from fixed to floating currencies, equity investment in the low interest rate country becomes more attractive to all investors and the capital stock in such a country will rise. Which country ends up in this situation depends on its relative performance in the fields of physical investment opportunities, wealth levels, variance and covariance of capital returns, exchange rate variability, and the degree of risk aversion of investors.

Chapter 4 puts this model to an empirical test. It addresses the question of how the transition from fixed to flexible exchange rates in the early 1970s has affected capital accumulation in Switzerland. Consistent with the theoretical results of the previous chapter, it is found that Switzerland, which emerged with a relatively favourable foreign exchange risk premium after the transition to flexible exchange rates, indeed experienced an increase in its physical capital stock. Unfortunately, Rötheli does not comment on the question whether this result can be generalised to other countries that underwent a similar regime change. As is well known, Switzerland often constitutes a maverick in empirical financial research due to its specific financial structure that is influenced by its famous banking secret.

Finally, chapter 5 addresses the issue whether even fully anticipated fluctuations induced by monetary policy (that is without any risk) can have an effect on capital accumulation. Employing a monetary growth model with an infinite horizon it is shown that inflation variability is nonneutral with respect to the capital stock, even when there is no uncertainty attached. The magnitude of the effect depends, inter alia, on the average level of inflation.

In sum, Rötheli has written a thorough study of the various ways in which the choice of the monetary regime influences open economies by changing the risk characteristics of inflation and exchange rates. Despite the extensive use of mathematical tools to illustrate the various propositions, the intuition behind the results is presented in a clear and non-technical fashion. The study can therefore be recommended to any economist interested in the increasing international integration of financial markets and the resulting interdependencies among national policies.

Klaas Knot

Roger Guesnerie, *A Contribution to the Pure Theory of Taxation*, Cambridge University Press, Cambridge, UK, 1995. £ 35

The title of this volume in the *Econometric Society Monographs* series adequately reflects two characteristics of this book. It is a *contribution* to the pure theory of taxation: it is a monograph mainly based on earlier work authored or coauthored by the writer, presented here in a more or less extensive form. Second, it is a contribution to the *pure theory* of taxation: it is – indeed – purely theoretical and of a relatively high mathematical sophistication.

In this monograph informational constraints related to taxation play a central role. The theory of taxation is viewed as being closely linked with the theory of incentives, mechanism design and implementation, following the pioneering work of Diamond, Mirrlees, and Hurwicz from the seventies. This link is explored in particular in chapter 1 (*Institutional Economics of Taxation*), in the framework of a two-good economy (labour and consumption) with a continuum of agents characterized by privately known one-dimensional characteristics affecting personal productivity. There is a fixed level of public good that has to be balanced by tax revenues. The distribution of individual characteristics is common knowledge. It is the central planner's task to design a taxation scheme (personal transfer scheme) that solves the second-best problem of maximizing total utility under the informational constraint. Parallel to the *revelation principle* in the design of mechanisms that are Nash or Bayesian-Nash implementable, in the context of taxation too, there is no loss of generality in only considering direct mechanisms, in which agents are asked to report their characteristics. However, again parallel to mechanism design theory, this insight is of mainly theoretical value – its proof is a logical rather than a mathematical or economical argument. The practice of taxation is of course quite different, and characterized by the use of linear or piecewise linear income and transaction tax schemes. An important question therefore is: How much is there to be gained by considering more general but still anonymous transfer schemes? A conclusion of chapter 1 is that there is nothing to be gained if Bayesian-Nash implementation is adopted (which is quite natural in the described model) and if there is no correlation between the agents' individual characteristics.

Chapter 2 (*Positive Economics: the Structure of Tax Equilibria*) introduces the model that serves as the basic model for the rest of the book, and which is a variant of the Diamond-Mirrlees model of 1971. It is a general equilibrium model involving consumption, production, and a public good. The central concept under study is the *tax equilibrium*, and chapter 2 is concerned with an investigation of the structure – local as well as global – of the set of tax equilibria. This is the chapter with the highest level of mathematical sophistication; it uses results from differential topology, in particular a variant of Sard's Theorem. The results of this chapter concerning the structure of the set of tax equilibria are relevant for chapter 3 (*Normative Economics of Taxation: Reform and Optimization*), in particular the so called connectedness problem: if the set of tax equilibria is not connected, then local gradual tax reforms may remain within the same component of the set of tax equilibria, so that better equilibria in other components are not reached. Chapter 3 also investigates the possibility of extending local (comparative statics) tax reforms to larger reforms; mathematically, this involves classical results concerning the extension of a solution of a differential equation to a maximal domain. Tax reforms are considered from two criteria: move to Pareto superior equilibria (within the set of equilibria), or impose a social welfare function that has to be improved. The first approach is basically the approach of optimal taxation. Chapter 4 (*Normative Economics of Taxation: Further Essays on Optimization and Reform*) gives a more detailed exploration into tax reform issues and in particular into the theory of optimal taxation.

The final chapter 5 has a more tentative character and deals with the *Political Economics of Taxation*. The 'government' or 'planner' featuring elsewhere in the book is modeled here in more detail. One approach is to view the determination of taxes as a social choice problem. It is established, however, that only under heavy restrictions the taxation problem can be turned into a problem of voting with 'Euclidean' single-peaked preferences. A second approach is to view the taxation problem as a coalitional (non-side payment) game; here, the emphasis is on the study of the core.

This book requires a great deal of maturity on the part of the reader. One should be used to second-best ideas, general equilibrium and game-theoretical thinking in order to really appreciate it. On that level, however, it is a pleasure to see so much mathematical sophistication combined with deep economic intuition.

Redundant to say that this work is not suited as a textbook. On the other hand, researchers who really want to understand all details have to consult the original manuscripts or articles as well, because at many places the arguments (e.g., the mathematical proofs) are rather sketchy. Statements 'left to the reader' to check or prove in this book are seldomly straightforward exercises. A nice feature is the long *Introduction* preceding chapter 1, in which the rest of the book is outlined in relatively much detail. Chapter 1 contains a concluding section of two pages; unfortunately, in the other chapters this section is much shorter or non-existent. Each chapter ends with an extensive biographical note.

Hans Peters

C.W.M. Naastepad and S. Storm (eds.), *The State and the Economic Process*, Edward Elgar, Cheltenham, 1996. Pp. xvi+264. £ 49.95

The book contains eight contributions on the state and the economic process. Seven of them were discussed at an international conference organised by the Centre for Development Planning of the Erasmus University Rotterdam and the Dutch-Belgian Association of Post-Keynesian Economics, and afterwards reworked in the light of the discussions. Each of the contributions is followed by a short comment. The eighth contribution is the introduction of the book written as a background to the chapters.

The book discourses upon the role of the state in the economic process at large. Successively it addresses the situation in developing countries, in OECD countries, in transitional economies, and the possible contribution of the state to a sustainable world system. On the one hand this means that a broad public will be able to find one or several chapters that lie in the personal sphere of interest, on the other hand that – despite the binding introduction – the coherence between the chapters is limited. This lack of coherence is strengthened by the differences in analytical approach chosen by the different authors.

The first part of the book on the role of the state in developing countries consists of a contribution by Bhaduri. He presents an interesting analysis of the possible strategies for large, predominantly agrarian economies, focusing on the participation ratio, the occupational distribution and the sectoral productivity of labour. As far as the role of the state is concerned he sketches a balanced picture of possible market and government failures and stresses the need to attune the role of the state to the stage of economic development. Special attention is given to the time sequence of investment decisions in the process of industrialisation. In contrast to most conventional thinking Bhaduri argues that this sequence should not be determined by supply-side linkages, but primarily by considerations of final demand.

The second part of the book consist of two chapters on OECD countries. Glyn analyses the evolution of labour-capital relations since the Second World War and tries to identify the role of the state in the golden age boom and its collapse. His main conclusion is that the balance of power between labour and capital, rather than the ability of the state to use Keynesian measures, was the key problematic element. Illustrative for Glyn's view are his statements that the post-war boom was not based on a mutual beneficial settlement between capital and labour, and that the importance of the Marshall Plan was less in the fact that it helped relax the acute dollar shortage but more in its contribution to the restoration of social and financial discipline. While stressing strongly the importance of labour-capital relations Glyn fails to recognize that the class structure of post-industrialised societies is less clear-cut than in the past and the simple dichotomy between labour and capital has disappeared. His analysis also does not do justice to important structural processes as the development of information and communication technologies and internationalisation.

Lipietz also sketches the development of OECD countries in the post-war period, but focuses on the trends in the organisation of labour. The crisis of Fordism and the search for alternative organisational models are seen as the thriving forces. In this interesting chapter Lipietz describes the various routes that have been taken by different OECD countries, in their balancing act between what he calls liberal flexibility and negotiated involvement. While doing so he consciously ignores that, given the process of internationalisation, describing trends in regulation models for countries or even groups of countries has become a risky exercise. Lipietz' choice to oversimplify enables him to sketch the effects of those strategic choices on the international division of labour and to explain how different forms of labour-capital relations and different wage regimes can coexist within an integrated continental bloc.

Part three of the book contains two interesting papers on the role of the state in post-communist economies in transition. Nuti gives a sound description of the near-demise of the state in the transition process. However, his analysis of the underlying reasons for de-statisation in those countries is not very new and his conclusion that alternative routes could not have been viable in the given situation is not very well motivated. Braguinsky starts his contribution with an interesting endogenous theory of transformation. This theory explains the evolution of the planned economy in the Soviet Union since 1917 from changes in the relation between the planning authorities (the principal) and the state-owned enterprises (the agents). The second part of Braguinsky's contribution focuses on the state under transition. He concludes that the state in the transitional process should not try to design the general direction of the transforming process, but should give selective support to tendencies through policy proposals that are based on the self-interest of economic agents.

The fourth and last part of the book that deals with a sustainable world system, is not very convincing. Taylor's chapter on environmental feedbacks in macroeconomics presents an overview of the ways in which macroeconomic variables interact with environmental variables that might be interesting for readers who do not deal with environmental issues on a daily basis. For more specialised readers, who have experience with integrating environmental issues in economic modelling, his analysis probably does not offer much surplus value. Sachs' chapter on ways to revert the present trends toward economic and social polarisations within nations and across nations, was originally written for the March 1995 Summit on Social Development in Copenhagen. Perhaps this explains why his unstructured contribution reads more as a political pamphlet than as a scientific analysis. In itself this does not disqualify Sachs' chapter. However, the way Sachs describes the world-wide emergence of what he calls 'unbridled social Darwinism' and presents far-reaching policy proposals for South, East and West in less than 15 pages, is far from convincing.

To conclude, in my view this book contains a mixture of contributions, some of which are very interesting. However, despite the existence of a central theme the book suffers from a lack of coherence. As a consequence it is hard to recommend the book as a whole. Most readers will have to select the chapters that fit their personal sphere of interest best.

Edwin Oskam

A.M.G.M. Spithoven and Y.S. Brenner, *Mijlpalen in het economisch denken*, Boom, Amsterdam 1996. Pp. 285. Dfl. 67

On the history of economic thought many books of high quality are nowadays available. Also in Dutch several books come to mind, such as the works of L.J. Zimmerman and F. Broekman, both under the title, *Geschiedenis van het economisch denken* (History of Economic Thought). Of course, a new book in Dutch on the same topic cannot be introduced without reasonable justification. Its quality must at least be comparable with the books we already find on the shelves and in the classrooms.

The authors' intention seems to have been to relate the economic theories discussed to their respective social backgrounds. It is highly unclear however, what the authors exactly have in mind and the actual description of the theories does not provide us with a clue. I feel that the book can be read as a straightforward story on the development of economic theory. The story begins with the mercantilists and the physiocrats, moving to the classical economists, Smith, Ricardo, Malthus, Mill, and Say and then to Marx and Engels, up to neo-classical economists, like Jevons, Edgeworth, Walras, Pareto, Fisher, and Marshall in order to end up with Keynes and his aftermath. It is interesting to note that Cournot is missing in the *tableau de la troupe*.

I am concerned about the quality of the book and the impact this might have on the lessons about the history of economic thought at the University of Utrecht. Not on the history of economic thought itself but more on the teaching of economic theory. Let me give a few examples to illustrate the background of my concern.

According to the authors (page 34), Ricardo wrote the first seven chapters of his *Principles* between the outbreak of the French Revolution in 1789 and the end of the Napoleontic wars in 1815. In fact, Ricardo – born in 1772 in London – started thinking about the *Principles* during the course of 1815 and on 14 October 1816 an extensive draft, covering the ground of the first seven chapters was sent to James Mill, the spiritual father of the project. The date of publication is 19 April 1817. The speculations of Spithoven and Brenner, based on the idea that Ricardo was writing over a period of twenty five years, are therefore utterly incorrect. Contrary to what is written by the authors there is no chapter 31 on machinery in the original edition of the *Principles*. The first edition only contained 29 chapters in total. As is widely known the famous chapter on machinery, chapter 31, is included in the third edition of 1821. The authors refer to historical events to suggest that Ricardo wrote this chapter in 1816 or 1817, but the fact of the matter is that it was actually written between December 1820 and February 1821. Ricardo's pessimistic view of the effect of machinery on labour is a complete change of opinion in the 1821 version. Spithoven and Brenner's statement that Ricardo presented a pessimistic view on mechanisation in the first edition of the *Principles* is therefore obviously incorrect. The phrase on page 31 stating that Ricardo was old enough to remember the ten years before the French Revolution in England is peculiar as he was seven in 1779 and furthermore lived in Amsterdam from 1783 to 1785. To discuss Ricardo's value theory without a reference to his distinction in reproducable and non-reproducable goods is comparable to the treatment of Quesnay without reference to the *Tableau Economique*.

The authors are not aware of the fact that the first edition of Malthus', *An Essay* of 1798 was published anonymously. There is no question of a 'pamphlet without a title' (page 45). The statement that the economic theory of Malthus is identical to that of Smith and Ricardo, is not only in striking contrast with their own treatment but also with the ongoing debate on Ricardo's theory, a debate that is completely lacking with respect to Malthus. In discussing Edgeworth the authors confuse indifference curves with the contract curve. They do not seem to understand why indifference curves are introduced. They are apparently unaware of the concept of utility functions. This explains sentences like 'when an individual disposes of a certain quantity of one good, he attaches another utility to this quantity than when he can also dispose of another good. This makes utility even less measurable and brings it back from an almost unmeasurable quantity to the chaos of an infinite number of influences' (page 110). I have unfortunately found a very great number of these shameful proofs of lack of understanding of the basic principles of economics. A case in point is their comment on the wellknown Fisher-equation $MV = PT$ on page 123. That the authors have great difficulty with Paretian welfare theory comes to the fore on page 119 as they confuse the Pareto-criterion with Pareto-optimality. Their criticism of neo-classical economics is based on the idea that economists like Marshall 'assume that the form of the curve of the utility function is negative' (page 135). It is, in short, difficult to take the authors seriously.

Led me add two other examples. Spithoven and Brenner present the comparison of Keynes with the Classical Theory in terms of ex post identities like $Y = C + S$ and $Y = C + I$, without realising that no analysis whatsoever is involved (page 156). In a graph on page 168 they represent consumption and output on a vertical axis and income and employment on a horizontal axis, yet still describe the 45°-line as $Y = C+I$. Samuelson's famous multiplier-accelerator model is presented as a first-order difference equation, while the essence of his model is reflected in a second-order difference equation. The authors do mention this equation, but it is clear from the verbal text that they do not understand what it is all about.

I regret that I cannot recommend this book. It should never have been published. It needs complete rewriting from beginning to end. My suggestion is that they ask Dr F. Broekman to come to their rescue.

A. Heertje

Paul de Beer, *Het onderste kwart; werk en werkloosheid aan de onderkant van de arbeidsmarkt* (*The Bottom Quarter, Employment and Unemployment at the Lower End of the Labour Market*), Sociaal en Cultureel Planbureau, 1996, VUGA Uitgeverij, Den Haag. Dfl. 45.50

The economic functioning of the lower end of the labour market is a major policy problem in The Netherlands. Unemployment, and more specifically long-term unemployment is especially high among those with low educational attainment. Ethnic minorities, Turks and Moroccans in particular, are strongly overrepresented in jobs at the lower end of the labour market and a large majority of youngsters (under the age of 25) have a low-paid job. Nevertheless, as the calculations in the book under review show, half of the jobs in the bottom quarter are held by people of 35 and over, and by people with secondary or higher qualifications, and 4/5 are held by Dutch people.

The present study by De Beer of the Social and Cultural Planning Bureau provides extensive scrutiny of this major policy problem in The Netherlands. The first part of the book tries to give a proper definition and demarcation of the lower quarter of the labour market. A number of different microdata-sets are used to paint a detailed, quantitative picture of this part of the labour market. Firstly it is investigated which kind of *jobs* can be qualified as work in the lowest quarter. It appears that more than half of these jobs are found in the industrial and agricultural sectors, and in transport. These three sectors comprise only one quarter of total employment in The Netherlands. Next, the author portrays the characteristics of the *workers* at the lower end of the labour market. Three clusters are distinguished, namely low-paid flexible work, traditional low-skilled full-time work, and part-time jobs in services. It appears that the number of low-skilled workers at the lower end of the labour market has decreased considerably during the last 15 years, whereas the number of the workers belonging to ethnic minorities has increased. Next the author analyzes *unemployment*. Again the low-skilled workers and ethnic minorities appear to be overrepresented.

The final chapter of the descriptive part of the book deals with the empirics of the dynamics at the lower side of the labour market. Here the author discusses the fulfilment of jobs, the inflow into unemployment and the determinants of the escape probabilities from unemployment. Unfortunately this analysis does not yield a consistent picture of labour market flows at the macrolevel, but it solely focuses on a micro-oriented approach of deriving escape probabilities from panel data and retrospective questions in the survey. Therefore only partial insight is obtained in the processes of job destruction and job creation in this quarter of the labour market and on job mobility from this part of the labour market ('bad jobs') to the upper quarters of the labour market ('good jobs'). Some data are given on the probability of upward mobility, but again no information on the size of flows can be derived from these probabilities for individuals.

The second part of the book has an analytical character and focuses on explanations for the developments at the lower end of the labour market, and on policy analysis and prescriptions. The theoretical argumentation is eclectic: the relevance of various theories of labour economics is discussed for the Dutch situation. Moreover, an extensive survey is given of the results of a large number of micro-oriented studies on specific aspects of the Dutch labour market, for instance the employment behaviour of firms and the relationship between the reservation wage of the unemployed and the type of work offered.

With respect to labour market policy the author notes that there has been a shift from a general policy to specific policy measures, which aim at enhancing the labour market position of the weakest groups. So many different measures have been taken that labour market policy in The Netherlands in the last decade can be regarded as one big experiment. Yet, most of these measures were taken without a proper evaluation of their effects.

Although De Beer's picture of Dutch labour market policy in the last decade is quite correct, he somewhat underexposes the discussions on labour market policy at the macrolevel. The wedge between gross wage costs and net earned wages played a central role in these discussions. In the formation period of the Kok-government there was some consensus on the fact that the wedge should not only become smaller by a reduction in government spending, but that there should also be a *twist* of the wedge so that low-paid labour should become less expensive relative to high-paid labour. These discussions, which are hardly mentioned in De Beer's book, were both on the employment effects of this twist of the wedge – it was referred to as Robin Hood policy which results in an increase in the marginal wedge – and on the question whether the twist of the wedge should be implemented by a reduction of taxes paid by employers or by employees. Tax reduction for employees was proposed by the Advisory Commission on the lower segment of the labour market (the so called Andriessen Committee) which suggested to give every employee a tax reduction of 200 guilders per month in order to stimulate supply at or just above the existing minimum wage level. However, in the policy agreement of the Kok-government and consequently in the effected policy measures the twist of the wedge was obtained by a reduction of the employers' taxes, which is, in first instance, meant to affect demand. Of course, in the long-run context of general equilibrium it makes no difference whether the reduction and the twist of the wedge is implemented via the employers or the employees. Yet, the long run is too far ahead in actual policy implementation.

De Beer does not derive clear and pertinent policy conclusions from his extensive investigations. He seems to be in favour of specific (or targeted) policy measures instead of grand designs for new policies. He holds the opinion that every little contribution helps. I would agree that at present there is no need for such grand designs and revolutionary proposals, which only work in the ivory towers of the proposers. Yet I think that De Beer lays too much emphasis on specific policy measures, whereas a general policy which consistently takes labour market developments at the macrolevel into account, is equally important or even more important in the present situation. This accords with the results of recent academic research on the working of the labour market, which questions the effectiveness of policy measures targeted at specific groups or sectors of industries (see e.g. Davis, Haltiwanger, and Schuh, 1996).

Finally, De Beer suggests that only a radical change of direction would produce significantly better policy results in the short term. Two possibilities he mentions are a variation on the formal Swedish model of strong government intervention, and alternatively, a more Anglo-Saxon approach with far-reaching liberalisation of the labour market. I agree with De Beer that it is by no means certain that the improved prospects for disadvantaged unemployed people which might result from such a change in direction, would sufficiently offset the drastic social and economic consequences. I would even advocate that the so called Rhineland or Polder model, which now receives increasing admiration from our neighbours, is much more adequate for The Netherlands than one of the two alternatives mentioned above. Here, there is the need for a gradual reconstruction of the generous social security system into a system which reactivates people to employment.

F.A.G. den Butter

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J. Mulberg, *Social Limits to Economic Theory*, Routledge, London, 1995. Pp. 200. £ 45

The title of this book is a bit puzzling. The book offers a methodological criticism of *mainstream* economics and discusses how methodological shortcomings have worked out in the history of economic thought. The main argument is that *mainstream* economics is unable to adequately deal with many real life problems because of its positivistic outlook. At the root of the alleged lack of realism are, according to the author, frictions between the claims of positivism. On the one hand we have the requirements that scientific knowledge ought to be value free, that it ought to be based on empirical testing, and that it ought to enable predictions about the real world. On the other, we have the claim that positivistic and hence objective knowledge can develop instruments to get in control of the real world which are far superior to those instruments founded on subjective viewpoints. At the heart of Mulberg's criticism on economics lies the contention that a social science which seeks control of the real world necessarily embodies a political theory. It cannot be value free. In economics this political theory is, according to the author, largely hidden for those engaged in normal science. A metatheoretical analysis is needed to bring it out. Such an analysis is the core of Mulberg's book. The author stresses that his conclusion is most unfortunate for the positivists among economists, a majority of which has a strong preference for a market economy, since the implicit political theory in the economists' positivistic research programmes appears to be 'planning'. The next step in the argument is that the social limits to economic theory are the subjective political convictions of the economists themselves. '*This book will suggest that the lack of realism in economics is a defensive strategy which has been invoked to avoid the political conclusions to which economic theory would otherwise lead.*' (p. 3) Well, that is quite something!

Statements like this one are, of course, fatal to the joy of reading. However, the persevering reader, who refuses to be knocked sideways, will find out that the author tries to substantiate his reproach rather subtly and that he discusses well-documented selected subjects in the history of economic thought with great insight. Chapter 1 is on methodology, chapter 2 discusses the history of utilitarianism, chapter 3 reviews the debates on market socialism, chapter 4 is about American institutionalism and chapter 5 about modern institutionalism. In the last chapter the author argues, not surprisingly, for a non-positivistic research agenda in economics and explicits his own (green or environmental) guidelines for research. The main arguments of the book are found in chapters 1, 2, and 3, so I confine myself to a discussion of these.

Chapter 1 brings up many intricacies of economic methodology. Since measurement without theory leads nowhere, a theory is needed to identify regularities. This is where, according to the author, social values slip into economics. The author's point is that in economics the unifying theoretical principle, which is rational choice, is strictly an individual evaluation and that there is no way to establish an empirical link between this principle and empirically observed phenomena. This means that the framework in which these phenomena are studied is a matter of interpretation. A scientific statement along positivistic lines necessarily depends, according to him, on sticking to all three of the following claims.

1 Economic knowledge is only known to individuals.
2 This knowledge is capable of aggregation.
3 From this, one can arrive at the best overall outcome.

What the author is getting at becomes clear when he brings up an alleged preference of many economists for an unhampered market functioning. Any positivistic statement that the market system leads to the best overall outcome (claim 3) must confront the two other claims. Having to drop one of them would mean that a preference for market economy cannot be founded scientifically. But, according to the author, dropping one of them is precisely the thing which is inescapable. That the three claims cannot come together is exemplified in his discussion of the history of utility theory and the debates on market socialism.

Chapter 2 describes the history of utility analysis from Bentham's felicific calculus, which assumes that additive individual utilities can be measured by a third party, to our modern utility concept, which is founded on claim 1. This is also the story that utilitarianism started as a political movement striving for the greatest happiness for the greatest number of people and that the classical economists, who endorsed this political goal, added to it the expectation that this goal can be realized by *laissez faire*. The chapter offers a review of the contributions by Bentham, Jevons, Edgeworth, Marshall, Pigou, Pareto, Robbins, Hicks, Samuelson, and Scitovsky. The main argument is that as a side product of the development starting with Jevons it became increasingly difficult and eventually impossible to arrive at a social welfare function (claim 2) and that what remained of utility analysis is '... *an abstract theory devoid of practical application.*'

In chapter 3 on market socialism the author tries to convince us that economists either have to admit that their implicit political theory is 'planning', or have to give up the pretention of doing positivistic science. The rhetoric in this chapter is a combination of deduction and storytelling: by relating the different viewpoints of the participants in the debates in the thirties to the methodological principles as explained in chapter 1, the author tries to propound that the outcome of the controversy could not have been otherwise. The story about market socialism functions, of course, as a pars pro toto for economic theory. Basically, the story is the following. The market socialists, among who Lange and Lerner, strongly believed that although utility evaluations are strictly individual (claim 1), government agencies can act as a Walrasian auctioneer and gather summarized information about preferences and endowments (claim 2). This information allows the socialistic government to simultaneously develop the theoretical instruments to evaluate the market outcome and the policy instruments to arrive at the socialistic goals (claim 3). Hence, the market socialists are the truly positivists. The Austrians, in particular Hayek and Mises, started their criticism with the argument that the necessary information, however summarized, cannot be obtained (claim 2 is dropped). In the logic of the author this had the consequence that the Austrians gave up the possibility of developing along positivistic lines instruments to evaluate the market outcome (claim 3 must be dropped). The subsequent developments in Austrian theory, which the author describes, are well known. Hayek was to reject the positivistic research programme in economics altogether, calling it 'scientism'. The rather libertarian outlook of the Austrians on the market economy was founded on what comes close to evolutionary principles: since only with free choice individuals will learn about their possibilities by interpreting price signals, the market must show up a tendency in time to allocate resources at its best uses. No Walrasian theory is needed to argue that.

What to think of all this? Let me start with the good things. There can be no doubt that the book is extremely well documented. The author certainly covers the relevant literature, in both methodology and the history of economic thought. As a Dutch reader I was pleasantly surprised to find Pierson's famous article on value quoted. The treatment of the sources is as a whole reliable. And, apart from the many statements of the kind I quoted at the beginning of this review, the book is highly readable.

My problem with the drift of Mulberg's argument is the following. It should be pointed out that at the heart of his criticism on positivism in economic theory are the problems with the subjective embodiment of political knowledge, i.e. knowledge capable of developing instruments to steer social processes. Indeed, when the author ascribes to the positivistic economist the ambition to get in control of the world, he confronts his readers with a modern variation on the high theme of the philosopher-king, well known from old Greek philosophy. For centuries philosophers have elucidated the failures of this concept of despotism. The best known argument, which is taken up by Mulberg in the context of positivism in social sciences, is that it is inconceivable that our philosophic or scientific autocrat can reach such a level of disengagement from society that he can be considered objective. The solution to this problem is not easy, particularly not when it is assumed that those who have the knowledge also strive for the power to control the world (Mulberg's claim 3). In my opinion the solution is certainly not bringing the battle of ideas altogether in the political arena by arguing for a politically engaged scientific research programme, as the author does. For even if the social preferences and consequently the research interests of the individual economists differ, they all should try hard to engage in a high-quality conversation about their subject with their colleagues, even if this colleague is a poor lonely dissenter.

It is a bit of a pity that the author studied only one Dutch article (in translation) and that he had no access to the rich Dutch literature on the theory of economic policy, largely written in the sixties. (Its main contribution has only recently been translated: P. Hennipman, 1995, *Welfare Economics and the Theory of Economic Policy*, Edward Elgar).

Had he done so, it could have occurred to him that instrumentalism in economic theory must be carefully distinguished from partisanship. To be sure, there is nothing wrong with arguing, as John Stuart Mill did, that economic theory must be looked upon as the theoretical foundation of economic policy. But the very foundation of a democratic theory of economic policy is – in contrast to old Greek despotism – the functional separation of tasks between those engaged in research, in policy advising, and in setting the goals of economic policy. In a democracy the goals of economic policy are simply not the territory of the economic theorist, whether he or she is a libertarian, socialist, or environmentalist.

Henk W. Plasmeijer

John Smithin, *Macroeconomic Policy and the Future of Capitalism. The Revenge of the Rentiers and the Threat to Prosperity*, Edward Elgar, Cheltenham, 1996. Pp. ix+150. £ 45

The subtitle of this book refers to the closing chapter of Keynes' *General Theory* in which Keynes argued that a sufficiently low level of interest rates would be the best means to promote prosperity. A low (real) interest rate stimulates business investment and employment but it does not favour the rentiers who thrive on high interest rates. Keynes therefore argued that the 'euthanasia of the rentiers' caused by low interest rates would actually be a good thing for the economy. However, John Smithin's main claim in his book is that nowadays the rentiers or, in other words, the financial investors are not a rare species at all. On the contrary, since the beginning of the 1980s macroeconomic policy in the industrialized countries has primarily resulted in relatively *high* real interest rates thereby serving the interests of the financial markets at the expense of business and labour. In his view policymakers in general and central banks in particular focus too strongly on nominal targets (like inflation, budget deficits, and the exchange rate) and put far too less weight on realia. Things were different according to Smithin in the first decades of the post-war period when macroeconomic policy helped to maintain a strong level of aggregate demand with only moderate rates of inflation and relatively low, albeit positive, real interest rates. This situation benefited not only business and labour but it was also acceptable for the financial markets. This fruitful compromise fell apart in the 1970s when policymakers permitted inflation to get out of hand and the rentiers were confronted with negative real interest rates. Smithin states that after the second oil crisis macroeconomic policy (notably monetary policy) has been far too restrictive. This enabled the 'revenge' of the rentiers because deflationary policies boosted real interest rates. This shift in macroeconomic policy was stimulated by the developments in academic research which vindicated the idea that there is, certainly in the long run, no trade-off between inflation on the one hand and output and employment on the other hand. How should we end this heyday of the rentiers? Smithin thinks that a return to the aforementioned compromise between business, labour, and the financial markets is the way to go. This can to a large extent be achieved if the emphasis in macroeconomic policy shifts somewhat from achieving very low inflation rates towards stimulating output and employment.

This brief summary of the main argument of the book illustrates that this is certainly not a book by a mainstream economist. The author advocates a shift in macroeconomic policy that will be considered as near intellectual heresy by some mainstream economists. More or less accepted ideas in mainstream or neo-classical economics are questioned by Smithin. This is especially clear in the chapters 5, 6, and 7 which deal with inflation, the budget deficit, and the exchange rate, respectively. With respect to all of these nominal variables Smithin claims that the conventional analysis has got it wrong. Despite (new) classical claims to the contrary, the trade-off between inflation and output is alive and kicking and the costs of inflation are exaggerated whereas the costs of output and employment loss of a deflationary policy are greatly underestimated in his view. Consider for instance the following statement (Smithin is a Canadian-based author): 'It is a serious question whether, in the Canadian context for example, life was better for most people in 1992 (with an inflation rate of 1.5 per cent and an unemployment rate of 11.3 per cent) or in 1979 (with an inflation rate of 9.2 per cent and an unemployment rate of 7.4 per cent). Yet such questions do not seem to get asked in esoteric academic discussions of rational expectations, policy rules, shifting Phillips curves, and the time-consistency properties of various dynamic models' (p. 71). Indeed, these questions are not frequently asked but for good reasons in my opinion if only because the 'esoteric' discussions Smithin refers to have made it clear that macroeconomic policy is more complicated than choosing a point on a negatively sloped Phillips curve. With respect to the other two nominal variables, the budget deficit and the exchange rate, Smithin also questions conventional ideas. Budget deficits are not the cause of high real interest rates but are instead more likely to be caused by high real interest rates with the latter being largely determined by monetary policy. In a similar vein, the importance of targeting the nominal exchange rate is overrated and Smithin applauds countries (like the UK in 1992) which opt for a strategy of monetary sovereignty. Each country should be able to strive for a level of real interest rates that best suits its economy and this may very well imply (p. 114) that a nation can succeed in depressing its (real!) interest rate levels below those prevailing elsewhere. The question as to how this can be achieved in our interdependent world is, however, not answered by the author.

At some instances the criticism of conceived wisdom is well-argued (e.g. policymakers do have the tendency to forget that disinflationary policies are not without their real costs). Unfortunately, in most cases the author is just controversial in the sense that he merely states that the bad guys (central bankers, financial markets, and academic economists) are bad without convincing the reader why they are so mistaken in their beliefs about macroeconomic policy. In this respect the book is more a pamphlet than an in-depth analysis of, let us say, the theoretical flaws of neo-classical macroeconomics or the empirics of the real interest rate. On the other hand, in an era in which economic models often still have a place for the representative agent only, it is at least refreshing to read about an economy in which different (opposing) classes are distinguished. In the end, however, the book (which is well-written) claims too much and argues too little. As an antidote to the prevailing classical views on macroeconomic policy it is worthwhile to read this book, but Smithin does not offer a substantive alternative for 'the future of capitalism'.

Harry Garretsen

Peter B. Kenen, Francesco Papadia, and Fabrizio Saccomanni (eds.), *The International Monetary System*, Cambridge University Press, Cambridge, 1994. Pp. xv+342. £ 40 (hardcover), £ 16.95 (paperback)

This book contains the proceedings of a conference organized by the Banca d'Italia to honour the memory of Rinaldo Ossola, the former Director General of this bank and Chairman of the Deputies of the Group of Ten (1967–1976). The book is divided in ten chapters, each containing one paper plus discussion, covering three broad areas: international liquidity, exchange rates, and policy coordination.

The book starts with an overview of the topics at hand. In two papers, Jacques Polak and Guido Carli sketch postwar discussions and developments in the international monetary system. Polak's paper is the most extensive and focuses on problems of adjustment and liquidity which have dominated many discussions in the Bretton Woods period. With the benefits of hindsight he analyses these discussions and concludes that adjustment and liquidity problems are still with us. The question whether there still is a liquidity problem in the world economy is discussed in papers by John Williamson, Andrew Crockett, and William Branson. Williamson is clear about the relevance of this liquidity problem: 'unless capital mobility collapses, the set of ideas that underlay the debate on international liquidity has become irrelevant' (p. 63). His main argument is that growing capital mobility has decreased constraints on financing problems for countries. Creditworthy countries can always attract enough money. However, if countries are not creditworthy (e.g. developing countries) they still face a liquidity problem. Therefore Williamson launches the idea to reallocate SDRs in the direction of developing countries to alleviate their financial problems. Andrew Crockett continues on this topic and he broadly agrees with Williamson that the integration of financial markets has diminished the liquidity problem for a number of countries. Further, he argues that there is no evidence that the demand for international reserves had reduced or become more unstable in the post Bretton Woods period. Hence the increased elasticity of the money supply that at least creditworthy countries face, has been reducing the importance of discussions about international reserves as a tool for international economic management. His paper ends with a plea for increased control over international liquidity because this enables influence over national macroeconomic policies. Branson focuses on the unusually high interest rates during the slowdown in the world economy (1989–1992). 'Is the problem a shortage of saving or of liquidity?' It is obvious that the answer to this question has implications for monetary and fiscal policy. Branson analyzes this problem first with the help of a world *IS/LM* model and shows the adjustment paths that the economy follows after (un)anticipated shifts in liquidity demand and excess saving. Observing that the reduction of excess saving in Central and Eastern Europe was of major importance, he analyzes their consequences in a two-country model (Germany-US). He concludes from these models and macroeconomic data covering the period 1989–1992 that an anticipated reduction in excess saving has caused the high interest rates, and tightening fiscal policies would be the appropriate policy answer to reducing interest rates.

Part III discusses exchange rates. Peter Kenen starts with a paper in which he relates recent theoretical insights to the discussion about which exchange rate regime is to be preferred. Two insights are important. First, the understanding that fighting inflation is very important and second the pessimism that democratic governments can not really commit themselves to fighting inflation vigorously. Hence, fixed exchange rates are often proposed to make monetary policy more credible. However, 'why should a commitment to a pegged exchange rate be more credible than a straightforward commitment to price stability?' (p. 144). I can agree with this hesitation about the commitments. Finally, Kenen presents evidence showing a long-term trend towards more flexible rates. Obviously, he concludes, the appeal of exchange rate flexibility is still very strong. Further, he notices that many countries that returned to flexible rates have been unable to reduce their inflation rates. Although this is not a new fact, it strengthens the evidence that fixed exchange rates do not automatically guarantee credibility of domestic monetary policy. In discussing exchange rate systems, the question whether a system of fixed but adjustable exchange rates is stable in the long run often comes to the front. This question is adressed in the paper by Zhaohui Chen and Alberto Giovannini. In their view, there is a clear link between fixed exchange rates and expectations held by agents in financial markets and therefore they focus on the determinants of expectations of *parity* changes. Despite some theoretical remarks, the paper mainly consists of a number of regression results from which it can be concluded that the per cent deviation of the exchange rate from the centre of the band and the occurence of a recent realignment are the main determinants of expectations of parity changes. Not very surprising, I think, and not enough evidence to conclude that fixed but adjustable systems are not stable, as the authors claim. Nevertheless, their paper is interesting because of the methodological problems they sketch in constructing an expectations variable. This part ends with a paper by Pietro Catte, Giampaolo Galli, and Salvatore Rebecchini concerning the question whether concerted interventions have had any effect on the dollar rate. By an accurate analysis of daily data they conclude that, in many cases, concerted interventions have indeed reversed the movement of the dollar rate. Although their method of analysis can be heavily criticized, I think that their evidence opens a lot of possibilities for research concerning the effectivity of (concerted) interventions.

Part IV discusses the international framework for national economic policies and contains two papers. Tamin Bayoumi and Barry Eichengreen start with a paper in which they compare the economic performance of the G-7 countries under alternative exchange rate regimes using a simple *AS/AD* diagram to analyze this performance. Their aim is to analyze whether unwillingness of politicians to commit themselves, change in the variety and magnitude of demand and supply shocks, or the economy's capacity to adjust to disturbances can be held responsible for the continuous changes in exchange rate stability. They conclude that the slope of the short-run *AS*-curve has decreased since the start of this century, implying a loss of macroeconomic flexibility over time.

Morris Goldstein evaluates policy coordination, both in terms of theoretical achievements and practical procedures. In his view policy coordination is 'helpful and worth improving' (p. 302). This prudent attitude is kept throughout the chapter. Basically, Goldstein pleas for sound domestic policies ('which come close to being a necessary condition for exchange market stability' (p. 307)) to guarantee global economic stability. Concerning the practical procedures he stresses the need for follow-up procedures following the discussion between the Executive Board of the IMF and the member countries. Especially when these discussions result in policy advises and economic reforms, a follow-up procedure is necessary.

I found this book both informative and interesting to read. It discusses in a bird's eye view a number of problems related to the functioning of the international monetary system. Although the relations between the discussions during the Bretton Woods system and the discussions about the EMS could be elaborated, the relation between the topics that are discussed and topical problems is nicely developed. The book is interesting because it is also very rich in (provocative) ideas. For example, in one comment Richard Cooper holds a plea for one world currency as being the optimal monetary arrangement. Hence, if you are interested in problems related to the international monetary system, I can recommend this book.

Tom van Veen

Jagdeep S. Bhandari and L.A. Weiss (eds.), *Corporate Bankruptcy: Economic and Legal Perspectives*, Cambridge University Press, Cambridge, 1996. Pp. 550. £ 19.95

Attention to the nature and efficiency of bankruptcy law follows the number of business failures with a time-lag. An economic crisis leads to a wave of bankruptcies causing public and later academic attention to shift towards bankruptcy. The economic crisis of the early eighties led to the first systematic work on bankruptcy law, T. H. Jacksons' *Logic and Limits of Bankruptcy Law*. The crisis of the early nineties made way for the book under review, *Corporate Bankruptcy*. However, bankruptcy does not derive its relevance from the connection with economic crises. Bankruptcy is, despite the negative connotation, a useful mechanism for reallocating scarce resources. A business which uses resources inefficiently goes bankrupt and makes way for a more efficient competitor. It is a necessary ingredient of the process of 'creative destruction' as described by Schumpeter. This process of destroying old structures and thereby facilitating the rise of new and better ones increases the wealth of a society. The public opinion which considers bankruptcy to be harmful to business is therefore false. This book helps to understand the mechanism and economic rationale behind bankruptcy.

Corporate Bankruptcy consists of 32 articles (of which 31 have been published before) divided into six parts. Each part is introduced by some general comments from the editors. The editors hope that this wide array of practical and theoretical essays by various authors will provide valuable insights to bankruptcy professionals and scientists. Another goal is to demonstrate the interdisciplinary nature of bankruptcy law and policy. The book draws from insights in economics, law, and business and finance.

Since there is no such thing as bankruptcy without credit, the book commences with a part on the nature and role of credit. The three articles in this section explain in a very accessible way the benefits of debt. The standard debt contract does not exclusively derive its popularity from a tax advantage, but rather serves as a solution to an agency problem. Debt is a fixed obligation, i.e. not dependent on the amount of profit the company makes. If debt were dependent on the well-being of the company, the entrepreneur/equityholder would have an incentive to underreport profits.[1] The standard debt contract solves this problem. The ones that make the decisions about the course of the company bear the residual risk. The entrepreneur/equityholder has the right to keep all extra profits once all debtholders are paid. If he/she cannot pay off all debt, the entrepreneur/equityholder loses his stake and authority in the company. This creates the right incentives for the entrepreneur-equityholder. He will exert maximum effort both because he retains all upside potential and because failure to pay back debtholders will lead to the loss of his stake. The bondholders will only have to monitor the firm to determine if firm value is above the value of total debt outstanding. If this is not the case, then they may seize the assets. In this way debt serves as a bonding device to condition the behaviour of the entrepreneur/equityholder. Debt is efficiency-enhancing by increasing the pressure on the entrepreneur/equityholder to reach a certain amount of profit, by reducing the amount of financial slack available for empire-building and by concentrating the firms residual claim in the hands of the entrepreneur/equityholder who is responsible for the future of the company. These benefits are by now well known. It is strange that none of the articles mentions the origins of these insights which can be found in the 'Optimal Contracting' and 'Costly State Verification' literature. An article on optimal contracting would have fitted better in this section than Jensen's article on 'Agency Costs of Free Cash Flow'.[2]

The second section introduces the important notion of the 'creditors' bargain'. If a firm fails to meet its obligations, the creditors may run and seize the assets of the firm. This expected rat-race not only compells firms to closely monitor the actions of fellow creditors but may also destroy going concern value. These costs can be evaded if the creditors could coordinate their actions. The idea of the 'creditors' bargain' is that bankruptcy law imposes on creditors the deal they would have reached if they could overcome the coordination problem. The role of bankruptcy law is limited in this view. Bankruptcy law should be largely procedural and should alter pre-bankruptcy entitlements as little as pos-

1 The manager/equityholder can delegate power to a manager, but in this context this does not make a difference.

2 E.g. Allen, F. and A. Winton (1995), 'Corporate Financial Structure, Incentives and Optimal Contracting', in R. Jarrow et al. (ed.), *Handbooks in Operations Research and Management Science*, 9, Amsterdam: Elsevier.

sible. Otherwise some of the players (e.g. management) get perverse incentives to seek bankruptcy because their position inside bankruptcy is stronger than outside bankruptcy. The notion of the creditors' bargain is useful since it sets a clear goal for bankruptcy law and also limits its scope.

The third section elaborates on the creditors' bargain. It contains several empirical articles that investigate the deviations from the absolute priority rule (APR). The APR states that the most senior creditor should be paid in full before the next most senior creditor receives anything. Both contract law and the creditor bargain theory state that the APR should be strictly adhered to. In practice APR is often violated because junior creditors or residual claiments have power in the bankruptcy proceeding; e.g. equityholders can delay the proceedings and must give their consent to a reorganization plan. Although deviations appear frequently, they seldom concern a high percentage of firm value.

The small fourth section deals with the choice between a private settlement plan and the official reorganization procedure. The future of the firm in financial distress is the subject of a bargaining game. How much are the equityholders and creditors willing to give in on their claims in order to keep their firms outside the official procedure. Even in private contracting the bankruptcy law sets the limits of the bargaining game. A pre-packaged bankruptcy combines the benefits of private contracting with the benefits of the official procedure.

The fifth section questions the process of reorganization in a Chapter 11 procedure. It seeks to find a more efficient way to solve the coordination problem among creditors than the costly and lengthy court-supervised procedure. Several solutions are offered. One is just simply selling all the assets and divide the proceeds according to APR. If the firm is worth more to the incumbent managers or senior creditors, either group could make a bid. In this way the difficult valuation problem whereby a court must decide the value of the firm is resolved. L.A. Bebchuck comes to the conclusion that it is best to give the most junior creditors the right to buy the firm by paying off all the more senior creditors. If they do not wish to do so (because the firm is worthless) the next most junior creditor gets the right to buy out the remaining senior creditors. In this way the correct value will be assessed without asking the court to value the firm. Although a better bankruptcy procedure can be easily formulated in theory, in practice there is little chance of a radical reform. The reasons why the Chapter 11 procedure still exists are (perceived) problems with market-based valuations, institutional advantages given to Chapter 11 procedures (e.g. tax exemption), and resistance to change.

The last section examines bankruptcy laws in other countries and provides a comparison to US law. These chapters contain valuable insights and promising avenues for further research. Michelle White elaborates on the significance of ex ante bankruptcy costs (before the firm is in financial distress) and on the fact that most scientists fail to see these costs. The goal of minimizing ex post costs (as most scientists focus on) might very well increase the chance that bankruptcy actually occurs. Takeo Hoshi, Anil Kashyap, and David Scharfstein stress that the optimal bankruptcy regime is dependent on the economic system. In Japan there typically is a close relationship between a financing company and the debtor firm. This reduces the coordination problems between the firm and its creditor and therefore the need for and the cost of a bankruptcy regime.

In general, the book contains several elegant articles and has a very broad scope. It tackles many different problems from a variety of angles, thereby doing justice to the complexity of the problem of financial distress. The structure is clear and the devision in six sections is well chosen. The introductions to the different articles are clarifying and valuable as a summary. Surprisingly, however, the editors did not add a conclusion to the book. This is a major omission since the editors let the articles speak for themselves so that many of the arguments are not conclusive and contradictions between articles are not sorted out. A critical evaluation by the editors would have been a valuable addition. Maybe this omission can be corrected in a second edition. Hopefully, we do not have to wait until after the next recession for further advances in bankruptcy literature.

H.L. van Beusekom

Malcolm B. Coate and Andrew N. Kleit (eds.), *The Economics of the Antitrust Process*, Kluwer Academic Publishers, Boston/Dordrecht/London, 1996. Pp. 260. Dfl. 175

This concise volume focuses on the antitrust process in the United States. The majority of the contributions is concerned with merger control. Most of the authors (including the editors) share some present or past affiliation with the Federal Trade Commission. This agency and the Department of Justice are jointly responsible for antitrust enforcement at the Federal level. The insider background of a large number of authors certainly increases the value of the essays. Although the book is entirely focused on the United States, it offers exciting material for readers interested in competition policy in European countries. After all, antitrust enforcement in the United States dates back over 100 years. I necessarily limit myself to issues in the various essays that struck me in particular.

Firstly, according to Blumental (chapter 2) there is a tendency to over-investigate merger filings. Because the document requests represent a ‘free lunch’ to the government, agencies impose undue costs on merging firms. In Langenfeld’s contribution (chapter 3) the total costs for firms are estimated at hundreds of thousands of dollars. The editors suggest eliminating the free lunch by requiring the antitrust agencies to pay some small fraction (e.g. 5%) of the total costs imposed on firms. Secondly, the consideration of efficiencies in the merger control process receives a lot of attention. There is something of a paradox in this field. Economists tend to stress that efficiency considerations are not sufficiently taken into account in merger control analysis, as they do in this book. But at the same time it is widely recognized that wherever efficiency effects were analyzed, actual merger control decisions are not very much affected by efficiency considerations. Langenfeld states that efficiencies have convinced the antitrust agencies not to challenge some mergers. In the contribution of McFetridge on the Canadian experience, however, the conclusion is that relatively few cases are concerned with efficiencies. In nearly ten years there has not been a single case in which efficiencies have saved an anticompetitive merger in Canada. Furthermore, firms have a strong incentive to exaggerate the efficiency impact of a merger, while the agencies have a hard time assessing the evidence (a typical case of asymmetrical information!). So the key question remains: Why bother about efficiency defence in merger control? Thirdly, I enjoyed the cases described in the book. There is a contribution by Levinson on the famous Microsoft case (chapter 9). Even more interesting in my view is the Wal-Mart case on predatory pricing in the retail trade, described by Boudreaux (chapter 10). This author presents a convincing argument in which he demonstrates that firms lack an economic rationale to engage in predatory pricing. He argues that if predation ever occurs, it will not take the form of easy-to-mimic price cuts, but a non-price form (e.g. quality improvements).

In summary, this volume is very useful for people interested in the economics of American antitrust practice, with an emphasis on merger control. Because of the rich history of American antitrust, there is a lot to be learnt for Europeans involved in antitrust. If I may end with a personal note, I would be delighted to see a volume like the present one on the economics of Dutch competition policy in say five years after the introduction of the new Dutch competition law in 1998!

Noé van Hulst

@inproceedings{Cuthbertson2004QuantitativeFE, title={Quantitative Financial Economics: Stocks, Bonds and Foreign Exchange}, author={Keith Cuthbertson}, year={2004} }. Keith Cuthbertson. Published 2004. Economics. Partial table of contents: RETURNS AND VALUATION. Basic Concepts in Finance. Modelling Equilibrium Returns. Valuation Models. Efficiency predictability and volatility. Empirical Evidence on Efficiency. Rational Bubbles. Anomalies, Noise Traders and Chaos. THE BOND MARKET: Bond Prices and the Term Structure of Interest Rates. Empirical Evidence on the Term Structure. The foreign exchan...